

Romans and Batavians. A Regional Study in the Dutch Eastern River Area

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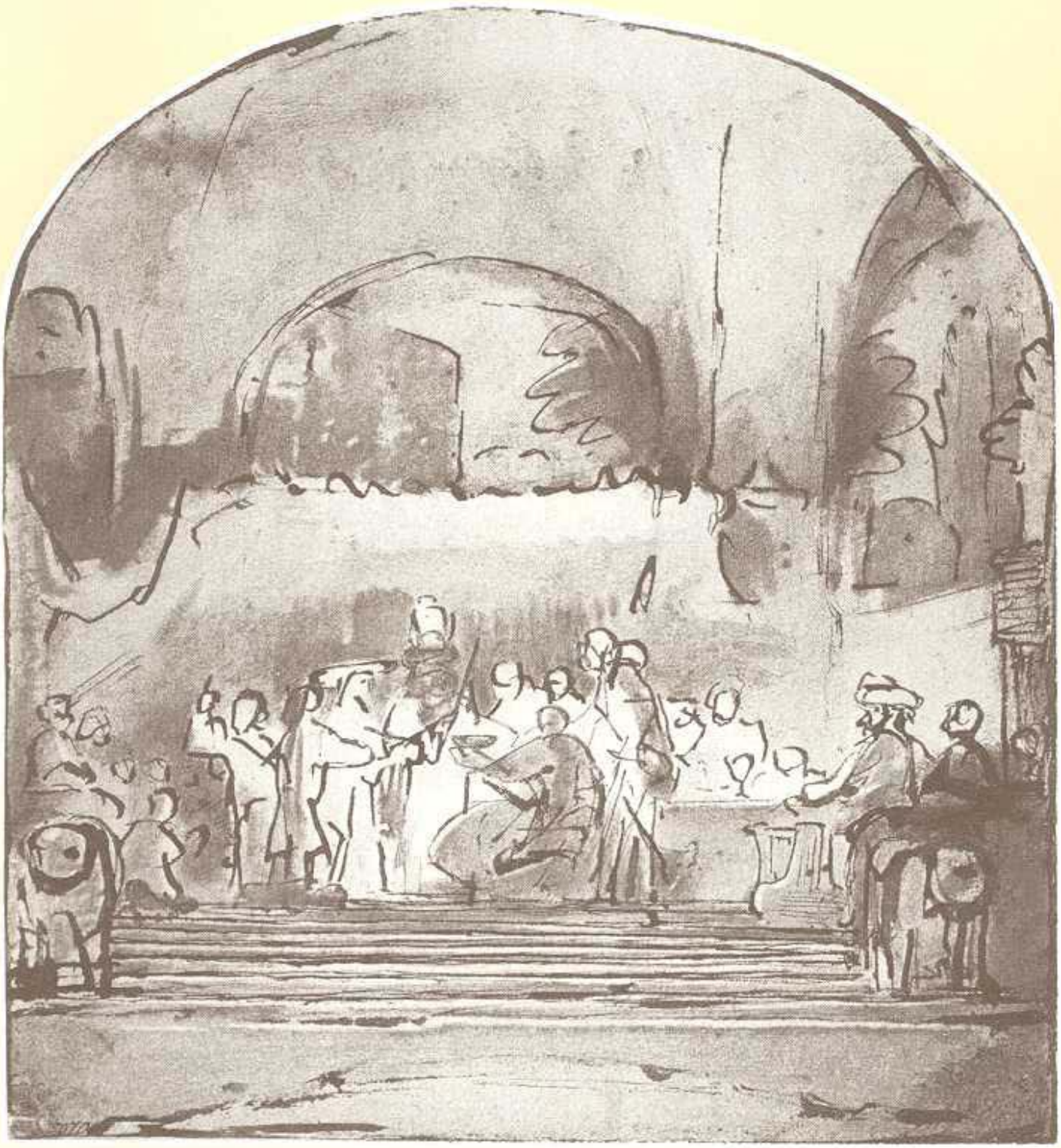
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WILLEM J.H. WILLEMS

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COVER ILLUSTRATION

Drawing by Rembrandt (1661). Study for 'The Conspiracy of Claudius Civilis: the Oath', his largest painting of which only the central part survives. It was originally intended for the new town hall of Amsterdam as the first of a series of eight paintings representing scenes from the Batavian revolt, then seen as the prototype of the recent Dutch revolt against Spain. Collection: Staatliche Graphische Sammlung, München.

J'aimais ces lieux tristes, qui semblaient
hideux à mes aides de camp, ce ciel brouillé,
ces fleuves boueux creusant une terre informe
et sans flamme dont aucun dieu n'a modelé
le limon.

The emperor Hadrian on the Dutch river area.
Marguerite Yourcenar, Mémoires d'Hadrien, 1951

Voor Vader en Moeder
Voor Mieke
Voor Dirk en Annemarie

Universiteit van Amsterdam

Romans and Batavians. A Regional Study in the Dutch Eastern River Area

Academisch Proefschrift
ter verkrijging van de graad van
doctor in de Letteren
aan de Universiteit van Amsterdam,
op gezag van de Rector Magnificus
dr. D. W. Bresters,
hoogleraar in de Faculteit der Wiskunde
en Natuurwetenschappen,
in het openbaar te verdedigen in de
Aula der Universiteit
(tijdelijk in het Wiskundegebouw, Roetersstraat 15)
op dinsdag 27 mei 1986, te 16 uur.

door
Willem Johannes Hyacinthus Willems
geboren te Blerick

Promotores Prof. dr. W.A. van Es

Prof. dr. J.H.F. Bloemers

Stellingen

1 Het traditionele beeld van Bataven die via de Rijn ons land binnenkomen is waarschijnlijk juist. Onjuist is echter het idee dat ze dit op eigen initiatief hebben gedaan.

2 Er bestaan goede redenen om aan te nemen dat Castra Herculis (Ammianus Marcellinus XVIII 2, 4 en *Tabula Peutingeriana*; cf. Libanius, *Oratio* XVIII, 87) op de plaats van het tegenwoordige Meinerswijk heeft gelegen.

3 Het is mogelijk om op grond van regionaal archeologisch onderzoek, waarvan landschapsreconstructie een integraal onderdeel uitmaakt, in combinatie met een analyse van Romeinse militaire strategie en tactiek ook bij afwezigheid van vondsten verantwoorde uitspraken te doen over de locatie van castella tussen circa 70 en 270 na Chr.

4 In de buurt van Randwijk heeft tussen circa 70 en 270 na Chr. een castellum gelegen.

5 Het vóórkomen van Romeinse militaire dakpanstempels op vindplaatsen in een brede zone achter de *limes* levert geen rechtstreekse indicatie voor een militair karakter van daar gelegen nederzettingen.

6 De Romeinse villae in zuidoost Nederland, van Nijmegen tot aan Maastricht, dateren alle van na 70 na Chr. en zijn – al dan niet continu – in meerderheid bewoond gebleven tot rond 400. Het is van groot belang nader te onderzoeken of ook hun functie vóór en ná het laatste kwart van de 3e eeuw dezelfde was.

7 Steekproefsgewijs opgraven van nederzettingen dient, als techniek om een correct beeld van een nederzetting te verkrijgen, te worden bijgezet op de plank van onbruikbare en misleidende onderzoekstechnieken. Een dergelijke procedure heeft alleen nut op lagere of hogere niveaus van onderzoek zoals de huishouding en de regio. Om nederzettingen te leren kennen is het nodig goed geselecteerde sites volledig op te graven.

8 Zowel om antropologische kennis werkelijk in het Nederlandse archeologische denkraam te integreren als ook om deze kennis voor meer technisch gericht onderzoek bruikbaar te maken en internationaal de aansluiting niet te verliezen dient Nederlands ethnoarcheologisch onderzoek met kracht bevorderd te worden.

9 Geschiedschrijvers van de Nederlandse archeologie zouden er zich eens op moeten bezinnen of hun activiteiten vooreerst niet beter gericht kunnen worden op de niet in publicaties terug te vinden maar voor een betere beoordeling van oud onderzoek onmisbare informatie over achteraf onjuiste werkmethoden, opvattingen en concrete beslissingen van archeologen in de eerste helft van deze eeuw, nu verschillende betrokkenen en de meeste van hun leerlingen daar nog zelf over kunnen vertellen.

10 Het niet noteren van de exacte vindplaats van mobiele vondsten is diefstal van cultureel erfgoed.

11 Dat tegenwoordig wetenschap moet worden bedreven onder de dreiging 'publiceren of ten ondergaan' is een juiste zij het nog grotendeels tot universitaire instituten beperkte constatering. Binnen de archeologie worden de negatieve aspecten hiervan echter gecompenseerd door de positieve, met name het sneller beschikbaar komen van opgravingsgegevens.
Lit.: H.A. Heidinga, *De Veluwe in de vroege middeleeuwen*, Amsterdam 1984, stelling 13.

12 De openbare bespreking van ZWO-aanvragen door de werkgemeenschappen van ARCHON leidt tot onterechte of overdreven kritiek, frustraties van persoonlijke verhoudingen en onnodig verlies van arbeidstijd. Het huidige selectiesysteem dient te worden vervangen door een procedure waarbij een commissie de aanvragen beoordeelt en na overleg met de aanvragers doorstuurt naar het bestuur. Dit bestuur dient niet het recht te hebben in de aangegeven prioritering veranderingen aan te brengen anders dan naar aanleiding van een beroepsprocedure.

13 Waar de mogelijkheid bestaat een proefschrift over een samenhangend onderzoek als geheel te publiceren dient het promoveren op afzonderlijke artikelen ten sterkste te worden ontraden.

14 Voor het psychisch welbevinden van de gemiddelde Nederlander in de toch al sombere wintermaanden verdient het aanbeveling niet-tikkende gasmeters in te voeren.

Stellingen behorende bij:
W.J.H. WILLEMS
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CONTENTS

PREFACE 7

PART I

1 INTRODUCTION 9

1.1 THE ERA PROJECT 10

1.2 ROMANS AND BATAVIANS 20

2 HISTORICAL DATA AND PERIODIZATION 22

2.1 LATE-IRON AGE – EARLY-ROMAN PERIOD 23

2.2 EARLY-ROMAN – MIDDLE-ROMAN PERIOD 24

2.3 MIDDLE-ROMAN – LATE-ROMAN PERIOD 26

2.4 LATE-ROMAN – MEROVINGIAN PERIOD 27

3 THE LANDSCAPE 28

3.1 INTRODUCTION 28

3.2 THE MAP: COMPOSITION AND SOURCES 28
3.2.1 The Legend: Holocene Deposits 29
3.2.2 The Legend: Pleistocene Deposits 36
3.2.3 Regional Characteristics and Sources 39

3.3 ROMAN RIVER-COURSES 45
3.3.1 Reconstruction and Dating 45
3.3.2 Pre-Roman Stream-Ridges 49
3.3.3 The Rivers 52

3.4 ROADS AND ROUTES 63
3.4.1 The Roads 64
3.4.2 The Routes 66
3.4.3 Conclusions 69

3.5 ANCIENT SETTLEMENT SOILS 70
3.5.1 The Settlement Soils as Findspots 71
3.5.2 Settlement Soils and the Completeness of the Archaeological Record 73

4 THE SITES: CHRONOLOGY AND RELATION TO THE DISTRIBUTION MAPS 76

4.1 CHRONOLOGY OF THE SITES 76

4.2 TYPOLOGY OF THE SITES (THE LEGEND ON APPENDICES 1–5) 89

5 THE SITES: CATALOGUE 93

CATALOGUE 95

6 THE FINDS 133

6.1 INTRODUCTION 133

6.2 WHEEL-TURNED POTTERY 136
6.2.1 Terra Sigillata 136
6.2.2 Colour-coated Wares 158
6.2.3 Gallo-Belgic Ware 159
6.2.4 Smooth Ware 165
6.2.5 Coarse Ware 166
6.2.6 Other Roman Pottery 170
6.2.7 Merovingian Pottery 171

6.3 HAND-MADE POTTERY 178

6.4 BUILDING-MATERIAL 182
6.4.1 Brick 182
6.4.2 Stone 183

6.5 EPIGRAPHY 186
6.5.1 Tile-stamps 186
6.5.2 Graffiti 192

6.6 OTHER FINDS 193
6.6.1 Loomweights and Spindle-whorls 194
6.6.2 Slags and Crucibles 195
6.6.3 Glass 195
6.6.4 Net-sinkers 197

PART II

7 INTRODUCTION 202

7.1 ADDITIONS TO THE CATALOGUE 204

7.2 THE LANDSCAPE AND ITS POTENTIAL 205
7.2.1 Geology and Reconstruction 205
7.2.2 Ecology and Land-Use 210

7.3 THE DISTRIBUTION OF SETTLEMENTS 221

8 TYPOLOGY OF THE SITES 222

8.1 INTRODUCTION 222

8.2 THE LATE-IRON AGE 223
8.2.1 Settlements 223
8.2.2 Cemeteries 230

8.3 THE EARLY-ROMAN PERIOD 232
8.3.1 Military Settlements 233
8.3.2 Civil Settlements 238
8.3.3 Cemeteries 243

8.4 THE MIDDLE-ROMAN PERIOD 246
8.4.1 Military Settlements 247
8.4.2 Civil Settlements 262
8.4.3 Quantitative Analysis of Settlements 287
8.4.4 Cemeteries 294
8.4.5 Isolated Finds 300

8.5 THE LATE-ROMAN PERIOD 302
8.5.1 Military Settlements 305
8.5.2 Civil Settlements 312
8.5.3 Cemeteries 316
8.5.4 Isolated Finds 318

8.6 THE MEROVINGIAN PERIOD 320
8.6.1 Settlements 321
8.6.2 Cemeteries 325
8.6.3 Isolated Finds 327

9 THE ROMAN FORT AT ARNHEM-MEINERSWIJK 329

9.1 INTRODUCTION 329

9.2 THE EXCAVATION 330
9.2.1 Period 1 332
9.2.2 Period 2 335
9.2.3 Period 3 338
9.2.4 Period 4 341
9.2.5 Period 5 345
9.2.6 Period 6 350

9.3 CONCLUSIONS 354

10 THE RIVER AREA: THE NATIVE BACKGROUND 357

10.1 INTRODUCTION 357

10.2 THE SOUTHERN LOW COUNTRIES UNTIL THE END OF THE GALLIC WAR 357
10.2.1 Historical Information 357
10.2.2 Archaeological Evidence 361
10.2.3 Conclusion 364

10.3 FROM CAESAR TO DRUSUS 366
10.3.1 Historical Inferences 366
10.3.2 Archaeological Evidence 368

10.4 SITUATION IN THE RIVER AREA 369
10.4.1 Social and Political Organization 373
10.4.2 Economic Organization 374

11 ANNEXATION, GROWTH, AND BREAKDOWN. THE RIVER AREA FROM 12 BC–AD 270 377

11.1 INTRODUCTION 377

11.2 IMPERIALISM AND COLONIALISM 377
11.2.1 Theoretical Considerations 377
11.2.2 Roman Expansion 380

11.3 FROM DRUSUS TO CORBULO 386
11.3.1 Imperial Policy and the Formation of the Civitas Batavorum 386
11.3.2 The River Area in the Early-Roman Period 391

11.4 FROM CORBULO TO POSTUMUS 400
11.4.1 Revolt, Consolidation, and Growth until the Frankish Invasions 400
11.4.2 The Development of a Frontier Zone 410

12 BATAVIA. THE RIVER AREA FROM THE LATE-3RD TO 6TH CENTURIES 432

12.1 ROMANS AND FRANKS. HISTORICAL DEVELOPMENTS 432
12.1.1 The Civitas Batavorum: End and Aftermath 432
12.1.2 The Frankish Kingdom 439

12.2 BATAVI AND SALII. CHANGES IN THE RIVER AREA 441
12.2.1 Ups and Downs until the End of the Batavian Civitas 442
12.2.2 Batavia Salica 450
12.2.3 The River Area in the Merovingian Period 460

SAMENVATTING (DUTCH SUMMARY) 462

REFERENCES 465

PREFACE

This book is the first in a series of monographs dealing with the results of the ERA project of the ROB. ERA stands for Eastern River Area, the eastern part of the delta of Rhine and Meuse in the Netherlands. An introduction to the project is provided in chapter 1.

That the book does not bear a number is due to the fact that it was originally published in two parts in the *Berichten van de Rijksdienst voor het Oudheidkundig Bodemonderzoek (BROB)*: the first part (chapters 1-6) in *BROB* 31, 1981, the second (chapters 7-12) in *BROB* 34, 1984. These two articles are nos. 5 and 8 of the ERA project papers, a series of publications on research resulting from the project which is included in the *BROB*. The monographs will in the future be published in the ROB series *Nederlandse Oudheden*.

As in the present case, some of these are also intended as a D. Litt. or D. Sc. thesis. In the near future, a volume will be published on stock–breeding, hunting, and ecology in the eastern river area by R. Lauwerier. Another volume, a first overview of the ROB excavations in Nijmegen, is being prepared by Professor J.H.F. Bloemers and a third one by P.A.M. Zoetbrood on the 4th-century cemeteries in Nijmegen.

The fact that the present book was originally published in two parts with two years in between has a number of negative consequences. The introduction to part 2, chapter 7, contains various additions and corrections pertaining to earlier chapters. Evidently, the chapters in the first part do not contain cross-references to the later chapters and, in addition, an index is lacking. In general, however, there proved to be solutions for most problems.

In a separate cover, five maps are provided which are part of this study. They are the Appendices 1-5, giving the distribution of archaeological sites against a relevant geological background. At the same time, these maps are part of the *Archaeological Map of the Netherlands 1:100,000* which is published by the ROB. Simplifications of these sheets are included in chapter 7 (figs. 60-64).

The basic data for this study were collected in the river area from 1978 to 1980, on a grant from the Netherlands Organization for the Advancement of Pure Research (ZWO). It has been completed from a more secure position on the staff of the ROB which, on the other hand, accounts for the fact that it took until the summer of 1985 before the final chapter was written.

Apart from this organizational background situation my work has, of course, greatly benefited from the stimuli, advice, and assistance I received from a large number of people. Their contributions, for which I am deeply indebted, have been acknowledged in part 1 (p. 21-2). There are, however, others: my wife, Mieke, whose support and endurance have been of essential importance; my children, who far too often missed the fatherly attention they had a right to; and my parents, who cared for me and created so many opportunities throughout my education and study. With deep gratitude, this book is dedicated to all of them.

Amersfoort, December 1985

1 INTRODUCTION

The present article is the first part of a study on the habitation in the eastern part of the Dutch river area (fig. 1) between 250 BC and AD 750. As such, it also belongs to the Eastern River Area (ERA) Project papers, which deal with various subjects within the same chronological and spatial co-ordinates. An introduction to this series, and to the aims of and participants in the ERA project, has been published recently.[1] The stated aims of the project largely coincide with those of the present, regional study, which was designed on the one hand to provide a suitable background and general framework for detailed investigations on special subjects and on the other to act as a pilot-study: to explore the limits of the data which are currently available, to integrate those data at a level beyond that of sherds, findspots, or distributions and thereby to locate the worst blind spots in the data-set as well as to formulate hypotheses, both of which should serve to direct further research.

The regional study of the eastern river area, which started in 1978,[2] was actuated by practical as well as theoretical motives. At a practical level, the increasing number of (rescue-)excavations of mainly Roman Period sites and the change in ROB policy which led to a concentration of the institute's resources in a restricted number of research projects[3] were a major impetus. As for theoretical motives, the growing concern with socio-cultural systems and processes had inevitably led to an acute awareness of the region as a level at which research should be conducted. The eastern river area seemed to be quite suitable for doing regional research which could be more than the compilation of distribution maps.

There were several reasons for that assumption. Firstly, the level of archaeological knowledge was already very high indeed. Antiquarians had occupied themselves with the area for centuries, and the first scientific excavation in Nijmegen took place in 1834.[4] It is also justified to say that the first real archaeological survey was carried out around the same time,[5] and an increasing body of work has been done in the century and a half since then. A peak was reached just after World War II, when the soil surveys of the Holocene river deposits led to the discovery of an incredible number of settlements: not just many, but actually most of all the settlements (Roman and later) which were not eroded.[6] The numerous post-war excavations by the ROB and other institutes, as well as the very intensive surveys and rescue work by competent amateurs since the 1960s, provided additional and more detailed data.

For these reasons, a regional study of the eastern river area could start without first conducting a field survey or, as a fashionable and time-and-money-saving alterna-

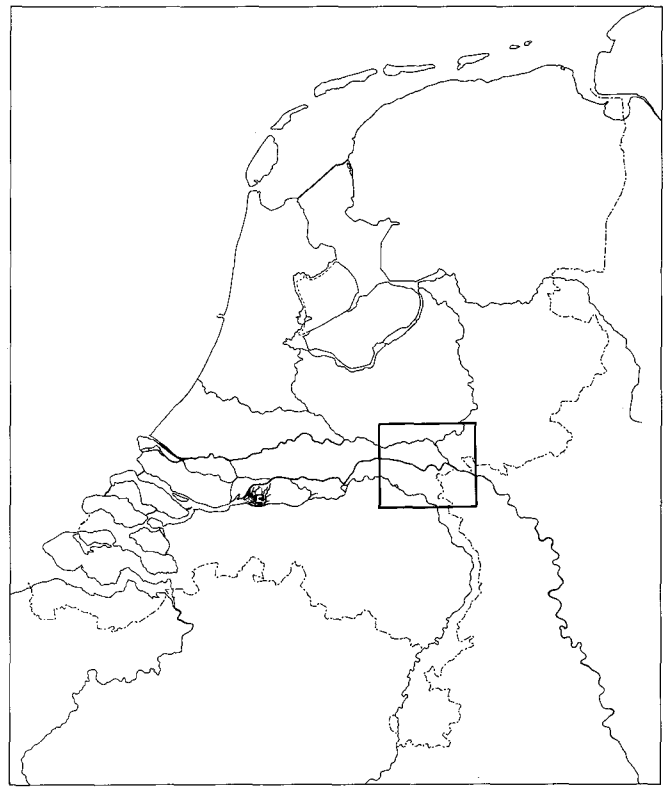


Fig. 1 Location of the eastern river area

1 Bloemers/Hulst/Willems 1980.

2 I am grateful to The Netherlands Organization for the Advancement of Pure Research (ZWO) for a grant which enabled me to do the basic research from 1978–1980 (grant no. 28–141). The entire study is intended to serve as a doctoral thesis.

3 See e.g. Van Es 1977.

4 Excavation by C.J.C. Reuvens and C. Leemans (see Brunsting 1949). For other data on the history of archaeological research, see e.g. Byvanck 1943, 360–4, 395–6; Halbertsma 1970; Brongers 1976b; Van Es 1977.

5 Heldring 1838–9.

6 See chapter 3, esp. 3.5.

tive, employ some sort of sampling strategy. This does not imply that in the end those will not be necessary for several purposes. Only that they were not necessary at the outset because the available data were acceptable and in part even much better than any survey could possibly hope to produce.

A second reason was the nature of the material evidence. From most sites at least some artifacts, but usually a fairly and sometimes even very large number, were available in accessible public and private collections. Equally important was the available information about the artifacts themselves. For the Roman Period, a century of research has provided detailed and reliable typochronological frameworks. For the Merovingian Period the situation is somewhat less favourable as far as some sorts of pottery are concerned. Only for the Late-Iron Age is the typochronological information rather meagre, at least as far as the most important category of artifacts, the pottery, is concerned. It is, however, a prehistoric period for which a chronological 'accuracy' of several centuries in pottery datings is a normal phenomenon. The unavoidable lack of detail in a prehistoric context is precisely the reason why the ERA project, which centres on the Roman Period, should be able to reach higher levels of interpretation.

All this is, of course, directly related to the fact that from the end of the Late-Iron Age onwards, prehistory changes into protohistory. This is actually the third reason for the attractiveness of the project. There are written sources which can be used to derive all sorts of historical, geographical, economical, and anthropological information. These can be used as a general background, verification of, or contrast to other data and their interpretation. In addition, there are specific sources referring to the region itself, which is, after all, the Batavian heartland.

Finally, a fourth reason for a regional study of the eastern river area was the availability of various studies on the natural environment and ecology of the region, and the prospect that several more were already in progress or were to be started, tailored to the needs of the project. As is discussed in chapter 3, usually very detailed soil maps of almost the entire area were already available in 1977. In addition, the Netherlands Geological Survey (RGD) was in the process of completing the research for sheets 39 O and 40 W of the Geological Map of the Netherlands, and their co-operation provided indispensable information on the Holocene deposits. A program of palynological research in the eastern river area, by the Department for Biogeology of the Nijmegen University, had already resulted in a framework of the vegetation history and the variable impact of human occupation from the 5th millennium BC onwards. Since 1977, the ongoing research has made various contributions to geological and archaeological problems.[7]

Important ecological information is expected from the archaeozoological investigations of the finds from a number of recent excavations in Kesteren (38), Heteren (93), Elst (105), Meinerswijk (126), Druten (214), Ewijk (232), and Nijmegen (403, 407, 408, 409/410, 412, and 416), which began in 1980.[8] Preliminary palaeobotanical research has also been started on several of the Nijmegen settlement-sites.[9] The intensification of this work and the necessary expansion to a regional scale are, however, still desiderata.

Taken together, all these circumstances led to as well as allowed the present study and also determined its limitations. The long-term research strategy of the ERA project is designed to produce data that will eliminate some of those limitations, although, as an independent enterprise, it has various other objectives. Therefore, it is useful to present here in some detail those objectives, the strategies employed to reach them and the degree to which these are incorporated in the present study.

1.1 THE ERA PROJECT

It is clear that the project did not start as an integrated whole, with a clear research program based on goals derived from a well-defined theoretical framework. On the contrary, it began, as it were, in retrospect, to organize the accumulated and still increasing amount of valuable data at a time when 'new perspectives in archaeology' had become visible in the form of conceptual tools to handle those data in a sensible way.

7 Apart from internal communications of the department, only a limited number of these have yet been published. See Teunissen 1980b; 1982; Teunissen/Teunissen-van Oorschot 1980.

8 By Drs R. Lauwerier (BAI). ZWO-grant no. 28–166: Stockbreeding, hunting, and ecology during the Roman Period in the Eastern River Area in the Netherlands (provisional title).

9 By Drs J. Buurman and Mrs P. Gerritsen-Wijnmalen (ROB).

These are based on a change from a view of culture as the sum of characteristic traits to its conception as a system, composed of various subsystems and operating along certain lines which determine their development (the process of change) and their structure or ordering at a particular moment in time. These aspects can be studied by building or deriving models of the ways in which they work and by formulating hypotheses which can be tested by archaeological means. This brief description, of course, does no justice to the complicated theoretical issues involved[10] or to the degree of appreciation for them by different participants in the project. It is only intended to indicate a way of thinking which influenced the objectives and strategy of the project at certain levels.

The ERA project is concerned with the development of society in a particular region during a specific stretch of time, notably the Roman Period, and mainly within the boundaries of the Roman empire. To monitor this development, and to try to understand the external and internal processes involved, requires a division into relevant chronological phases and levels of analysis. The former, which is also determined by historical knowledge, will be discussed in chapter 2. It is only mentioned here because a proper understanding of the how and why of developments during the Roman Period inevitably leads to research into what went before and what came after. Hence the inclusion of the Late-Iron Age and the Merovingian Period in the time covered by the project, which is thus approximately a millennium.

In theory, various levels of analysis could be relevant.[11] For practical purposes, these have been defined as follows:

1 *The household level*, at which the function, size, shape, and other aspects of houses and related features within a yard (*erf*) are studied.

2 *The site level*, at which, for settlement-sites, the relations between and the functions of the various elements constituting the settlement (anything from the smallest village to a town) are analysed, as well as such themes as its development through time and subsistence strategy. For burial-sites there are analogous themes, such as their chronological development and the differential treatment of the dead.

3a *The micro-regional level*, which is the area covered by this study, the eastern river area proper. This is the level at which the different sorts of settlements (size, function, position), the settlement-system and its development, the economic system, demographic factors (cemeteries), etc., are of primary relevance. For the project as a whole, these subjects should also be studied at what can be called.

3b *The macro-regional level* or polity, which is the largest coherent or centrally organized regional unit. This could be, at a given time, the Batavian tribal area, possibly including that of the Cananefates, or the Civitas Batavorum at another. Its boundaries may have changed in the course of time, but its delineation can be partly the result of work in an even wider context, namely.

4 *The supra-regional level*, for which spatial limits cannot be defined, for they vary according to the subjects studied. These can be the pre- and protohistoric exchange or trade networks, the way in which the (macro-) regional system is tied into that of the province or the empire, the effects of imperial policy, and similar issues.

It is expected that an assessment of patterns and developments at these main levels and of their spatial and hierarchical relations will yield insight into the operation of relevant social and economic processes. Because of its central importance within the project, and also because it is the subject of the present study, the regional level will serve as a starting-point for further discussion of goals, methods, and restraints. This will of itself lead to some issues which are or should be investigated at lower or higher levels.

The choice of region

The circumstances which led to a project in the eastern river area have already been outlined. Nevertheless, they did not automatically lead to the precisely defined area (the micro-region) as indicated on fig. 2, which was chosen on the basis of theoretical as well as practical reasons. A primary consideration was that the area should be, as far as possible, a geographical unit and, secondly, that it should be centred on Nijmegen, the capital of the Civitas Batavorum. This produced an area mainly composed of Holocene fluvial deposits and bordered to the north, east, and south by Pleistocene ice-pushed ridges and coversand areas. Nijmegen is situated on the protruding

10 See e.g. Willey/Sabloff 1974, chapter 6, Van der Leeuw 1974, Flannery 1976, or Clarke 1978 (rev. ed.) for historical, theoretical, and practical discussions.

11 Flannery 1976. A short summary on pp. 5–6.

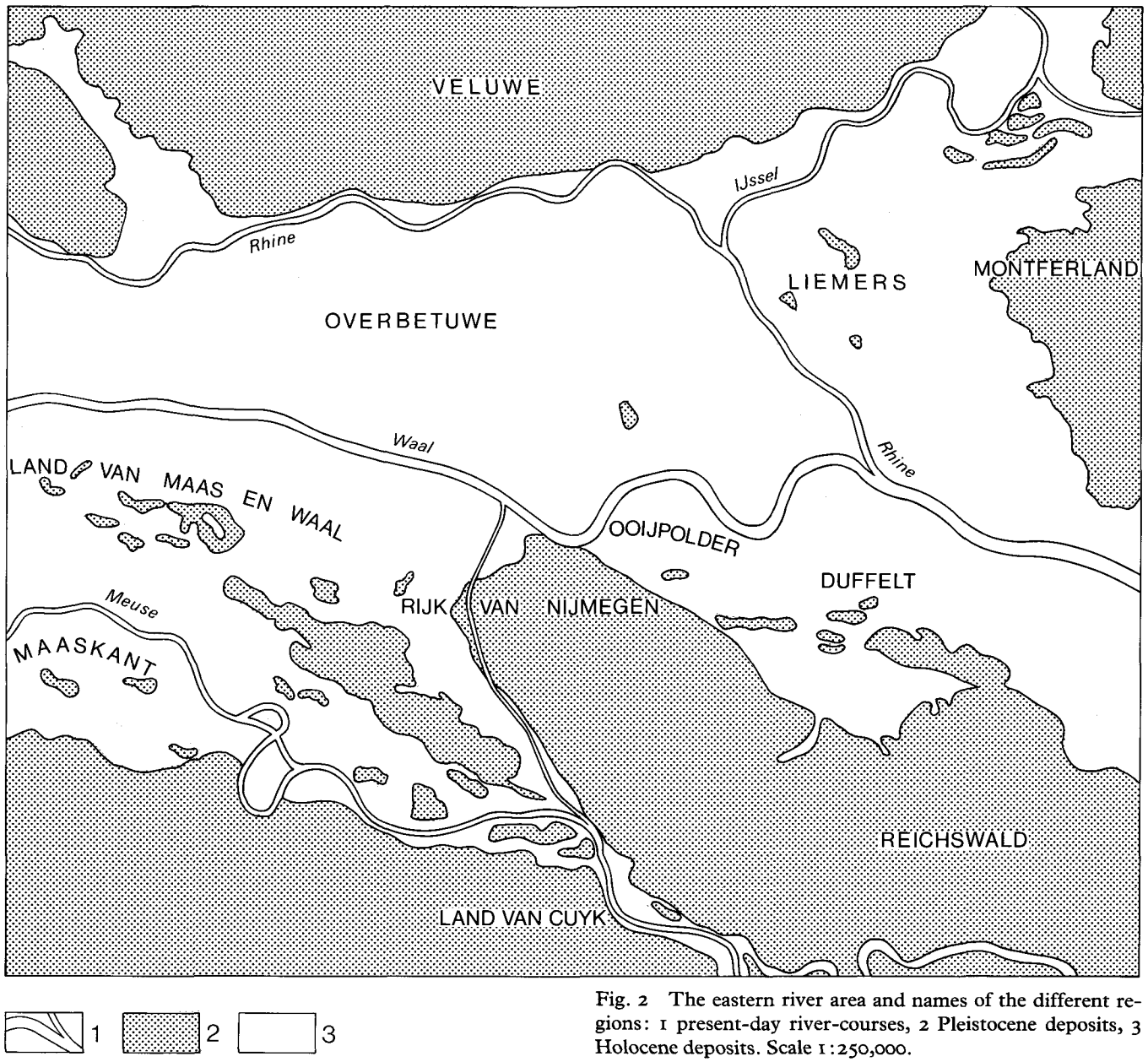


Fig. 2 The eastern river area and names of the different regions: 1 present-day river-courses, 2 Pleistocene deposits, 3 Holocene deposits. Scale 1:250,000.

tongue of one such ridge. As can be seen more clearly on fig. 3, this is actually the easternmost part of the Rhine-Meuse basin. Although inevitably somewhat arbitrary, the western boundary of the area is located at the point where this basin begins to widen quickly towards the coast and approximately at the border of the Over- and Nederbetuwe. The area thus covers what is generally considered the central region of the Civitas Batavorum and its northern and eastern limit, which coincides with the imperial frontier, the *limes*, along the Rhine. The inclusion of some terrain north of the Rhine as well as south of the Meuse was also a deliberate choice. The main area between these rivers, as seen from a supra-regional (provincial or imperial) level, was a frontier zone



Fig. 3 The geological and administrative context of the eastern river area in the second century AD: 1 coastal dunes, 2 marine clay deposits and peat, 3 Holocene fluvial deposits, 4 Pleistocene deposits, 5 theoretical boundaries of the civitates, 6 the eastern river area, 7 civitas capital, 8 possible civitas capital.

and it was thought to be useful to have at least a first link between that area and the regions beyond the frontier as well as those in the hinterland.
The definition of the micro-region for practical reasons was also a compromise. First, the already available data were going to be organized in the present study, which had to be completed within a reasonable time if it where to identify 'blind spots' and direct further research. Extension of the area beyond the indicated limits would lead to an unacceptable prolongation of the data-collection phase. Second, such an extension would meet with other limits, namely those of different projects surrounding and overlapping with the eastern river area. These projects do not all have the same goals. In its theoretical orientation, this study is primarily comparable to the regional projects of the IPP in the Assendelver Polders (north of the Rhine)[12] and of the AIVU/IPP in the Kempen (in the hinterland),[13] which also cover roughly the same period.
Nevertheless, there are many similarities to various other projects with which there is also a direct spatial relation. The Kromme Rijn Project of the ROB investigates the central part of the Dutch river area. It is focused on the Early-Middle Ages, but includes an inventarization of Roman sites.[14] For obvious reasons (see fig. 3!), the data-collection and the construction of a relevant geological background are closely connected to those of the eastern river area. A close co-operation with the (RMO) inventarization project of finds from the Land van Maas en Waal[15] proved to be very helpful for the data-collection, the territorial overlap being irrelevant in view of the very different goals of both projects. On the other hand, co-operation with the Maaskant project of the IPL[16] and the inventarization project of Roman finds from Brabant[17] could not be established. As a result, no information on recent finds could be obtained or used in any way. The data from the area south of the Meuse will, after publication, be adapted to suit the purposes of the ERA project, but for the present study only the information from older, published reports and all the geological information drawn from other sources were available.

12 See Brandt/Van der Leeuw/Voorrips 1979 and Brandt/Groenman-van Waateringe/Van der Leeuw (eds.) 1985, for the research proposal and first results of the project.
13 See Slofstra/Van Regteren Altena/Roymans/Theuws 1982.
14 See Van Es/Verwers 1980, 7–8 for an introduction and further references.

The latter was also indispensable because, as part of a regional investigation incorporating an inventarization of finds, the necessary maps could be included in the series of the 'Archaeological Map of the Netherlands 1:100,000'.[18] This had several advantages, but it implied that the limits of the area had to be determined by the grid of the Topographical Map of the Netherlands and preferably, although not necessarily, by the borders of the 1:25,000 or 1:50,000 sheets of that map. Even more important is the fact that the current series of the Soil Map and the Geological Map of the Netherlands employ the same sheets. These are not yet all available and thus, as a primary source of information for the construction of a relevant geological background, their absence has to some extent influenced the delineation of the area.

The region in a wider context

From the preceding paragraph, it is clear that the spatial definition of the micro-regional level is not in all respects satisfactory. Research at the macro-regional level is intended to overcome some of the problems which result from this definition, for example the development and structure of the settlement system. Some aspects thereof, especially when regarded from the top downwards (the position and function of the regional centre), cannot be entirely studied at the micro-regional level. An interesting illustration of this point is provided by a recent article on finds from Rossum/Lith, situated some 35 km west of Nijmegen in the central river area at the point where Meuse and Waal come very close together.[19] The writers propose this location as the site of the major pre-Roman Batavian centre. Their hypothesis will need some further proof, especially concerning the interpretation of dredging-finds, but it is at the very least a serious possibility. The site is, however, located outside the region of the ERA project and this could lead to grave misunderstandings of the settlement system at the end of the Iron Age and the possible drastic changes after the arrival of the Roman armies.
There may well be other potentially very important data

15 Peddemors 1978. I am grateful to A. Peddemors for various corrections and additions to the sites mentioned in the catalogue (chapter 5).
16 G.J. Verwers 1981.
17 W.J.H. Verwers 1977, 165–8.
18 See chapter 3, p. 28.
19 Roymans/Van der Sanden 1980, see esp. pp. 191–2.

of this kind which will emerge only when the inventarizations in the central river area and in Brabant become available. In the second part of the present study, the macro-regional level can thus only be treated in a more general way. Some preliminary considerations have already been published elsewhere,[20] including a map showing the approximate extension of the Civitas Batavorum (fig. 3). At least during most of the Roman Period, this is the area in which macro-regional structures and processes should be studied.

The borders, as indicated on fig. 3, are analytical. They are simply the perpendiculars drawn at the mid-points between civitas capitals, producing Thiessen polygons which could be the service areas of those centres.[21] These do, however, conform quite well to probable natural borders. To the north this is, of course, the Rhine. To the east, the perpendicular roughly coincides with the large peat area of the Peel (not indicated: west of the Meuse) and to the west with the approximate limits of the large Dutch peat areas (Holland peat). The southern border is nearly identical with the watershed between the drainage basins of Meuse and Scheldt.

The macro-region is thus composed of geologically and ecologically different areas: the rivier-clay area (the classical *insula Batavorum*) and the Pleistocene sands of Brabant. The varying degree of coherence between these areas in the course of time should in itself become an important subject of study for the ERA project. The trajectory from a frontier zone into the hinterland could also be investigated in this way. This would imply a coordination of the above-mentioned Kempen project with the ERA project at a higher level, which is at the same time an intermediate step towards research at the supra-regional level.

The choice of region then becomes very dependent on the sort of problems to be investigated and can easily be adapted to various research goals. Nevertheless, in view of the particular geographical situation of the eastern river area at the frontier of an empire, which inevitably leads to a specific interest in phenomena beyond and behind that border, some sort of primary spatial definition of the supra-regional level did emerge. A first approach to research at this level has been published by Bloemers,[22] who defined the area as the basins of the Scheldt, Meuse, Rhine, and Ems (fig. 4).

The present study is not intended to extend too deeply into this level of research although, as has already been shown elsewhere,[23] it may contribute to some of the issues which are supra-regional in scope. The same is true for other investigations or projects, and this has led to the formation of an (informal) research-group in which archaeologists from various institutes participate.[24]

Regional research goals

Within this wider context, there are some specific subjects which should be investigated at a regional or lower level. A central question is the relation of Nijmegen to its immediate hinterland. This leads to an analysis of the different sorts of settlements which are present in the region and to a model of the socio-economic relations between them. Ethnographic models and historical sources are important tools in evaluating the archaeologically visible structural and functional differences between settlements and the way in which they were tied into the regional network.

Evidently, several important changes are to be expected between 250 BC and AD 750. The region became part of an organized whole, the Roman empire, which fell apart again several centuries later. This leads to particular questions regarding the way in which and the degree to which the region was integrated and the effects of the later desintegration. These are problems which can be investigated at the regional level, even though they are evidently induced by developments elsewhere (in a historical context) or are an example of general socio-economic processes (in a social science frame of reference). By the latter are meant the general process of acculturation and the specific form of that process which has earlier been described as a case of colonialism.[25]

In this respect, the regional consequences of the imperial policy of defence and control, as reflected in, for example, the regional deployment of troops, is also subject of study. In several ways, the presence of these troops rais-

20 Willems 1983a.

21 The lines were derived from Bloemers 1980, fig. 2, reproduced here as fig. 4. See also Bloemers 1983. The civitas quality of the supposed area of the Frisiavones but especially the location of its capital are, of course, speculative (cf. Bogaers 1967, 101-6).

22 Bloemers 1980, 1983.

23 Willems 1983a.

24 The so-called 'Romanization-symposium' which was held in Amsterdam in December 1980 (Brandt/Slofstra (eds.) 1983) was an initiative of this group.

25 Willems 1983a, see esp. p. 105–7 and 115–20.

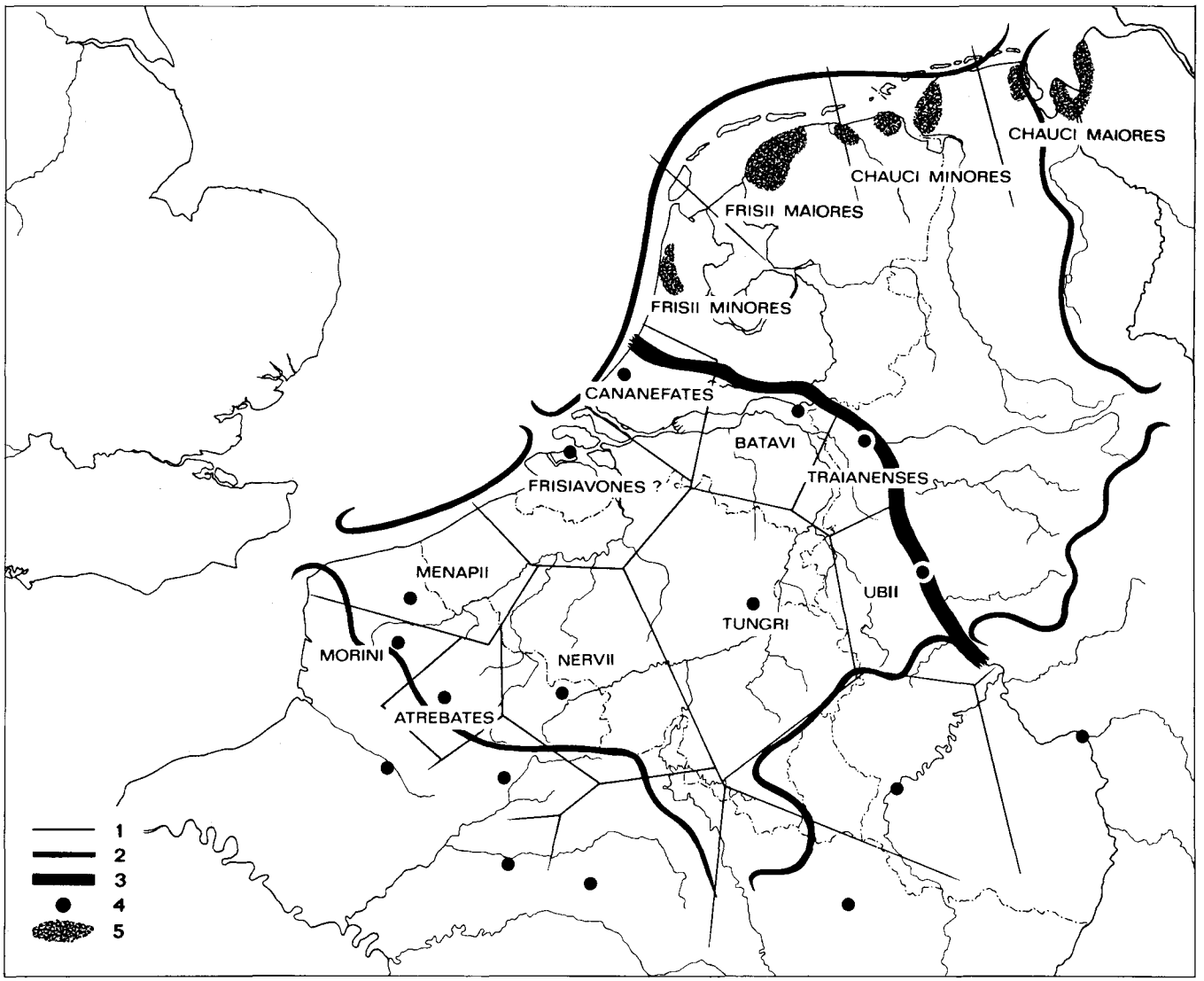


Fig. 4 The supra-regional level, defined as the basin of Scheldt, Meuse, Rhine, and Ems: 1 theoretical boundaries of civitates and tribal areas, 2 geographical limits of the supra-regional level, 3 frontier of the Roman empire between c. AD 50 and AD 270, 4 civitas capitals, 5 concentrations of native settlements (After Bloemers 1980, fig. 2).

es other questions regarding the area and its inhabitants. The demographic and social consequences of the fact that large numbers of Batavians served in the army is one aspect. The particular structure of the landscape and its natural resources are others. These require investigations of the sometimes highly illuminating choices of location for frontier forts, but also lead into the difficult estimates of the region's carrying capacity and the extent of its self-sufficiency in different periods.

These aspects are intimately connected with the reconstruction of the landscape on the basis of the geological and soil maps and with the reconstruction of its vegetation history by palynological research. The archaeozoological and palaeobotanical information is, of course, primarily processed at the settlement level but it can be combined with the other data at the regional level. With the information on technological developments and changes in the economic system, it is hoped that some insight will be gained into the regional subsistence strat-

egies in different periods as well as the varying degree of dependence on imports.
Within the ERA project, the study of burial sites has also been started. At a regional level, this is still limited to the relation between cemeteries and settlements and the road system, and occasional comparisons between cemeteries. Only after more work has been done at the site level will it be possible to reach conclusions about such themes as social structure or acculturation or, possibly, religious beliefs that are more securely grounded than the usual superficial remarks based on incidental observations or historical sources.
Apart from the above-mentioned subjects, the ERA project is also concerned with aspects of regional material culture. An important issue is the development of a typochronological framework for the native hand-made wares, in particular the so-called late-Iron Age and native-Roman pottery formerly called 'Batavian'.[26] A technological approach to the particular problems of ordering this pottery has not yet begun. It is, however, an additional goal of the project to use various physicochemical methods to study the economics and technology of this, but in particular the Roman pottery and brick produced within the region.
This objective is, of course, directly related to several other of the themes which have been outlined, for they are all closely connected in various ways, just as are the different aspects of the research strategy employed to investigate them. This strategy, and the place of the present study in it, will be considered next.

Research strategy

One of the first tasks of the project is to assemble data on the incidence, chronology, and nature of sites in the region. It is obvious that the sequence of these three aspects means increasing involvement with research at the site level. The incidence of sites must be studied at the regional level, on the basis of collections of surface-material and other indications. The same can apply to the chronology but, depending upon the degree of accuracy desired, some work at the individual site may become necessary. The nature or classification of a site involves establishing certain basic characteristics, such as the distinctions between settlements and cemeteries and a number of specifically regional research goals, such as the position or function of a settlement in relation to its neighbours. But in order to do that, and to determine what kinds of settlements existed, individual settlements have to be investigated.
As has already been mentioned, the present study is primarily intended as an inventory of the available data. To this purpose, all the archaeological information has had to be collected, evaluated, and interpreted. As a first step, this involved a survey of the pertinent literature, museum archives, the *Centraal Archeologisch Archief*,[27] and a description of all material stored in private and public collections. An exception was made for published or unpublished excavations where such a description was either superfluous or impossible. In addition, active collection of new data was begun by supporting an archaeological survey carried out by the regional organization of amateur archaeologists,[28] but this yielded useful results only in a few areas. Secondly, the scrutiny of maps produced by several soil surveys revealed a number of so-called ancient settlement soils[29] which had never been investigated. These were visited in order to collect surface material; in one case, site 126 in Meinerswijk, the visit led to a small-scale excavation.[30]
Especially with regard to the older finds or reports on older finds, the reliability of the information had to be evaluated; this included the vagueness of findspot-indications, accuracy of identifications of finds no longer available for inspection, and the credibility of informants or antique dealers. After this evaluation was completed, a large amount of information about findspots and finds remained to await further interpretation. The particular details of this interpretation are outlined in paragraph 4.2, but for the sake of terminological clarity the relevant concepts should be defined here.
At the lowest level, there are locations where material has been found: the findspots (*vindplaatsen, Fundstellen*) which are the direct or indirect result of human activities. These have to be interpreted in spatial terms as well as on the basis of the sort of actvity for which they

26 Bloemers/Hulst, in prep.
27 Central Archaeological Archives, henceforth to be referred to as the CAA. The CAA is a storage system, now partly computerized, for all archaeological information from the Netherlands. Its maintenance and development is a task of the ROB in Amersfoort, where it can be consulted.
28 The Netherlands Association of Amateur Archaeologists *(AWN)* Subdivision Nijmegen and environs *(Afdeling Nijmegen e.o.)*.
29 See paragraph 3.5.
30 Willems 1980a, 1980b. See also below p. 54–5.

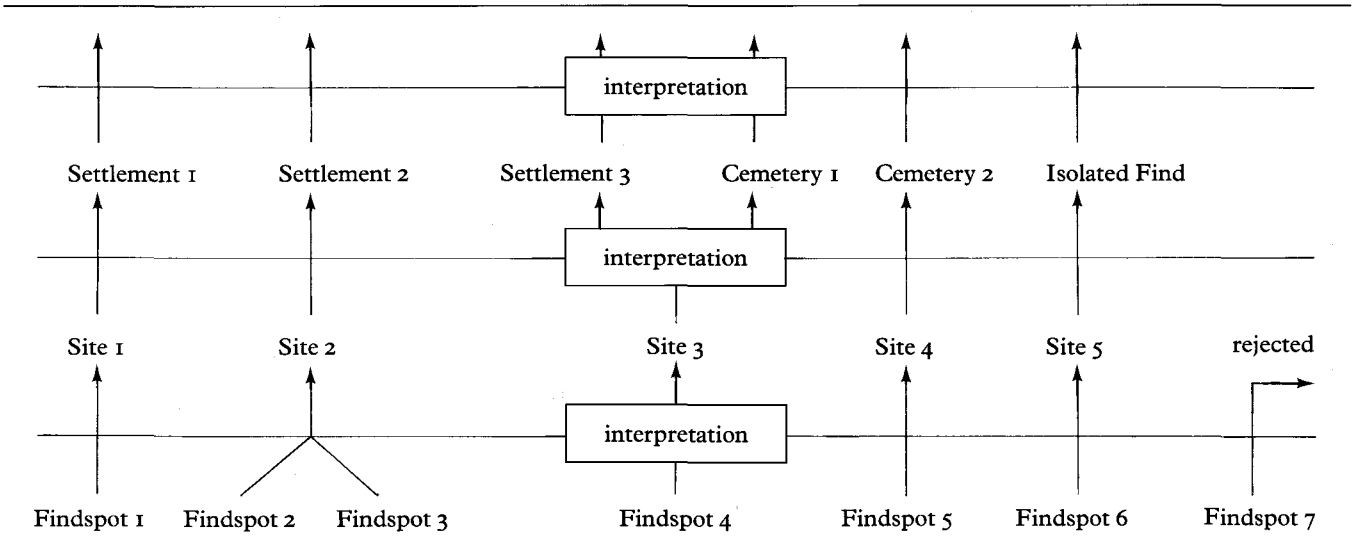


Fig. 5 The relation between findspots, sites, and types of sites

served. Findspots are thereby converted into sites, either by themselves or by combining one or more spatially associated findspots into one site. The general type of activity then leads to a general classification of a site as a settlement-site (a settlement), a burial-site (a cemetery) or, in cases which are neither, a special site (an isolated find). The latter includes findspots which represent a single activity, such as hiding a treasure or leaving an offering, which cannot be defined because they are the locations of stray finds, or which are merely the indirect result of human activities. The latter kind of findspots can also be completely irrelevant and thus be rejected (not graded as a site), as in the case of stray sherds transported by water and found on river shores.

It should thus be kept in mind that concepts sometimes used interchangeably elsewhere are employed here with a specific meaning in a conceptual framework. A settlement is always a site but a site need not be a settlement; a site can be identical to one or to several findspots and, conversely, one findspot may be the location of more than one site; a stray find is only one example of an isolated find, and so on.

The various concepts are presented in fig. 5, which also indicates the ongoing interpretational process which should lead to the recognition of the various types of settlements, cemeteries, and other sites, which, in their turn, are important building blocks for further analysis.

Fig. 5 also indicates the level of interpretation at which the evidence is presented in this article. The analysis of (especially) settlement types and regional relations and processes belongs to the second part of this study. As seen from the project, this is, of course, still only a first step.

It is clear that all interpretations require testing in the field, that there should be a feedback between the specific or general hypotheses and the basic data. It is still relatively easy to test the proposition that, on the basis of its surface finds, settlement x should be a military settlement (for example, a frontier fort). But it is far more complicated to investigate hypotheses on hierarchical relations between settlements, their differential participation in regional economic or social subsystems, and the like. Nevertheless, the project has (also) been designed to do just that: instead of carrying out haphazard rescue archaeology, research should become problem oriented in that the sites selected for excavation are chosen because that excavation is expected to contribute to the improvement, confirmation, or rejection of as many hypotheses or ideas as possible. The present study is not primarily concerned with excavations, but many of its propositions are based on the outcome of recent excavations and can only be checked by carefully planned new research at the site and household levels.

The latter is, by the way, not very easy to do, mainly because hardly any floor levels have survived. In some cases and for some purposes a traditional solution in the

form of structural details is available. For example, some house types clearly have living-quarters and a byre.[31] In others this is not evident, but the location of a byre can still be determined, for example, by means of phosphate analysis. In general, however, detection of specialized activities at the household level is extremely difficult and tends to rely mainly on generalizations from incidental, fortuitous discoveries.

The probability of such discoveries is, on the other hand, greatly enhanced by the research strategy at the settlement level which, in principle, aims at nothing less than its complete excavation, even though this ideal is only rarely realised completely. This approach is, of course, typical for Dutch archaeological practice as a whole and inspired by the general shallowness of sites and the absence of floor levels, thus allowing relatively fast excavation of large surfaces, and also by the fact that virtually only threatened sites are investigated and anything left unexcavated is inevitably lost.

Nevertheless, it is possible and from several points of view – among which an effective expenditure of time and money is not the least – even desirable to employ some kind of sampling strategy. But this, as a principle within the ERA project, has been rejected. It is felt that sampling at the site level, where complete or nearly complete (because the law of diminishing returns does, of course, pose certain limits to what is still worth while and what becomes superfluous) excavation is possible, would be ridiculous. On the other hand, and this is a point which has not been adequately considered in the past, the required time and money input presupposes extremely careful selection of the sites to be excavated. Such a selection can only follow from a better understanding of the regional variability, which brings us back to the regional level.

There, sampling is not only desirable but very necessary. The present study is based mainly on available evidence, but this is often clearly insufficient for several purposes and, even more seriously, its reliability is difficult to assess and at the very least needs some kind of probability sampling to be expressible in more exact terms. A discussion of some of the limitations of the available data will suffice to illustrate this.

As discussed in paragraph 3.5.2, there are reasons to assume that, because of the presence of ancient settlement soils, virtually all settlement sites on Holocene river deposits have been discovered. This would come close to the ideal situation where – in a statistical sense – almost the entire universe of settlements is available, at least those which have survived to the present day. For a few smaller areas this situation is not yet so optimal, but that is due to technical difficulties (depth of borings) which can be overcome by one season of concentrated research effort.

The latter is indeed one of the 'blind spots' revealed by the present study and which the project should eliminate in the future. But another factor should be added to such an undertaking. It is reasonable to suppose that only random errors are responsible for missing sites and the arguments presented in paragraph 3.5.2 support that notion. Nevertheless, it is essential to check this by a sampling procedure[32] so that the reliability of the data becomes more clear. In fact, such a procedure should be part of a regional, stratified sampling design because in itself it is only concerned with the area of Holocene deposits. The Pleistocene deposits are scheduled to be completely surveyed by the ROB at some time in the future, but this may never be realized. Therefore, the variety of sampling techniques which is available should be used to explore the various parts of Pleistocene deposits, each of which poses specific problems. For example, large parts of the Veluwe, the Rijk van Nijmegen, and the Reichswald are woodland, while portions of the coversand areas are effectively 'blacked out' by ancient cultivation soils or drift-sand.[33]

These problems are, of course, limiting factors as far as the present study is concerned and it is useful to indicate these in a more coherent way. Apart from factors of erosion, which are primarily relevant in the Holocene area, and those of coverage, which occur in different forms in the entire region (these are all discussed in chapter 3), there are a number of others. The woodland in some areas has already been referred to, but the coverage (not to mention the destruction) by medieval and recent settlement is even more important. Some of the work done a few years after World War II has become invaluable for this reason, but it was restricted in scope and primarily aimed at the river area, for which the soil surveys had

31 See Van Es 1973 for examples from sites 1 and 7.

32 See p. 75.

33 See p. 35 and fig. 9. A more encompassing account of the difficulties to be expected can be found in Woltering's (1979) evaluation of the Texel survey (esp. chapter 3, 15–47). Sampling may be an indispensable tool, but the effort involved in making sure that nothing is overlooked in the selected areas could well prove to be staggering.

provided a very extensive archaeological data base anyhow.

The latter leads to yet another factor which is very important for the present study, namely, the quality of archaeological reconnaissance. Again, in the river area this is very high because of the soil surveys. In addition, amateur groups from Kesteren and Nijmegen have, over the years, done quite a lot of fieldwalking in the Betuwe. The Nijmegen group has done the same in the Land van Maas en Waal, including the Pleistocene deposits around Wijchen; the previously mentioned 'systematic' survey had spectacular results in some areas, such as the coversands of Middelaar and Milsbeek.

This does, however, indicate that the available information is spread unevenly and to some extent is determined by the activities of amateur archaeologists, a phenomenon which is very difficult to handle as long as a regional sampling strategy has not been effectuated.[34] Areas which have not been very actively prospected by amateurs are the eastern part of the area north of the Rhine and IJssel, the Montferland, and the Reichswald. In the western part of the area north of the Rhine, the Liemers, and the Duffelt this situation is slightly better, and the same is true for the northern and western parts of the ice-pushed ridge in the Rijk van Nijmegen. It is considerably better in Brabant, south of the Meuse, but, as explained above, the relevant data could not be obtained, let alone incorporated in this publication. A similar problem, concerning known but unavailable sites, may also exist for the entire area of German territory. Information about all registered sites was generously provided,[35] but it could be that more have been discovered by amateurs, which are not registered.

In general, it can thus be concluded that the Holocene and Pleistocene areas are not comparable as far as the level of information is concerned. For the ERA project, that situation can be remedied as outlined above, but for our investigation it is a built-in deficiency. As long as this limitation is taken into account, there is no need for concern, for limitations always arise at any stage of research. The available information on individual sites, for example, is sometimes rather minimal, as can be seen in chapters 4 and 5. This is also something which can be improved at a later stage by sampling a considerable number of settlement sites.

Although this seems to be contradictory to what has been said above about research at the site level, it is not. The program which is currently being prepared involves nothing more than a trial trench and/or a few small test pits intended to provide some insight into the chronology, stratigraphy, and the general nature of those settlements, in order to be able to assess the site with more accuracy at the regional level. If more information about the site level is desired, excavation should follow instead of digging more random holes. In practice this will, however, be rare because the sites to be tested are in particular those which are protected under the Monuments Act or otherwise unthreatened and thereby debarred from excavation.[36] Until now, there have been only three sites which have been tested as preliminaries to this program. These are the trial trenches – preferrably not to be called transect samples – in the centre of Elst (site 105), in Meinerswijk (site 126), and in Heteren-Uilenburg (site 95).

The first was intended to provide information regarding the extent of the important Roman centre under the present village, the second to investigate the general nature of the site, suspected to be a frontier fort, and the third was a rescue excavation.[37]

1.2 ROMANS AND BATAVIANS

In the preceding paragraphs, a number of technical, methodological, and theoretical aspects of the current investigation and its status within the ERA project have been outlined. Romans and Batavians (and others) have been kept in the background where they will largely remain, at least as far as the present paper is concerned. They will emerge – in a way that is not, or at least not primarily, historical – when the research has been completed. This will be presented in a subsequent paper, now in preparation.

34 For an illuminating study on this subject, see Hamond 1980 on Neolithic sites and amateurs in the West Jülicher Börde in Germany (the 'Aldenhovener Platte' project).

35 I am grateful to Dr W. Piepers (Rheinisches Landesmuseum, Bonn) for his cooperation and assistance in locating the relevant information.

36 The degree and rate of destruction of cultural resources is such that, with the available means, there will – for a long time to come – be more than enough sites which are judged to be relevant for the project and which can be excavated.

37 Elst: excavation in 1981 by R.S. Hulst (*JROB* 1981, 30); Meinerswijk: Willems 1980a, 1980b; Heteren-Uilenburg: see p. 102, note 16.

This article is mainly restricted to a presentation of the data which should lead to a better understanding of the how and why of developments in the eastern river area between 250 BC and AD 750. In chapter 2, a brief account of historical data which are relevant for a chronological framework is presented. Chapter 3 is devoted to the geological framework, together with a few related subjects. As such, it is also an account of the relevant sheets of the Archaeological Map of the Netherlands (Appendices 1–5), whose archaeological data are discussed in chapter 4. This also presents an overview of the chronology of the sites and the major certainties and uncertainties involved. Chapter 5 is a catalogue of all the sites, usually with an indication of the finds from them and the relevant older literature. Complete inventories of most sites (or the constituent findspots) are available from the CAA. The catalogue has been kept up to date until the end of 1981. All known later discoveries (from 1982) have been added,[38] but these could no longer be depicted on the distribution maps.

In chapter 6, a number of find categories and aspects thereof are examined in more detail. These include matters of typology and chronology, but also of production and of the possible implications of relative frequency of certain artifacts and their distribution. In some ways this chapter already anticipates later subjects, such as chapter 8 which is devoted to the typology of the various sites in different periods. Because this matter is intimately related to the final conclusions, or rather proposals and hypotheses, of this investigation, it is not included in this article but will be incorporated in the second part of the present study.

This will also include the final report on the small excavation in Arnhem-Meinerswijk (chapter 9). The main purpose is, however, to investigate native society and the ways in which it changed during and after the Roman occupation. This will be done primarily by considering the development of the settlement system and the social and economic implications involved. Naturally, the particular historical and geographical context also calls for an evaluation beyond the regional level, which may indicate what is specific for the region and what is not. It is expected that while, on the one side, general information form elsewhere will be necessary to supplement the lack of it at a regional level, the research in the river area will, on the other, become relevant to more general subjects. Thus, a consideration of the economy will presumably not (yet) be based on sufficient regional ecological data. It is also hoped that this study will not only make a substantial contribution to our knowledge of Roman military establishments along the Rhine but be relevant to the wider field of 'frontier studies'.[39]

38 I am grateful to R. S. Hulst, who provided most of them.

39 See e.g. the various papers in Miller/Steffen 1977 and Savage/Thompson 1979, or Dyson 1974.

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This article could never have been written without the generous practical support of Rudi Hulst, who has been working in the river area for nearly twenty years. The general perspective of the research has been influenced by many people in the Netherlands and abroad, but I am particularly grateful to my friend Roel Brandt for endless hours of fruitful discussions.

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2 HISTORICAL DATA AND PERIODIZATION

Although most of the relevant sources about them refer primarily to events that took place in the 1st century AD, the Batavians are among the most well-known tribes in the Roman empire. This probably also arose from the romantic image that grew up around them, in part already during the Roman Period. The poet Martialis (c. AD 40-104) describes them as big and strong, with blond or red hair, [1] and Tacitus (c. AD 55-120) tells us about their prowess in war and their mastery of the art of swimming.[2] They were in demand as soldiers[3] and their reputation made them desirable bodyguards for the emperor himself and his family, as we know from Caius (Caligula) and Agrippina.[4] This fame lasted into the 4th century as demonstrated by the request from Constantius II, emperor in the east, to Julian, then still *caesar* in the west, in which he specifically demands a contingent of soldiers including Batavians, who were apparently considered indispensable to the late-Roman imperial guard, the *scholae*.[5] Julian's somewhat evasive answer[6] may have been due to his better knowledge of the conditions in the northern part of the empire.[7]
The glory of the Batavians lived on into later times, sometimes even by forged inscriptions.[8] More recent examples are the name Batavia for the colonial capital of the Dutch East Indies (now Indonesia) and the Dutch state being called the *Bataafsche Republiek* (Batavian Republic) from 1795 to 1806. Even today, the story of the Batavians who sailed down the Rhine on rafts is a more cherished tale in Dutch primary schools than the (American!) myth of little Hans Brinker, who stuck his finger in the dike.
These examples are not just amusing details. They are

1 Martialis, *Epigrammata* XIV, 176.
2 Tacitus, *Hist.* IV, 12; *Ann.* II, 8.
3 See e.g. Sprey 1953, 26–7; Bogaers 1960–1, 285, note 130; Alföldi 1968, 13–4, 45–8.
4 Caligula: Suetonius, *Cal.* 43 and Cassius Dio LV 24,7; Agrippina: Tacitus, *Ann.* XIII, 18. For an overview of the German bodyguard, the *Germani corporis custodes*, during the Julio-Claudian dynasty, see Bellen 1981.
5 Ammianus Marcellinus XX 4, 1–2.
6 Ammanius Marcellinus XX 8, 13. He offered '*laeti*, the offspring of barbarians, born on this side of the Rhine, or at any rate *deditícii*, who desert to our side'.
7 *Batavia* was occupied by Francs at that time, as we know from some panegyrics (see De Boone 1954, 15), but this may not be relevant in the political context of the demand (see Jones 1973, 119–20). In any case the famous *vexillum Batavorum et Herulorum* was an important regiment and Batavians fought *cum regibus* at the battle of Argentoratum in AD 357 (Amm. Marc. XVI 12, 45).
8 See e.g. Brunsting 1970 (legitimation of a revolt). The work of Cornelius Aurelius: *Defensio gloriae Batavinae* (Antwerp 1586, see Brongers 1976b, 12) is an example. He also used the famous inscription .... *Gens Batavorum amici et fratres romani Imperii* (*CIL* XIII, 1338), disposed of by Bogaers 1976.

also an illustration of popular or political use of historical data which reflect the way the scientific community in a particular period thought about them. This chapter is, however, not devoted to reflections on the nature of scientific thinking, or to an extensive discussion of the available historical information. It is only intended to present a first interpretation of some of this information which leads to a periodization relevant to the purposes of the present study. Of course, the information and also its interpretation are not new, and the resulting periodization is only one of several possible approaches.

2.1 LATE-IRON AGE – EARLY-ROMAN PERIOD

One of the themes of the ERA project is the effect of the Roman occupation, and this led to the inclusion of the Late-Iron Age in the entire time-range which had to be studied. Exactly when the Late-Iron Age begins, and why, is hardly relevant in the present context. The Iron Age was not subdivided by the 1965 Symposium for Dutch Prehistory,[9] but subsequent work[10] has led to the usual, obligatory division into 'early', 'middle', and 'late', the latter starting around the mid-3rd century BC. For our purposes, the last century BC would have been sufficient, but that was impossible for practical reasons. There are some artifacts, such as glass La Tène bracelets, which occur regularly and also allow somewhat finer chronological subdivisions, but in general one can be quite satisfied that it is at all possible to recognize late-Iron Age sites as such. The Late-Iron Age has thus been defined as a period starting around 250 BC and ending at the beginning of the Roman Period.

A fixation in time for the latter is, of course, very relevant indeed but unfortunately also very complicated. This has to do with the difficulties in interpreting historical information. There are various excellent recent discussions of the events reported in the classical sources and other data.[11] They begin with the campaigns of C. Julius Caesar, which started in 58 BC and reached northern Gaul in 57 BC. Between 57 and 51 BC, there were several campaigns against tribes or chiefdoms[12] who either inhabited (the Menapii and Eburones) or invaded (the Tencteri and Usipetes) territories in the southern Netherlands.

The effects of these events must have thoroughly disrupted native society. Unfortunately, nothing is known of the events in or directly relevant to the river area between 51 and 12 BC, when new campaigns started. The area was not subject to some sort of direct control, at least there are no historical or archaeological indications for it. On the other hand, inference from the relevant sources[13] allows the conclusion that the Batavi settled in the eastern river area after 50 BC and before 12 BC, as did some other tribes in adjoining areas. It is extremely difficult to understand the processes involved in these events. There must have been possibilities created by the disruption of the supra-regional balance of power, but how they were used and by whom is not exactly obvious. Occasionally, such as the moving of the Ubii by Agrippa in 39 or 38 BC, we know at least that it was not an autonomous move. The degree of Roman interference with the moving of the Batavians is unknown. It may have been considerable, despite Tacitus' remarks on the *seditio domestica* among the Chatti[14] and also the *vacua cultoribus* of the new Batavian area, which is contradicted by archaeological evidence.

In any case, there were many changes as a result of Caesar's campaigns and the fact that the river area was probably not actually conquered or pacified is in itself no reason to reject 57 or 50 BC as a relevant caesura between Iron Age and Roman Period. We may even be in for a few surprises in this respect. The region of Rossum/Lith has recently[15] been suggested as the site of a major Batavian settlement. If the interpretation of the dredged finds is correct, one might even go further and postulate a Caesarian or post-Caesarian camp of Batavian auxiliaries there.[16]

9 See *BROB* 15–16, 1965–66, 7–11.

10 Verwers 1972, 123–4; Lanting/Mook 1977, 9–10, 147–66.

11 See esp. Von Petrikovits 1978, chapter 2 and Van Es 1981, chapter 3.

12 For a discussion on the nature of societies in northern Gaul, see e.g. Roymans 1983.

13 See Sprey 1953, 16–9, but compare note 16 below.

14 Tacitus, *Hist.* IV, 12. See also Sprey 1953, 16–9.

15 Roymans/Van der Sanden 1980.

16 Which could (but not necessarily) revive the discussion on the probably corrupt (*ER* I, 46 and Sprey 1953, 16–9) reference to the Batavi by Caesar (VI 10,2) and their presence among Caesar's troops mentioned by Lucanus (*Pharsalia* I, 431). For a discussion of the archaeological evidence for such units, see Wightman 1977, 117 ff and Furger-Gunti 1981. For Celtic coins in Roman camps, see also the overview in Gechter 1979, 71–6.

Native 'national' irregular units with their own equipment and under their own commanders are normal in this period[17] and the silver Celtic coinage may also provide a clue, because it is a replacement for denarii in the payment of troops.[18] Moreover, the presence of Batavian troops at a strategic spot in the river area may have provided them with a readily available region to settle down in when their kinsmen were expelled by the Chatti.

For the time being, all this is pure speculation, but it is a warning against a hasty rejection of sizable Roman interference in the river area before the arrival of Drusus in 12 BC. Nevertheless, it remains speculation and it is quite well possible that 'the Batavians', whatever that may entail, moved to the river area only in the second decade BC, as a direct preparation for the planned campaigns into Germany. These plans became realistic after the campaigns in Spain had been successfully completed by 19 BC.[19] In any case Drusus, who was sent to Gaul by his stepfather Augustus in AD 13, found in the river area a readily available base for his operations. From then on, this region was directly and continuously involved in Roman military affairs. As a relevant date after which things started to change, 12 BC is not unsuitable. It definitely is the moment when a really tangible process of interaction must have started, after the preliminaries in the preceding decades. Although its tangibility in terms of archaeologically testable hypotheses will be minimal as far as the initial stages of this process are concerned, 12 BC can serve as the accidentally surviving caesura between Iron Age and Roman Period.

2.2 EARLY-ROMAN – MIDDLE-ROMAN PERIOD

The Roman influences in the river area, which became an assembly area for military operations (*Bereitstellungsraum*) in 12 BC, were channelled through the army for a long time. The successive campaigns of Drusus (12-9 BC), Tiberius (AD 4-5), and Germanicus (AD 15-16) testify to the continued importance of the region for expeditions into Germany. The region also became organized from an administrational point of view, undoubtedly by working through the native social structure and using local chiefs as intermediaries. At least these people (but presumably also others, especially solders) had adopted some degree of Roman norms and values fairly quickly, as illustrated, for example, by their names. The first generation we know of, and which must have been in power roughly in Ia, still has native names like Chariovalda and Vihirmas.[20] The latter's son Flavus (the Blond), who was *summus magistratus* of the civitas Batavorum in approximately Ib,[21] has a Roman name but lacks the *tria nomina* of citizenship. The third generation, however, which was in charge around Ic, definitely had Roman citizenship: we know of Julius Civilis, Claudius Paulus, Julius Maximus, Claudius Labeo and also Civilis' nephews Claudius Victor, Julius Briganticus, and Verax.[22]

Exactly when a civitas Batavorum was formed is uncertain, but it is logical to assume that this happened in IA.[23] Even without a detailed account of all manner of military and non-military developments, one can say that a clear military and administrative structure had emerged during the Flavian period, and that the integration of the region into the empire was formally completed and exerted an increasing influence upon the lives of its inhabitants.

Two moments can be identified which could serve as significant turning-points leading to this and later developments. The first was the Batavian revolt of AD 69–70, an event which attracted much attention and had very important consequences, such as the stationing of a legion in Nijmegen and the growth of a new capital, *Ulpia Noviomagus*. If it is seen as the final episode in a process

17. E.g. Kraft 1951, 38–9 and Callies 1964, 139–42. An early Batavian example is the cavalry unit under the command of Chariovalda, *dux Batavorum*, who participated in Germanicus' expedition of AD 16 (Tacitus, *Ann.* II, 11).

18 E.g. Wightman 1977, 120.

19 For accounts of the events and the various plans of Augustus and Agrippa, see Wells 1972, chapters 1 and 5, and Von Petrikovits 1978, 53–5.

20 Chariovalda: Tacitus, *Ann.* II, 11; Vihirmas: *CIL* XIII, 8771, and Bogaers 1960–1, 268–70.

21 As Bogaers (*op. cit*, 270–1) has shown, the inscription must be early for various reasons. He dates it in IA. Although this does not really prove anything, Flavus does not figure in Tacitus' stories of the Batavian revolt. In any case, he is likely to have belonged to the generation before that of Civilis, who was born c. AD 25. This puts his father Vihirmas in Chariovalda's generation.

22 For further references see Bogaers 1955, 189. Only Verax, who helped Civilis in the attack on Vada, may be an exception.

23 Cf. Bogaers 1960–1, 271–4 and Bloemers 1978a, 83–4. Contra: Rüger 1968, 94.

of consolidation, after which the blessings of the *Pax Romana* could finally be enjoyed,[24] then it would be quite suitable as dividing-line between 'early' and 'middle' Roman. There is much to be said for such an approach, which would have an additional advantage because 'early' would be synonymous with 'pre-Flavian', as opposed to 'Flavian and later' which is a major division in typochronological schemes.

The Batavian uprising is, however, also a native revolt which exhibits a number of characteristic features common to numerous similar events in comparable situations in the more distant as well as recent past.[25] Phenomena like romanization or westernization often create opposition in the form of political and religious movements described as 'nativistic' or 'prophetic' or 'messianic'.[26] Essentially, they are a response to strong acculturative pressure, headed by a charismatic leader and taking place some time after the initial conquest or pacification. The description of the Batavian revolt as one example of a rather common phenomenon does, of course, in no way inflate its significance as a historical event. Had it been successful in the end, like the revolts of Arminius in AD 9 or, to some extent, the Frisians in AD 28, it would have been a major break in a process which had only just begun. As it turned out, the rebellion only accelerated further the developments which had started it.

As a historical date of major importance for the development of the river area, AD 47 is much more relevant. In that year, the general Corbulo was recalled from his expedition into Germany by the emperor Claudius.[27] This decision led to the final abandonment of the old plans for expansion into Germany and the establishment of a closed frontier system along the Rhine, the *limes*. This did not mean some kind of Maginot Line, but a replacement of the offensive, expanding structure by a system of preclusive defence.[28] In Lattimore's terminology,[29] it is a change from an open 'frontier of inclusion' to a 'frontier of exclusion'. For the river area, it implied the construction of a permanent base-line, in the form of military forts along the Rhine (see fig. 6).

Of course this was a more or less gradual development. Some forts, such as those in Vechten, Meinerswijk (126), and possibly in Driel (117) and south of Herwen-De Bijland (183), already existed under Augustus. Some others, but at least Valkenburg, were also built before AD 47.[30] A considerable number of forts[31] or, to be on the safe side, initial military strongholds, can, however, be related to AD 47: Leiden-Roomburg, Alphen a/d Rijn, Zwammerdam I, Bodegraven, Woerden, Vleuten-De Meern, Utrecht I, as well as Cuijk (499) in the hinterland. There is not yet enough evidence for a number of other forts, but it is quite possible that Meinerswijk II (126) and Duiven-Loowaard (194) should also be included, although the former was certainly occupied earlier (but not necessarily continuously!) and the latter has yielded some material which should preferably be dated at least a decade earlier.

There are some sites about which nothing can be said with certainty, such as Katwijk, Wijk bij Duurstede/Rijswijk, Kesteren (37), and Randwijk (18b). In a few cases only has evidence for a dating after the Batavian revolt been found. These are Maurik, Herwen-De Bijland (182), and probably Rindern(450).

In general, it can thus be said that a chain of forts was probably erected around AD 50,[32] the remaining gaps being filled in during the massive rebuilding effort which became necessary after AD 70. At least most of the evidence points to a concentrated instead of a long drawn effort, even though we may not be dealing with fully fledged forts in all cases. Therefore AD 47 – or, as a generalized caesura, AD 50 – represents not only in principle but also in practice a significant change in developments, which is very suitable as a chronological divide.

24 See Van Es 1981, 215.

25 For an admirable analysis, see Dyson 1971. Compare also Dyson 1975.

26 For general comments, see Linton 1943, Lanternari 1963, or Köbben 1971 (esp. chapter 4: *Weerstanden tegen verwestering*).

27 Tacitus, *Ann.* XI, 18–20.

28 See Luttwak 1976, chapters 1 and 2.

29 Lattimore 1962, 469–91.Compare also Luttwak's notion (*op. cit.*, fig. 1.2) of a hegemonic v. a territorial empire.

30 See De Weerd 1977; Van Es 1981, 96–7.

31 For most of the places mentioned hereafter, see Bogaers/Rüger 1974. Directly relevant additional information is to be found in Haalebos 1977 (Zwammerdam), Beunder 1980, Bogaers 1980, and Haalebos 1980 (Bodegraven), Van Es 1981, 101-3 and Van Es/Verwers 1983, fig. 4 (Rijswijk), Haalebos 1976 (Maurik), Willems 1980a (Driel, Meinerswijk, Loowaard) and elsewhere in this article (sites 37, 18b, 117, 126, 135, 194, 182, and 183). Huissen (135) could also date to around AD 47, but it was probably not a fort.

32 In addition to the rebuilding (Valkenburg II, Vechten?, Meinerswijk II), which also took place at that time

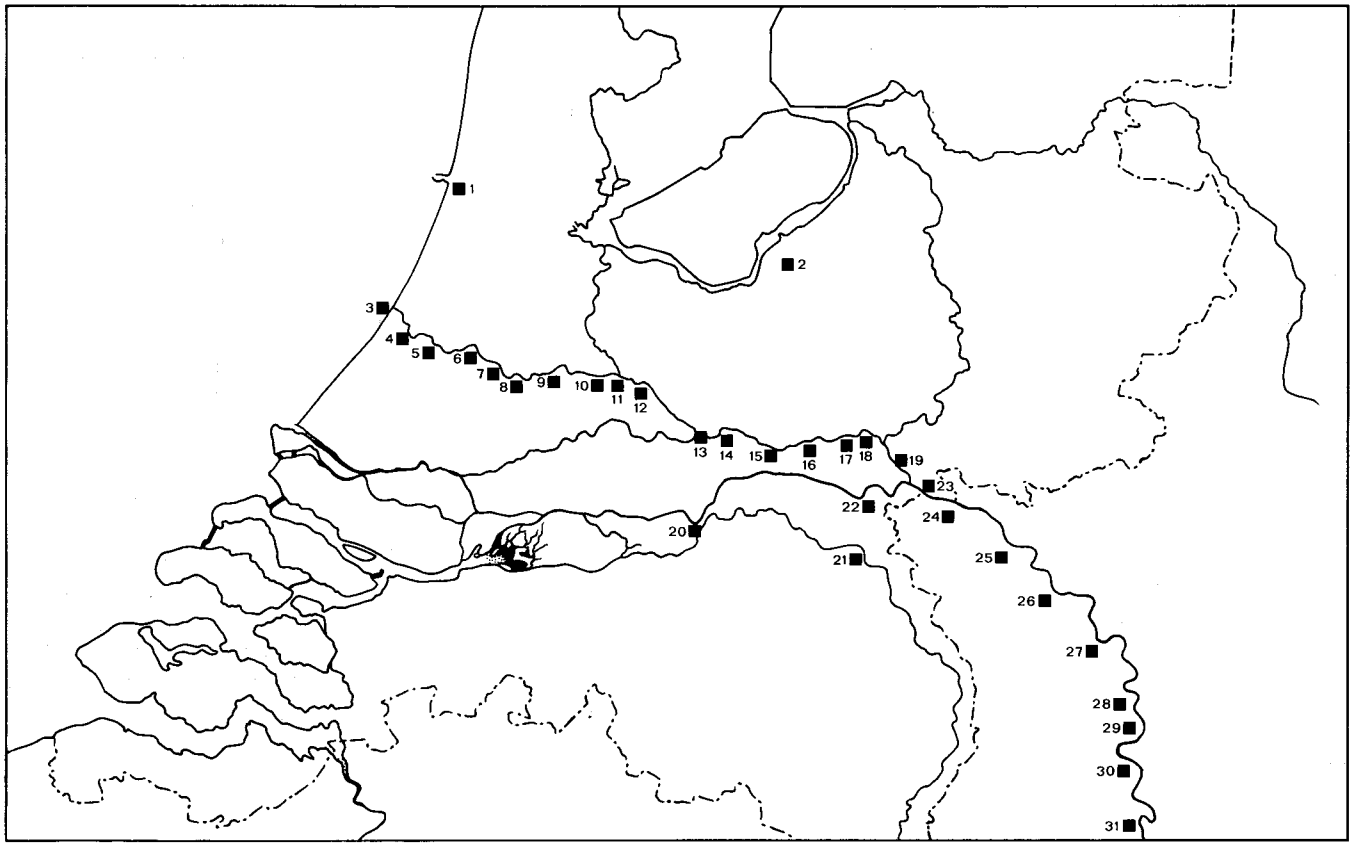


Fig. 6 Sites which are or suspected to be military establishments of some sort and related to the *limes* system: 1 Velsen, 2 Ermelo, 3 Katwijk, 4 Valkenburg, 5 Leiden-Roomburg, 6 Alphen a/d Rijn, 7 Zwammerdam, 8 Bodegraven, 9 Woerden, 10 Vleuten-de Meern, 11 Utrecht, 12 Vechten, 13 Rijswijk, 14 Maurik, 15 Kesteren, 16 Randwijk, 17 Driel, 18 Meinerswijk, 19 Duiven-Loowaard, 20 Rossum, 21 Cuijk, 22 Nijmegen, 23 Herwen-De Bijland, 24 Rindern, 25 Altkalkar, 26 Xanten, 27 Rheinberg, 28 Moers-Asberg, 29 Rheinhausen-Werthausen, 30 Krefeld-Gellep, 31 Neuss.

2.3 MIDDLE-ROMAN – LATE-ROMAN PERIOD

In contrast to the preceding issues, there is little to be discussed about the end of the period of continuous development which had started around AD 50. The various historical events which testify to its termination during the 3rd century AD have been amply documented elsewhere, together with diverse comments on the how and why.[33] It is not possible to select one specific event which represents a major turning-point for the river area. Instead, the generalized date of AD 270 seems to be quite suitable for our region, thereby following the usual argumentation for this part of the empire. From c. AD 240 until the death of the usurper Postumus in AD 268, who at least tried to maintain what was still left by then, the existing conditions came rapidly to an end. From the last decade of the 3rd century onwards, when the caesar Constantius Chlorus brought *Batavia* back under Roman control,[34] the military and administrative, social and economic circumstances were completely different.

33 For recent overviews, see Von Petrikovits 1978, chapter 3; Bloemers 1978a, 84–7; Van Es 1981, 47–9, and various references throughout the book; see also Luttwak 1976, chapter 3, and Bloemers 1983, as well as Willems 1983a for some preliminary remarks on the eastern river area.

34 See De Boone 1954, 57–8.

2.4 LATE-ROMAN – MEROVINGIAN PERIOD

During the last phases of the Roman Period, the river area was in the foremost line of a defence-in-depth system and thus perpetually subjected to invasions and counter-attacks. The effective frontier (*Machtgrenze*[35]) oscillated back and forth over the river area, which must have profoundly affected the way in which, or indeed the degree to which, habitation continued. Historical information on the river area is sparse for this period, but it is not absent.[36]

It is possible that a gradual process of disintegration from the empire took place during the 4th century, but changes may also have occurred more swiftly. Unfortunately, historical sources are of little use in evaluating this matter. The fact that *Batavia* was no longer inhabited by Batavians but by Franks, especially the so-called Salii,[37] does not necessarily imply anything in this respect.

It is, therefore, difficult to say when the Roman Period came to an end. Van Es[38] has remarked that nothing really changed from the 4th century into the 5th and that is, for example, also the story told by the imports into the eastern river area of common pottery from traditional sources.[39] Nevertheless, the reign of Honorius was the last under which any sort of central authority was still backed by force. Either AD 423, the time of his death, or AD 406, the date of a massive successful invasion launched by the Vandals who crossed the Rhine at Mainz, may serve as formal historical events that terminated the Roman Period. For our region, the later efforts of general Aetius, who was the last major figure to resist various invasions in Gaul until he was murdered in AD 454, are hardly relevant any more.

The 5th century is characterized by the elimination of all remaining 'Roman' strongholds, among them the town of Köln which was conquered by Franks only in about AD 460, and a gradual unification of various areas controlled by warlords or petty kings. Some of these were initially still allies of Aetius, but a man like Chlogio (= Cloio), the king of the Salii, who by then were no longer in the river area but further south, was independent and already carving out an empire of his own.[40] His descendants, Childeric and especially Clovis (Chlodwig), succeeded in unifying a large area under a central authority again: the Merovingian kingdom.[41]

This process was more or less completed by the beginning of the 6th century. Although the eastern river area, as the region around the *Noita* (Nijmegen) of the Anonymus Ravennas (c. VIIIa), was probably never outside Frankish control, local developments and conditions cannot be deduced from the available sources. In order to acquire a reasonably clear picture of what happened after the Roman Period, it is thus necessary to investigate the entire Merovingian Period, instead of just the (from a historical point of view) preliminaries of the 5th century. It is difficult to judge whether the historical change from Merovingian into Carolingian with the accession to the throne of Pippin in AD 751 is at all a relevant moment to terminate this period. In any case, Franks had permanently settled in the river area by then. For our region, the decisive victory of Carl Martel over the Frisians in AD 720, which stimulated the hey-day of Dorestad as an international trade centre, is probably much more relevant. This was also approximately the time when imported and/or locally made wheel-turned pottery types finally changed significantly and thus, in view of the goals of the project and this study, is in several ways suitable as a termination point.

35 Von Petrikovits 1938.

36 See in particular De Boone 1954.

37 See Blok 1979, 11–21 and Van Es 1981, 56–9, both with references to further literature.

38 Van Es 1981, 53.

39 See below, pp. 167–70 and note 132.

40 De Boone 1954, 132, 140–4; Ewig 1980, 12–7.

41 The name is derived from Merowech or Meroveus (see note 40), the more or less mythical father of Childeric and presumably in some way related to Chlogio.

3 THE LANDSCAPE

3.1 INTRODUCTION

The vital importance of knowing as much as possible about the landscape, in which human activities take place, is self-evident. It is essential to present the archaeological information about those activities against the background of the landscape in order to understand them. But the landscape, just like its population, is not a static phenomenon, for it alters through time, both by natural causes and human interference. Thus, it is necessary to trace back those changes in time in order to see how the landscape looked during the period under investigation. In this respect, there is a difference between areas of Pleistocene and Holocene deposits, because – from an archaeological perspective – the latter have changed much more drastically and frequently than the former, sometimes up to the present day.

In both cases, there is a dependency on the geo-sciences which have to provide the basic data on which to proceed and these are not readily available. They have to be derived from very different sorts of publications and maps which do not always cover the entire area of study, are composed for varying purposes and according to different principles, and are published to different scales. Nevertheless, these data have to be refined or generalized, interpreted or converted in order to produce an archaeologically meaningful and homogeneous background.

This is essentially what the *Archaeological Map of the Netherlands 1:100,000* is all about. The production of this map is a long-term project which acts as an 'umbrella' for a number of studies in different regions and on different archaeological periods and phenomena, including the present one. The purpose and character of this Map, which is by no means a systematic series, need not be extensively discussed here.[1] After the initial map by Brongers[2] and two subsequent maps[3] the structure of this project has been reformulated, which has led on the one hand to some loss of uniformity[4] but on the other to substantial gains. These relate primarily to a new and uniform editing of the sheets and, even more important, a uniform – but not exhaustive – legend for the entire country.

Appendices 1–5 are the first sheets to appear in this new form; a series of sheets covering the province of North Holland is currently being prepared for publication by D.P. Hallewas. Although these and further maps are mutually comparable because of the uniform legend, they, and often even different parts of the same map, are not really uniform in the sense that they are based on the same amount and kind of information. As already mentioned, the reconstruction of the subsoil depends on the character and quality of available maps. In principle, the legend of the Archaeological Map of the Netherlands is composed of elements of three systematic series of maps, all published to a scale of 1:50,000. These are the Geological, the Geomorphological, and the Soil Map of the Netherlands. Hallewas[5] provides a very enlightening discussion on the nature of these different types of maps and the theoretical and practical problems which arise when they are employed in the reconstruction of former landscapes in areas of Holocene deposits.

In this chapter, a detailed account is presented of the cartographical sources used for the map on Appendices 1–5 and the particular difficulties which were encountered. Other topics are the characteristic elements of the landscape in the eastern river area, the reconstruction of former river-courses and the specific information on habitability and related issues in this region, which can be derived from the geo-sciences. Although directly related to these subjects, the difficult matter of the exploitation of the area, especially as regards agriculture, will be postponed to a later chapter on economical issues. Only the general suitability of various deposits is indicated here.

3.2 THE MAP: COMPOSITION AND SOURCES

The cartographical sources available for the eastern river area can be divided into two groups, namely, those which are primarily geogenetic and those which are more pedogenetic. This is a distinction that to some extent cuts through the division of geological, geomorpho-

1 See *JROB* 1979, 67–8, 1980, 52–3, and especially Hallewas 1981 for such a discussion.

2 Brongers 1976a, 39 and plate 7.

3 In Stoepker 1977 (Appendix) and Bloemers 1978a (*Beilage* 8).

4 Such as the abolition of the original division into 15 sheets which proved to be unpractical. It has been replaced by a verbal and pictorial description of the area involved.

5 Hallewas 1981.

logical, and soil maps. The first two are at least partly geogenetic. Soil maps can also be designed to indicate the genesis of a landscape, but this is often subordinated in favour of an approach intended to present primarily various types of soils and their properties. It is clear that the latter kind of soil-map is basically less suitable as a primary source for reconstructing former Holocene landscapes, although it contains a wealth of other information. For areas with Pleistocene deposits at the surface, there are fewer problems because these surfaces have remained more or less the same even though the soils have changed both through natural causes and anthropogenetic influences.

Another important disadvantage of many Dutch soil maps is that they are based on borings with a depth of only c. 1.20 metres, which means that information cannot be obtained about parts of former landscapes which are now below that depth. Nevertheless, especially in the river area, soil maps were the first to be used which great success for archaeological purposes.[6] These were regional soil maps, originally intended for agricultural purposes but composed with a very strong emphasis on the character and genesis of the landscape. They include the studies by Egberts, Van Diepen, and Pons[7] on the Betuwe, the Maaskant, and the Land van Maas en Waal which enabled Modderman[8] to execute comprehensive studies of their occupation histories. These publications have also been used for the present map of the eastern river area, although the main source for two-thirds of the area has been the new Geological Map of the Netherlands 1:50,000, namely, sheets 39 O, 40 W, and 40 O. Only the latter has been published to date,[9] but manuscripts of the others were made available by the Netherlands Geological Survey.[10] Together, they constitute the backbone of the map to which the southern part has been adapted with the help of various other maps.

All sources have, however, been converted to suit the objectives of the archaeological map, which aims at presenting those data which are relevant to the situation of the landscape between BC 250 and AD 750. It is necessary, therefore, to account for these conversions in a discussion of the deposits which have been differentiated in the legend, including also the reasons why some were left out. The deposits are treated in the same sequence as on the Appendices. This will be followed by a discussion of the characteristic elements of each of the regions into which the map can be divided, together with the sources which were used.

3.2.1 The Legend: Holocene Deposits

The Holocene deposits on the map consist almost entirely of fluvial deposits. The study of depositional processes of fluvial systems is a highly complex matter which need not be extensively discussed here. A basic introduction is given by Doeglas[11] and a detailed overview of the Holocene river-deposits which belong geologically to the Betuwe Formation[12] is provided in a recent study by Berendsen,[13] who investigated part of the central Dutch river area which is largely comparable to our region.

The Betuwe Formation consists virtually entirely of deposits of 'meandering' and 'straight' rivers, the latter being the recent or sub-recent and artificially straightened channels.[14] All fossil rivers were meandering rivers; deposits consist of the following lithological and genetic components (see fig. 7).

A Channel zone deposits (*stroomgordelafzettingen*) are agriculturally valuable soils. They can be divided into three components:

1 Channel-fill deposits (*verlandingsafzettingen, restgeulafzettingen*) are the remains of fossil channels (*restgeulen*). They consist of clay, deposited after the channel was cut off, and sometimes peat. Occasionally, channels still have a function in the drainage system as brooks or gullies.

2 Channel deposits (*stroomafzettingen*) consist of sands and gravel, sometimes with sandy clay. Especially in the eastern river area, these are as a rule based on Pleistocene sands, from which they are lithologically difficult to distinguish. They are deposited in the actual river chan-

6 See p. 9 and 70–5.

7 Egberts 1950, Van Diepen 1952, Pons 1957 and 1966.

8 Modderman 1949, 1950, and 1951.

9 Van de Meene 1977.

10 I thank Drs A. Verbraeck for his kind permission to use them and for the trouble he and his assistant, Mr J. van der Staay, took to instruct me in their proper use and on a variety of other subjects related to the geology of the river area.

11 Doeglas 1973.

12 Zagwijn/Van Staalduinen 1975, 24–5.

13 Berendsen 1982, chapter 5. Important other publications are Havinga 1969 and Verbraeck 1970, chapter 4.

14 Berendsen 1982, 96–8.

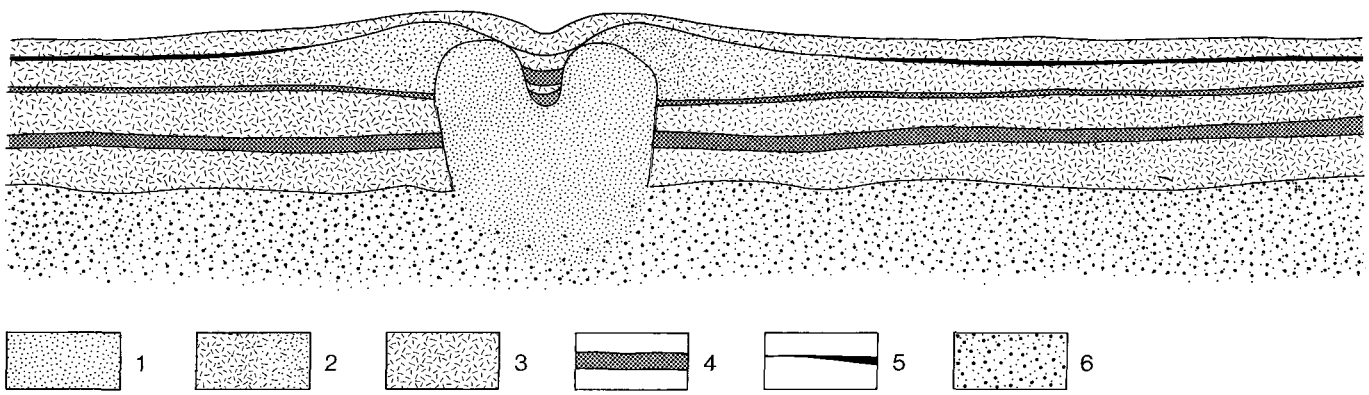


Fig. 7 Idealized profile with various characteristic deposits in the Holocene area: 1 channel deposits (sand), 2 bank deposits (sandy clay), 3 flood-basin and channel-fill deposits (clay), 4 peat, 5 fossil vegetation horizon, 6 Pleistocene deposits (coarse sand).

nel, and therefore their maximum extent constitutes the meander-belt (*meandergordel*), in which the remains of fossil channels can be observed.

3 Bank deposits (*oeverafzettingen*) consist of very fine sand and sandy clay. These are deposited outside the high-water bed of the river and continually increase in height after each overflow, thereby forming what is morphologically called a natural levee (*oeverwal*) on both sides of the river. The complex of laterally connected channel and bank deposits, which is visible as a ridge in the landscape, is morphologically described as a stream-ridge (*stroomrug*).

The height and the sand-content of the bank deposits decrease further away from the river, until they change into the

B Flood-basin deposits (*komafzettingen*) which are generally only suitable as grassland and consist of clay, usually interchanged with peat. The top of this peat is often connected to fossil vegetation horizons, visible as dark-grey layers (*laklagen*) in the basin-clay and occur especially in the transitional zone between stream-ridges and flood-basins.[15]

The third kind of river deposits is the result of a disruption of the normal sedimentation process, although they are by no means exceptional. These are the so-called

C Crevasse deposits (*crevasse-afzettingen, oeverwal-uitstulpingen*) also known as 'washovers'[16] or 'levee splays'.[17] These are the result of a more or less forceful overflow or break-through of the natural levee, resulting in a protrusion of the levee which usually consists of sandy and lithologically very complex deposits.

The units of our legend are in fact an abstraction from the complex geological and geomorphogenetic units as employed by Verbraeck and Berendsen. They indicate only major geological or geomorphological units which are relevant to the occupation during the Roman Period.[18] At a practical level, complex or very fine distinctions are not desirable because there are no detailed data for some areas and also because they are difficult to represent at a scale of 1:100,000. Crevasse deposits have, for example, not been indicated separately because a distinction from normal channel and bank deposits is irrelevant from an archaeological point of view. Various fossil channels and other small gullies were left out because they were difficult to represent. A description of the basic units employed is presented below.

Post-Roman channel (zone) deposits
and present river-courses

For the purpose of the map, all channel zone deposits can be divided into three categories: pre-Roman, those related to rivers functioning during the Roman Period,

15 See Havinga 1969, 24–5.
16 Verbraeck 1970.
17 Havinga 1969.
18 Actually this is only part of the era between 250 BC and AD 750, but it proved to be unnecessary to construct different maps for the successive periods. It is quite likely that some changes did occur, but these were not major ones. Also, they cannot be identified precisely enough.

and post-Roman (in practice: after AD 750). The post-Roman bank deposits covering older channel zone or flood-basin deposits have been deliberately omitted. In theory, they could be relevant in that their presence prevents archaeological observations in areas where they are well developed, but in practice this did not as a rule prove to be the case. Human interference, especially the construction of dikes from the 12th or even 11th centuries onwards[19] was undoubtedly an important factor, but the relatively high location of the archaeologically relevant deposits, those of the stream-ridges, has generally prevented too much sedimentation on top of them. Finally, the detailed soil surveys normally also reveal settlements (ancient settlement soils) in the rare cases where these are covered by more substantial later sediments.

In addition to the bank deposits, there are also the post-Roman channel deposits. These are more relevant because their presence implies erosion of previous deposits by post-Roman channels. Because river-courses changed only slightly after the Roman Period, this erosion is especially relevant to the Roman channel zone deposits. Only small portions of these have survived. In general, post-Roman channel deposits can be identified as those between the dikes along the present river-courses, but there are important exceptions to this rule. When they occur in the area behind a dike they are usually, but not always, identifiable because they are not covered by later deposists, because sometimes broad medieval or sub-recent channel-fill deposits or oxbow-lakes are present, and also because channels may be indicated on older topographical maps.[20] The opposite case, where (pre-) Roman channel deposits are still preserved beyond the dike under the present water-meadows, is almost impossible to establish because lithological differences are lacking or very difficult to evaluate. In this case, only the presence of an ancient settlement soil with Roman finds or other chronological evidence can provide a clue.

For this reason the post-Roman channel deposits on the map represent the maximally eroded area which will, in the future, prove to be smaller than is indicated. This is especially relevant for the Meuse deposits and those of the Rhine in Germany, for which no – or no detailed – geological information was available. In these regions, the lack of information on certain areas has led to their inclusion in the post-Roman deposits which may not be correct. In some instances this can already be proven by the presence of Roman Period settlements like 451 and 486 which are not eroded but seemingly on post-Roman deposits.[21] In these cases, the channel deposits on which they are actually situated, could not be bordered. Site 486 (Dieden) is particularly interesting, because it is situated on channel deposits of the Meuse which are covered by post-Roman bank deposits (not the site itself). The older deposits do not show completely on the soil map[22] which had to be used to construct this part of the map. Even though this soil map is also based on geogenetic principles, the depth of the borings prevented complete representation of the deeper parts of the older deposits.

Channel or fossil channel (fig. 8)

This unit is primarily employed to indicate those channels which are directly related to former river-courses, irrespective of whether or not they still carry water or certainly or possibly did so during the Roman Period. Genetically different channels, such as crevasse (or overflow), break-through,[23] and basin drainage channels, have usually not been indicated, thus avoiding too much detail which would be difficult to represent on the scale of the map and would also hamper an immediate understanding of the region's major drainage patterns. For the same reason, not even all fossilized remains of former river-beds could be indicated.

In order to keep the legend as simple as possible, the fossil channel symbol of the legend has also been used in areas of Pleistocene deposits, both to indicate still functioning streams such as the Niers and the Graafsche Raam and other drainage gullies, such as the presumed remains of late-glacial braided river systems,[24] as far as these are relevant to the occupation. For this reason, the braided system between Cuijk and Gasselt is indicated because the deposits with a high lying surface have – to

19 See e.g. Pons 1957, fig. 55.

20 See e.g. Hoppe 1970; STIBOKA 1975, *afb.* 20; Teunissen 1975, fig. 5; Van de Ven 1976; or Brunnacker 1978, *Taf.* I for various examples.

21 See Appendix 3. The same situation applies to the recently discovered site 542. See p. 40, note 54.

22 Van Diepen 1952, Appendix 2.

23 These are channels which do not just cut through one natural levee but through an entire stream–ridge.

24 Geologically, these are fluvial deposits of the Kreftenheye Formation.

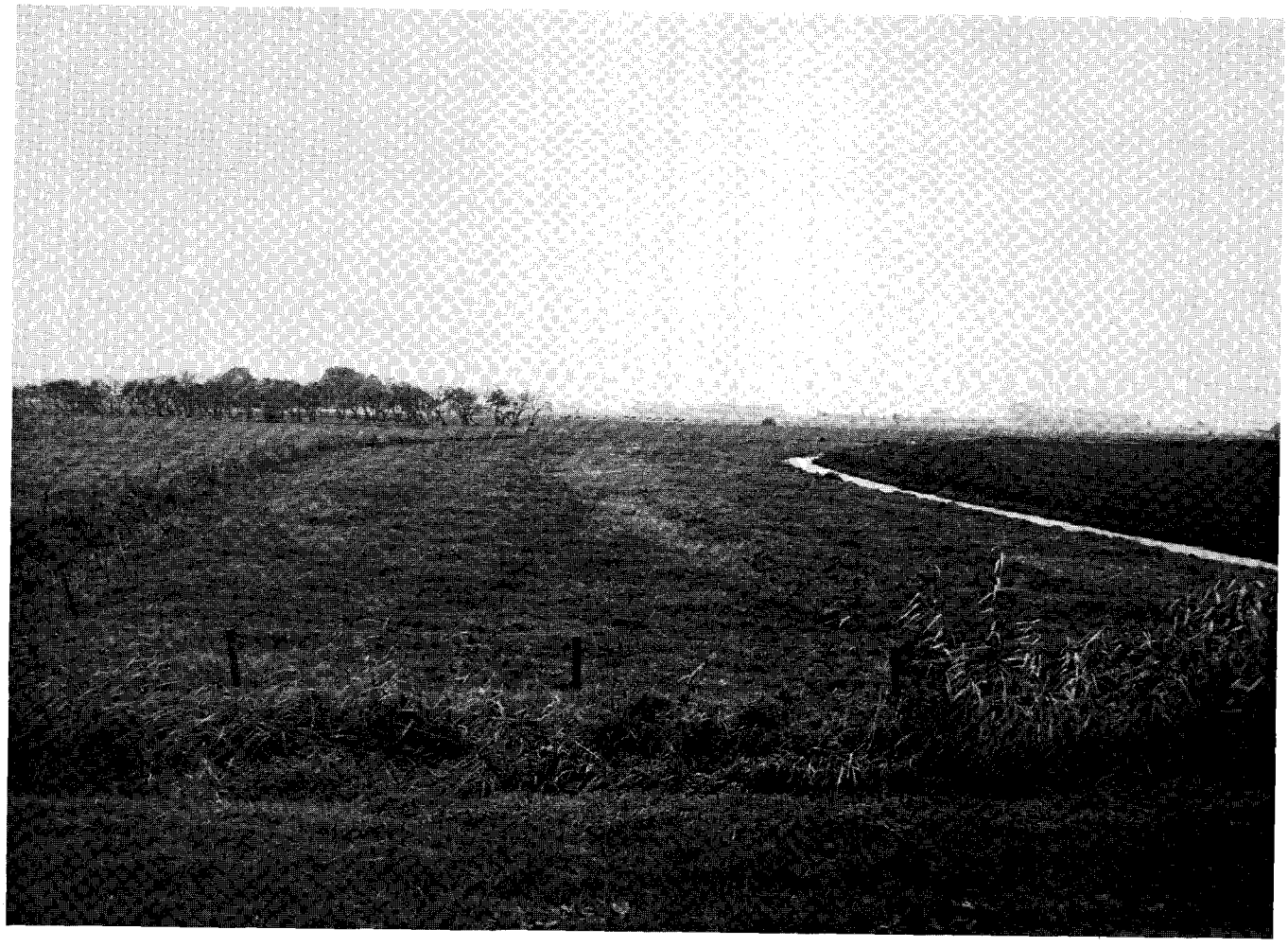


Fig. 8 View of a still very clearly visible fossil Rhine-channel in the Overbetuwe. It is the channel which curves around sites 117 and 118 in Driel, at the point where it is crossed by a modern road (see the Appendices, 185.35/441.30). On the right, the natural levee on the inside of the meander-curve is also visible.

some extent – been settled. The system in the Land van Maas en Waal[25] is not indicated because the channels there functioned primarily as basin-drainage channels and because most or all of the lower lying Pleistocene surface there is covered by presumably pre-Roman basin clay deposits (becoming increasingly thick to the west) and yields no indications of any permanent habitation during the Roman Period.

Pre-Roman and Roman channel deposits

These deposits are the meander-belts and some crevasse-ridges of rivers which functioned before and during the Roman Period. The representation of these on the map raised several difficulties. As already mentioned above, it is not always possible to separate Roman from post-Roman deposits. It should be noted that while the discovery of settlements dating from before AD 750 under present water-meadows can lead to the recognition of (pre-) Roman channel deposits, the absence of such settlements on deposits behind the dikes cannot be interpreted as evidence for a post-Roman dating of these deposits. This is because permanent settlements may be situated in the meander-belt of a functioning river, for

25 Pons 1957 and 1966.

example, on high lying parts such as point bars (*kronkelwaarden*) on the convex banks of meander curves, but a location on the natural levees was preferred. A striking example of the latter are sites 130–135 (on Appendix 3), which are just outside the meander belt of the Roman Rhine.

The pre-Roman channel deposits are another difficulty. In contrast to the Roman channel deposits, these were already stream-ridges during the Roman Period and in principle entirely suitable for habitation. These stream-ridges, however, vary greatly in age and are covered to very different degrees by later sediments such as bank and especially flood-basin deposits. When this situation occurs, the problem is to decide whether or not they were already covered by flood-basin deposits during the Roman Period and therefore should not be indicated on the map. If they have ancient settlement soils with Roman material, the problem is solved. If these are lacking, the problem is virtually insoluble, even in the only case where the results of very detailed surveys are available.

That is the area between Opheusden, Dodewaard, and Andelst in the Betuwe. In this area, there are two elongated crevasse-ridges, between approximately the co-ordinates 173/436 to 174/438 and 175.5/436.5 to 178/437. Havinga's very thorough survey[26] led to the conclusion that they were covered by basin-clay during the Roman Period. Nevertheless, there is at least one definite Roman and even Merovingian settlement (site 77) on the second ridge. Even if one assumes that all the other ancient settlement soils are indeed datable to the Bronze Age and or Late-Neolithic as Havinga concluded,[27] the presence of a permanent settlement in a flood-basin[28] is only understandable if the covered crevasse-ridge was still to some degree habitable. That this is true is demonstrated by the continued presence of an overflow gully along site 77 in the Roman Period.

For these reasons, the two crevasse-ridges have not been replaced by flood-basin deposits on the map, although Havinga's work has shown that at least large parts of them lay in a flood-basin during the Roman Period. Another argument is the fact that on the manuscript for the new geological map sheet 39O there are many instances where Roman settlements on stream ridges are covered by flood-basin deposits which are just as thick as those in Havinga's area of study, e.g. south of Ewijk and north of Herveld.

Because Havinga's work is the only study which is detailed enough to provide arguments to delete some of them, in practice all surviving pre-Roman channel deposits in the entire area are indicated on the map, including those on which settlements have not, as yet, been found.

Pre-Roman and Roman bank deposits

The bank deposits are, of course, directly related to the channel deposits, which implies that all bank deposits belonging to channel deposits considered pre-Roman and Roman are automatically indicated on the map. On the manuscripts of the new geological map sheets 39 O and 40 W, these are almost always described as a complex of bank- and flood-basin deposits, sometimes also as bank deposits on flood-basin deposits. The former, which are sometimes covered by flood-basin deposits of considerable thickness, are the result of the varying quantities of sand deposited by a meandering river on both sides of the meander-belt. In practice, these deposits can always be related to pre-Roman or Roman channel deposits. This is not true for the bank deposits on flood-basin clay, which may also belong to post-Roman channel deposits. When this situation is encountered far away from the present river-courses, as part of pre-Roman stream-ridges, there is no problem, but it occurs most frequently along the post-Roman channels. In that case they have been considered post-Roman also, unless there is evidence to the contrary in the form of settlements on the bank deposits. This is, for example, the case with the bank deposits along the Rhine near Zevenaar, along the Waal between Ewijk and Druten, and along the Meuse between Lienden and Batenburg. These are bank deposits of the Roman Rhine, Waal, and Meuse, belonging to channel deposits which have (presumably) been eroded.

Flood-basin deposits

In the flood-basin deposits, the presence or absence of peat has not been indicated, because it is very difficult to decide exactly where peat grew during the Roman Period. Only in the area between Wageningen and Rhenen has the part of the flood-basin deposit with peat been in-

26 Havinga 1969 and Havinga/Op 't Hof 1975.

27 Havinga 1969, 27–9. His conclusions have not been contradicted by subsequent finds and are most probably correct.

28 See Havinga 1969, Map 1 and 2, square 5 G, for a detailed picture of the situation.

dicated as such, in connection with the peat of the Singraven Formation.[29]

Flood-basin clay has been deposited over many pre-Roman and Roman channel zone deposits, but its thickness is usually rather limited. The same is true for the basin-clay covering some of the Pleistocene deposits, for example, to the southwest of Nijmegen and west of the Montferland. As mentioned above, clay-covers over channel zone deposits have not been indicated. The stream-ridges are more important from a geomorphological and archaeological point of view. The same situation applies to some of the Pleistocene deposits, such as the fluvial deposits between Cuijk and Gasselt, but in other cases areas are indicated as belonging to flood-basins.

As a result of this, some of the settlements on the Appendices appear to be situated in flood-basins. In the entire river area there is, however, not one single settlement for which that has actually been demonstrated. In each case there is an explanation for the apparent situation on basin-clay. For some sites, the reason is due to the inadequacy of the primary geological data, because parts of the flood-basin may not or only slightly have been covered by basin-clay during the Roman Period. Examples are sites 330 (Appendix 1), which is situated on Pleistocene gravel and sand deposits,[30] and 367, which is located on Pleistocene fluvial deposits with a relatively high lying surface. Similar situations may occur elsewhere, and it is quite well possible that in a detailed archaeological survey more sites will be discovered on specific pre-Roman deposits. As a result, these can then be differentiated from the surrounding flood-basin deposits on a map for the Roman Period.

Apart from these cases, there are also a number of settlements that are actually situated on bank deposits which are not marked on the available geological or soil maps or indicated in such a way that no satisfactory reconstruction of these deposits is possible. An example are the bank deposits south of the channel deposits around Ewijk, which were not indicated on the manuscripts of the new geological map, sheets 39 O and 40 W. A reconstruction with the soil maps of the same sheets would have been possible, but it would have resulted in a more or less arbitrary bordering of these deposits. Fortunately, in this case the data given by Pons[31] for this area allowed a reliable reconstruction of the true extent of the relevant bank deposits, because his soil map provided geogenetic data.

In other cases where no, or no adequate, geological data are available, such a reconstruction was not possible. Sites 251 and 252 are situated on flood-basin deposits in the legend of the geological map. The soil map (sheet 39 O) indicates channel zone deposits but these cannot be chronologically differentiated. It is clear from Pons' soil map that there are older channel zone deposits in the subsoil, but in this case no reconstruction was possible because only part of these deposits was reached by the borings.

There are quite a few instances like this, where it is clear why a settlement is located at a particular spot but where it is impossible to indicate this on the map without arbitrarily reconstructing deposits mainly on archaeological criteria. This would seriously affect the usefulness of the map, because the extent of deposits will have to be employed later to reach all sorts of conclusions about the habitation. If this habitation (the settlements) is used to reconstruct borders of deposits, we would end up with a perfect case of circular argumentation. Of course, there is already an element of circular reasoning in the use of archaeological data in the dating of deposits, but in that case the geological data remain an independent source. In addition, archaeological data are not the only means used to unravel the absolute or relative chronology of deposits.[32]

Peat and brook deposits

In general, the occurrence of peat in flood-basins and fossil channels has not been indicated, with the exception of the peat in the broad so-called Kermisdahl near Kleve[33] and the peat in the flood-basin between Wageningen and Rhenen.[34] This peat belongs to the Broek Formation, as defined by Berendsen.[35] As mentioned

29 See Zagwijn/Van Staalduinen 1975, 18–9.

30 Pons 1966, Appendix I, eastern sheet: a very small area of unit gY1 (described as 'coarse sand with gravel, contaminated with clay').

31 Pons 1966, Appendix 1.

32 The different ways of dating channel zone deposits are discussed in detail by Berendsen 1982, 113–44 (see especially p. 118), continuing on the course set by Verbraeck 1970 (chapter 4). The chronology of flood-basin deposits is discussed in Havinga 1969.

33 See below, p. 44.

34 See above, p. 33 and below, p. 37.

35 Berendsen 1982, 111–2.

above, the true extent of this peat during the Roman Period cannot be determined.
All other occurrences of peat belong to the Singraven Formation, which consists of deposits in brook-valleys and low lying areas or channels in Pleistocene landscapes. The extent of this peat is represented as far as it survives today, because areas with dug-out peat could not be reconstructed. This circumstance, however, only plays a role in the area between Veenendaal and Wageningen. The peat surviving there to date contained too much clay to be useful as fuel, but all the economically useful peat over the low lying coversands was dug out long ago.[36] As indicated in the legend, a substantial part of these Pleistocene sands may therefore have been covered by peat during the Roman Period. The same situation may have occurred in Brabant.
The valleys of brooks in the Pleistocene area usually contain some peat. Where this is absent or of minor importance, brook deposits are indicated. These also belong to the Singraven Formation and consist of sandy clay, silt, and clay.

Other deposits (not indicated on the maps)
For various reasons, several different and usually anthropogenetic deposits have not been indicated on the map. They are, however, very important in relation to the interpretation of the map, because they affect the archaeological visibility of former occupation.
First of all, the so-called ancient settlement soils (*oude woongronden*) are not indicated because they cannot be represented on a scale of 1:100,000. They are also already incorporated in the symbols for settlements on the map, so that separate representation on the geological part of the legend is not strictly necessary. The different aspects of this type of soil will be discussed in a separate paragraph.[37] It is extremely important for the discovery of settlement-sites.
Also important, but in a negative sense, are several kinds of deposits occuring in the Holocene and Pleistocene areas which may prevent the discovery of sites. Sites can be covered too deeply for material to be brought to the surface and recovered after normal cultivation of the soil. By implication, the deposits in question post-date the period represented on the map, and so they are omitted. Nevertheless, they are relevant because they may systematically or incidentally influence the visibility of archaeological patterns and should therefore be mentioned. Fortunately, none of them occurs very frequently in the area covered by the map.

The following deposits are concerned:
1 Drift-sand, which belongs to the inland dune sands of the Kootwijk Formation.[38] Drift-sand is the eolian redeposition of Pleistocene sands (mostly coversands) occurring throughout the Holocene. Most of the occurrences are, however, anthropogenetic in the sense that they are the direct result of bringing land under cultivation and destroying vegetation by removing sods during the Middle Ages. Small areas of drift-sand occur in the Montferland between Babberich and Elten, on the Veluwe north of Arnhem, and in Brabant south of Herpen and Berghem; with a few even smaller areas, all these are indicated on fig. 9.
2 *Es*-layers or ancient cultivation soils (*oude bouwlanden*), which result from centuries of fertilization – and thereby raising – of sandy soils with stable manure, compost, heath- or grass-sods, and the like. In some areas, this fertilization has been going on for around a millennium, and has produced a humus-rich layer at least 50 cm thick and sometimes even thicker. This, of course, usually prevents recovery of archaeological material.
On recent soil maps, the *es*-layers are indicated as *dikke eerdgronden*. They cover substantial areas on the sandy soils in Brabant, from Escharen to Schaijk, in the Montferland around Didam, and along the sides of the ice-pushed ridge Nijmegen-Kleve, especially south of Groesbeek and along the line Milsbeek-Mook-Malden. These, and other occurrences of *es*-layers, are also indicated on fig. 9.
3 Washovers (*overslaggronden*), which are the result of dike-breaks. These are lithologically and genetically comparable to the crevasse deposits caused by a break through a natural levee. Where the deposits resulting from dike-breaks cover older deposits, they have been omitted, just as the post-Roman bank deposits. In most cases, they are included in the post-Roman channel deposits, because they have eroded previous deposits. A remarkable exception is the washover in Deest (174/433), indicated on Pons' soil map.[39] It must have been of only minor importance because three Roman sites (219–221, possibly one settlement) survived undisturbed.

36 STIBOKA 1973, 51; Van de Westeringh 1983, 13.
37 See below, p. 70.
38 Zagwijn/Van Staalduinen 1975, 16–18.
39 Pons 1966, Appendix 1, eastern sheet, soil type RR02.

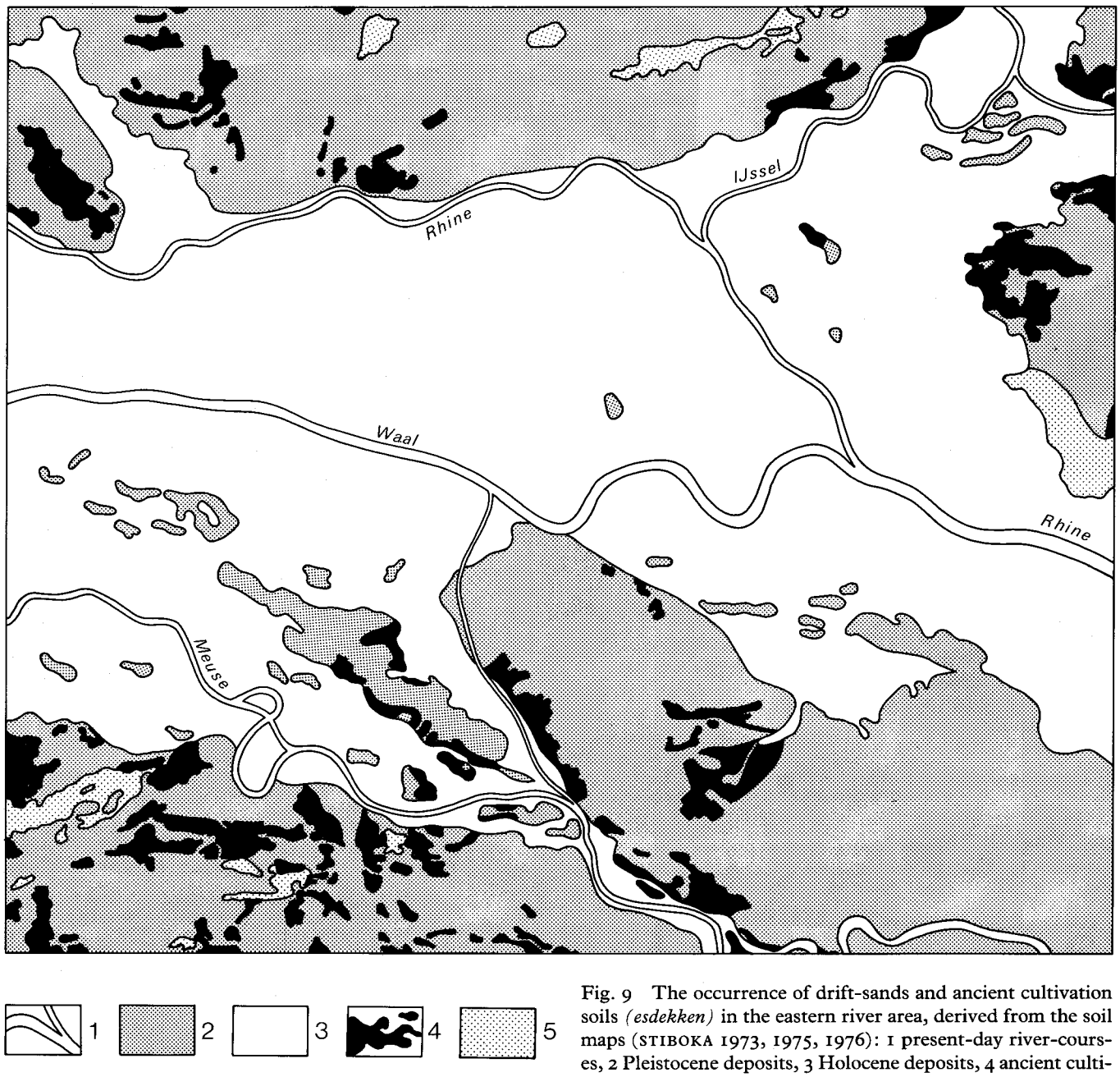


Fig. 9 The occurrence of drift-sands and ancient cultivation soils *(esdekken)* in the eastern river area, derived from the soil maps (STIBOKA 1973, 1975, 1976): 1 present-day river-courses, 2 Pleistocene deposits, 3 Holocene deposits, 4 ancient cultivation soils, 5 drift-sands. Scale 1:250,000.

3.2.2 The Legend: Pleistocene Deposits

The geological history of the eastern river area during the Pleistocene need not be discussed here. Adequate overviews are provided in the explanatory memoirs to the geological and soil maps of the region.[40] The Pleistocene landscape has remained relatively unchanged from the Roman Period onwards. Post-Roman changes, such as the digging of Holocene peat, the deposition of

40 Van de Meene 1977 and STIBOKA 1973, 1975, and 1976.

drift-sand, or the development of *es*-layers have been accounted for in the preceding paragraph. The legend for the Pleistocene has been composed in such a way that only the major types of deposits at the surface are shown, thus giving at the same time an insight into the genesis of the landscape and its morphology.
Although the legend of the Archaeological Map of the Netherlands permits the use of contour lines as an additional aid to indicate the relief, these have been omitted because they would seriously affect the readability of the map on and around the ice-pushed ridges. The only additional geomorphologic distinctions indicated are the hills and ridges in the coversand landscape and the dry valleys, which were formed by meltwater during the last ice age (Weichsel). Apart from being a distinctive element themselves, the direction and length of these valleys are, together with the choice of colours for the deposits, very good indicators for the general relief of the landscape.
A brief description of the different sorts of Pleistocene deposits at the surface is presented below.

Fluvial deposits
The surfacing deposits of Pleistocene rivers belong to the Kreftenheye Formation,[41] formerly known as Fluviatile Lower Terrace (*fluviatiel laagterras, Niederterrasse*) which was described in detail by Schelling.[42] They consist of clay (loam) and sands which were deposited by braided river systems mainly at the very end of the Pleistocene, during the late-glacial of the Weichsel ice age.[43]
The gullies are indicated by the same symbol as Holocene fossil channels. They are often filled with Holocene deposits. The deposits are divided into those with a low lying surface (usually wet and formerly described as 'low greyish-brown loam soils' or 'low grey river terrace soils') and those with a high lying surface, which are usually drier and also known as 'high reddish-brown' or 'medium high mottled river terrace soils'. The highest parts of the latter have usually been arable land for centuries and can presumably be considered suitable for various purposes during the Roman Period, although, with the exception of several sites around Cuijk, habitation is virtually lacking.

Coversands
A large part of the surface of the Pleistocene landscape consists of fine to medium coarse sands, which are indicated as coversands on the map. This category comprises two different – although lithologically very similar – deposits distinguished by the Netherlands Geological Survey. The first are the coversands proper, which are eolian deposits of local origin belonging to the Twente Formation.[44] The second is the result of eolian redeposition of fluviatile sediments, and therefore part of the Kreftenheye Formation.
While the former occur as a cover over older deposits in the Pleistocene landscape, the latter occur as river dunes in Holocene areas and are usually partly covered by Holocene deposits. They are known as *donken* (hummocks).[45] Usually these are fairly small but especially in the eastern part of the Netherlands they can be very large as is evident from the large sand ridge of Wijchen in the Land van Maas en Waal, which is in fact a *donk* (see fig. 10).
There is only one sand deposit in the Holocene area which may not be a *donk*, namely, the ridge at Rijkerswoerd in the Betuwe. On the manuscript of the geological map it is indicated as drift-sand (Kootwijk Formation) on basin clay, and the only archaeological finds from Rijkerswoerd date to the Middle Ages.[46]
The relief in coversand areas is indicated by distinguishing between high and low lying surfaces, according to the Geomorphological Map of the Netherlands. The latter may have been (partially) covered with peat during the Roman Period. In the complicated relief between Wageningen and Veenendaal, where high east-west ridges in the low coversand area[47] are connected to a high coversand area[48] in which also (north-south) ridges occur, the latter are indicated by a separate symbol.
As is evident from fig. 9, a sizable part of the coversands with a high lying surface is covered by *es*-layers (and smaller areas by drift-sand). Defective visibility of ar-

41 Zagwijn/Van Staalduinen 1975, 25.
42 Schelling 1951.
43 Early-Holocene deposition is also probable, cf. STIBOKA 1976, 45.
44 Zagwijn/Van Staalduinen 1975, 20–2.
45 Verbraeck 1970, 40–6; Louwe Kooijmans 1974, 84–6.
46 Cf. Modderman 1949, 81, nr. 27 and note 3.
47 The so-called *Jong dekzand I*, which dates to before the Allerød-interstadial.
48 Which is *Jong dekzand II*, deposited after the interstadial.

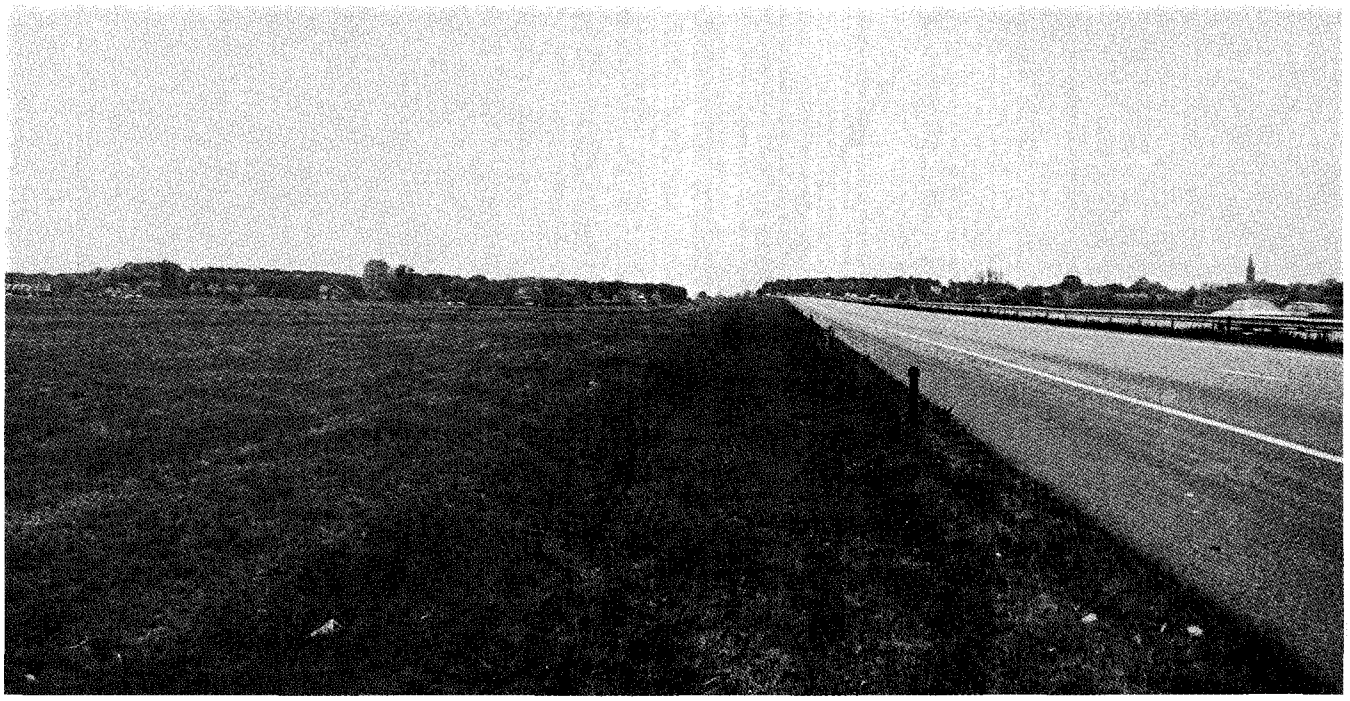


Fig. 10 View from the flood-basin at the Pleistocene sand ridge of Wijchen, at the point where it is crossed by the modern highway (see the Appendices, square 175/427). The tower on the right is the church of Hernen.

chaeological phenomena is therefore especially relevant to coversands. All high lying coversands were originally podzolic soils, usually with a fairly high humus-content. Those which are not covered by an *es*-layer are generally indicated as *humuspodzolgronden* (often Hn 21 or Hn 30) on the soil maps. Their suitability for (modern) agricultural purposes is often fairly high, but depends strongly on the groundwater level.

Loess

The eolian deposits, which principally consist of loam, sometimes containing find sands, also belong to the Twente Formation. Due to lack of detailed data about the large deposits on the ice-pushed ridge Nijmegen-Kleve, sandy loam or loamy sand (*lössleemhoudende zandgronden, Sandlöss*) have also been included as loess deposits.

In general, these soils are quite suitable for a variety of modern agricultural purposes because, with the exception of an area southeast of Groesbeek, they are not too wet. In the Netherlands, they are all in use as arable land, but most of the *Sandlöss* area in Germany is covered by the Reichswald.

Fluvio-periglacial and fluvioglacial deposits

These are sediments which were deposited by meltwater during an ice age. The fluvioglacial sediments on the map are glacial outwash (*sandr*) deposits and were formed during the Saale ice age. They belong to the Drente Formation.[49] Fluvio-periglacial deposits were formed under very cold circumstances and near absence of vegetation during the Weichsel ice age. They belong to the Twente Formation.

Both deposits generally consist of coarse or very coarse sand with gravel, although especially the periglacial sediments are sometimes less coarse and contain varying degrees of loam. They have been differentiated[50] because this is required by the legend of the Archaeological Map of the Netherlands[51] and not because the distinction is

49 Zagwijn/Van Staalduinen 1975, 14.

50 In one case probably wrongly, because the deposits around Groesbeek are presumably fluvio-periglacial (Crommelin/Maarleveld in Schelling 1949, 40).

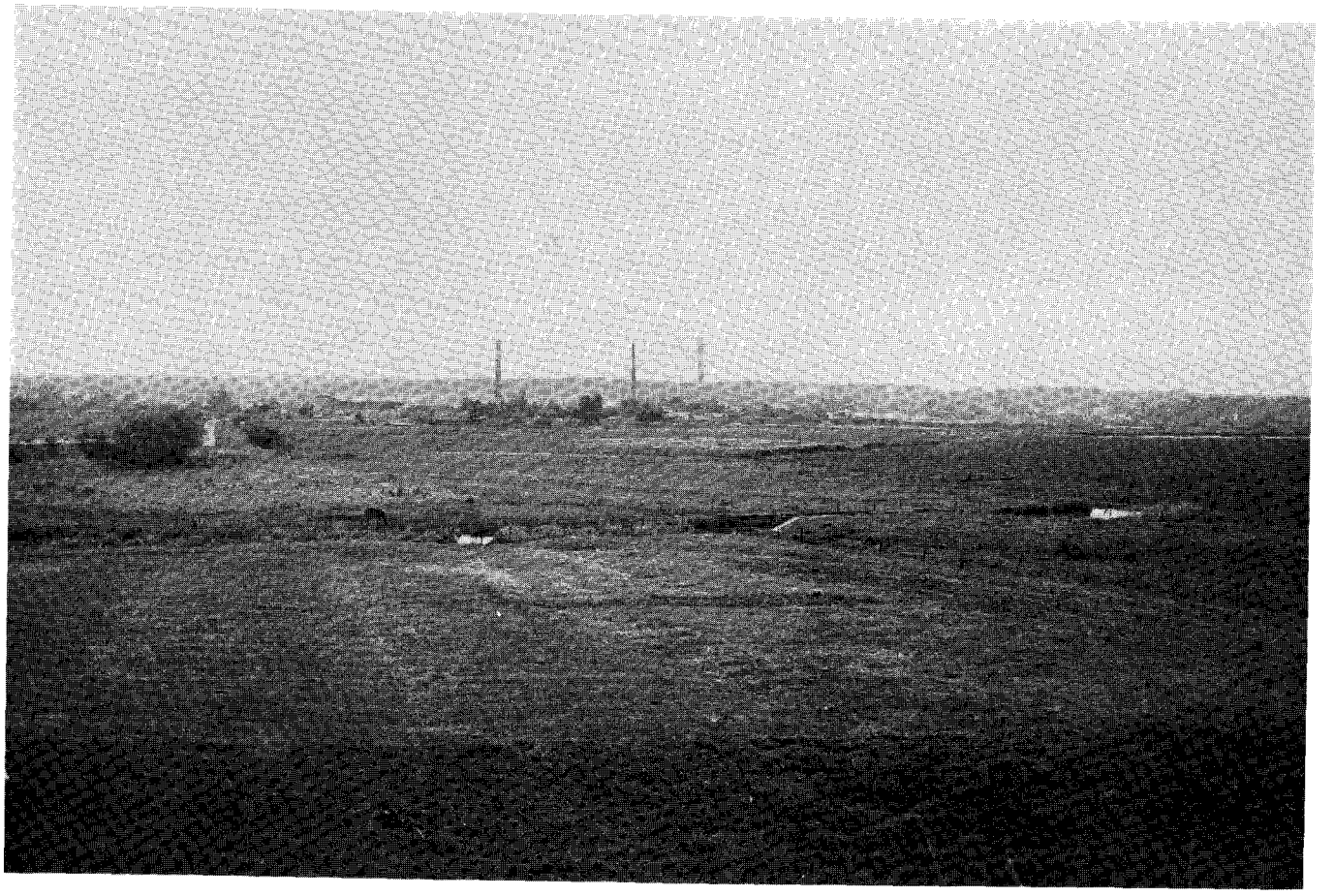


Fig. 11 View from the Overbetuwe at the ice-pushed ridge between Arnhem and Oosterbeek. The picture was taken in Meinerswijk. The chimneys belong to the brick-factory, in front of which lies the site of the Roman fort (126). The ditch is the fossil channel which surrounds it (see also fig. 18).

considered relevant to the occupation during the Roman Period.

Ice-pushed ridges

The gravelly coarse sand of the non-covered parts of the ice-pushed ridges (Drente Formation) is similar to the fluvio(-peri-)glacial sediments which generated from it, but the ridges are composed of extremely variable material over short distances. They are also shown as a separate unit because they are important geomorphological features that determine the relief of the landscape (figs. 11 and 12).

As far as they are not built over, the ice-pushed ridges, as well as the equally poor soils of the fluvio(-peri-)glacial deposits, are today almost completely covered by forest. On the soil maps, they are indicated as podzolic soils, mainly *loopodzolgronden* (gY30) and *haarpodzolgronden* (gHd30), the fluvio-periglacial deposits also as *vlakvaaggronden* (Zn23). Mainly because of the very deep lying grondwater levels, most of these soils are judged to be hardly or not at all suitable for modern agricultural purposes.

3.2.3 Regional Characteristics and Sources

The area north of the Rhine and IJssel

The region consists almost entirely of Pleistocene depos-

51 A.o. because the map also aims at showing the genesis of the landscape. The choice of colours indicates that the deposits are considered to be similar.

its. It is the southernmost part of the Veluwe, with in the west the southern limit of the Utrechtse Heuvelrug, which ends at Rhenen, and a small part of the Gelderse Vallei in between.
The basic data about the deposits were derived from (the manuscripts of) the Geological Map of the Netherlands 1:50,000, sheets 39 O, 40 W, and 40 O. The relief of the coversand area in the Gelderse Vallei (between Wageningen, Rhenen, and Veenendaal) was adapted from the preliminary sheet 39 O of the Geomorphological Map, which was also the source for the indicated dry valleys. Other geomorphological data were obtained from a geomorphological map of the Veluwe 1:100,000[52] and a detailed study of the geology and geomorphology around Arnhem.[53] After the publication of the sheets 40 W and O of the Geomorphological Map, which became available in 1981, a few additions and alterations were necessary.
The relief is determined by the high ice-pushed ridges of Arnhem, Wageningen, and Rhenen, the southern parts of which were eroded by the Rhine (see fig. 11). The slopes near Rhenen (the Grebbeberg), Wageningen (the Wageningse Berg), Doorwerth, and east of Velp are very steep. The glacial outwash deposits decline more gently towards Renkum. Two brook valleys, of the Heelsumse beek in the east and of the Renkumse or Kortenburgse beek in the west, cut deep into this landscape.
Apart from the peat, flood-basin clay, and brook deposits, a small area of Holocene channel zone deposits is indicated between Rhenen and Wageningen. This has been included – and therefore assumed to be of either pre-Roman or Roman age – for geological reasons only. The channel deposits are apparently covered with a considerable amount of basin-clay, and the bank deposits are described as an association of bank- and flood-basin deposits. This usually implies a pre-Roman age, but in the absence of any finds, the area may have been essentially a flood-basin during the Roman Period. It is also possible that the clay-cover has prevented sites from being discovered,[54] just as on the meander curve of the Waal in the Ooijpolder.[55]

The area between Rhine and Waal (Overbetuwe)
For the area between Rhine and Waal, the basis of the map was again derived by simplification from the manuscripts of the Geological Map, sheets 39 O and 40 W. Several adjustments were, however, necessary. The channel deposits of the Oude Rijn near Kesteren, which are between the present dike along the river and the Rijnbandijk, have been deleted because they are post-Roman. On the other hand, the deposits behind the dike near Dodewaard and Wely could be differentiated from those in the water-meadows and included as Roman channel deposits. This information was obtained from Havinga.[56] Bank deposits on basin-clay, which are often post-Roman in contrast to a complex of bank- and flood-basin deposits, have only been indicated where they can be shown to belong (also) to (pre-) Roman channel deposits. Examples are the bank deposits along the small channel west of Hemmen, which were also dated to the Roman Period by Havinga and on which several sites (62–68) occur.
Apart from Havinga's maps,[57] which are geogenetic and based on borings to a depth of 2.20 m, there are several soil maps which have mainly been used to derive data about ancient settlement soils and the courses of fossil channels. General information is provided by the first soil map of the Betuwe[58] and sheets 39 O and 40 W of the Soil Map of the Netherlands 1:50,000. For the purpose of tracing fossil channels, the very detailed soil maps (1:10,000) of the southern, northern, and eastern Overbetuwe,[59] made by the Soil Survey Institute (STIBOKA) as a basis for land reallotment schemes in the Overbetuwe, proved to be very useful. Only in the highly complicated situation around Flieren and Hulhuizen in the east, they were not sufficient to allow the reconstruction

52 Ten Houte de Lange (ed.) 1977, Appendix 1.
53 Teunissen 1961.
54 When the present article was about to be printed, this interpretation was confirmed by the discovery of an ancient settlement soil (site 542) under a 110 cm thick clay-cover. See Van de Westeringh 1983, addendum p. 13–4. The site is located just north of the Rhine-dike, which implies that the area between Nude and the dike, left blank on the Appendices, should also be included in the 'Pre-Roman and Roman channel deposits'.
55 See below, p. 51.
56 Havinga 1969, Appendix 1. They belong to the deposits of his phase 2 and are two of the few remains of the meander-belt of the Roman Waal.
57 I thank Dr Havinga for discussing his maps with me and for providing me with some additional and unpublished data on the region around Kesteren.
58 Egberts 1950.
59 Van der Schans/Steeghs 1957, Zegers/Zandbergen 1958, and Mulder/Salverda/Van den Hurk 1979.

of a coherent pattern.[60] The latter has, by the way, been the main objective in the Overbetuwe. It was not possible to represent all the details of fossil channels at a scale of 1:100,000.

The only Pleistocene (or early-Holocene) deposits in the Overbetuwe are the river dunes (*donken*) near Valburg and Bemmel. The dune at Rijkerswoerd has not been indicated on the assumption that it is either post-Roman drift-sand[61] – which could be checked, for example, by pollen analysis of the underlying basin-clay – or that it was covered by basin-clay during the Roman Period.

The area of the present Betuwe is dominated by pre-Roman stream-ridges, most of which are still clearly visible in the landscape, sometimes even to the inexperienced eye. The only (probable) remains of the meander-belt of the Roman Waal have already been mentioned. As for the Roman Rhine, there may be a small area left in the Roswaard, north of Doornenburg (c. square 197/434), but a more sizable part of the Roman channel zone deposits has survived between Huissen and Meinerswijk (from 193/438 to 188/442).

It is possible that not all of the indicated deposits have actually survived from the Roman Period onwards, because later channels may have eroded some parts of it. At least the southern levee and part of the meander-belt have definitely not been eroded, as witness the various settlements and one or two forts (see especially Appendix 3). From Meinerswijk to Driel or even Heteren, the meander-belt of the Roman Rhine must have followed exactly the same course as the post-Roman channel deposits marked on the map. From Heteren to the west, the post-Roman river has probably eroded more of the older deposits, including at least one (site 37) and possibly two (site 18b as well) frontier forts.

The area between Rhine and IJssel
(Liemers and Montferland)

The region consists of both Holocene and Pleistocene deposits. Most of the former are in the Liemers, while the latter constitute the Montferland and also the small area east of Doesburg with river dunes and fluvial deposits.

The basic data for this area were derived from sheet 40 O of the Geological Map of the Netherlands,[62] the manuscript of sheet 40 W, and from the geological *Übersichtskarte von Nordrhein-Westfalen 1:100.000*, sheet C4302.[63] In a few instances, such as the river dune around Duiven and the fossil channel under Schans, sheet 40 W of the Soil Map of the Netherlands[64] was used.

The back of the Montferland is formed by an ice-pushed ridge, of which only a small part is visible on the map. This ridge was originally connected to the ridge Nijmegen-Kleve until the Rhine cut through it in the late-Weichsel period.[65] Before that time, the river had followed a course east of the Montferland through the IJssel Valley. The area between Elten and Kleve is called the Gelderse Poort. Near Elten (the Elterberg) the slope is rather steep, but to the west the fluvio-periglacial deposits, which are largely under coversands, descend slowly until they are covered by basin-clay.

Apart from the river dunes, most of the Liemers is a flood-basin. Along the IJssel, definite (pre)Roman channel zone deposits have been identified,[66] but these could not be incorporated in a coherent way on the map. Moreover, as will be discussed below, there are reasons to doubt the basic assumptions involved in these identifications. Along the Rhine and the Pannerdensch Kanaal, a large part of the channel zone deposits of the Rhine in the Roman Period has survived. Because the Rhine has followed the same course until the Pannerdensch Kanaal was dug in 1706–1707,[67] it is not possible to separate post-Roman from earlier deposits for the area behind the dikes along the present and 'old' course of the Rhine. It is clear that, in contrast to the situation in the Overbetuwe from Huissen to Meinerswijk, the southern levee of the Roman Rhine has been completely eroded. Additional proof is provided by the finds of site 194 in the Loowaard: almost certainly a frontier fort situated on that levee.

The channel deposits between Groessen and Schans undoubtedly indicate the meander-belt of the Roman Rhine, even when they are not entirely its actual remains. The bank deposits north of this meander-belt,

60 For this area, the same is true for the provisional manuscript of the geological map which was used. It is possible that the deposits will be indicated somewhat differently in the final publication of the map.

61 See above, p. 37.

62 Van de Meene 1977.

63 Braun 1968.

64 STIBOKA 1975.

65 See e.g. Van de Meene 1977, 38, fig. 38 and 41–2; Van de Meene/Zagwijn 1978.

66 Harbers/Mulder 1981, fig. 4 and 5.

67 For a history of this enterprise, see Van de Ven 1976.

which extend as far east as Babberich, have been included because they were apparently inhabited, in one case (site 189) even from the Middle-Iron Age onwards. For the same reason, the crevasse deposits around Zevenaar, which are usually considered to be post-Roman,[68] are also indicated on the map. Site 190 appears to be situated in the flood-basin only because of the geological data that had to be used as a basis for the map. The soil map of this area indicates sandy clay (*zavel*) as far north as site 190, but these soils extend over such a wide area that they could not be used without chronologically dividing them for purely arbitrary reasons.[69]

The area between Waal and Meuse
(Land van Maas en Waal)

The data for the northern part of this area were derived mainly from the manuscripts of sheets 39 O and 40 W of the geological map, with a few additions and alterations. Near Druten, the channel deposits behind the dike are indicated because they clearly date to the Roman Period, as is testified in particular by the Roman barge from site 211.[70] The inadequacy of the geological criteria in relation to the bank deposits around Ewijk, which could be remedied by using Pons' soil map,[71] has already been mentioned. This same map was the source for the small river dunes (*donken*) in Druten and around Neerbosch and also for the fossil channel in the branch of the Meuse (from Lunen to Neersteind), part of which is known as the Wijchense Maasje. It could also be used to indicate the continuation of the Pleistocene sands at sheets 45 O and 46 W without difficulty. For the Holocene deposits between Overasselt and Niftrik, this proved to be more problematical. The reconstruction presented on the map is based on Pons' map and the soil map of sheet 45 O, the intrepretation of which was facilitated by an interpretative map by Pons[72] and some exploratory borings of the Netherlands Geological Survey.[73] The meander curve of the Meuse around Balgoij is post-Roman.[74]

The channel zone deposits between Niftrik and Batenburg, which are presumably remains of a Roman Meuse branch, have been indicated in approximately the same way as was done by Pons.[72] They are connected to the deposits south of the present river, near Dieden.

The Pleistocene fluvial deposits between Wijchen and Nijmegen have not been indicated. They are probably not covered entirely by Holocene basin-clay but, with the possible exception of the highest part around Heumen, the area can, for all practical purposes, be considered a flood-basin. The only known sites are Neolithic.[75]

The landscape of the Land van Maas en Waal is determined primarily by the Pleistocene river dunes, which extend from Hernen to Bergharen and by the adjacent, relatively narrow stream-ridge of the Wijchense Maasje. As already established by Pons,[76] this branch of the Meuse was still functioning during the Roman Period. It must, however, be considerably older than 'early Subatlantic'. This dating has been confirmed by late-Neolithic finds from the part of the fossil channel known as the Wijchense Meer (site 316). The deposits between Niftrik and Batenburg are the only remains of the other branch of the Meuse in the Land van Maas en Waal. The area where the fork was situated has been eroded by the post-Roman meander curve around Balgoij. Along the Waal, the only channel zone deposits apart from the deposits of the Roman Period in Druten are those of the pre-Roman stream-ridge around Ewijk.[77]

The area south of the Rhine and Waal
(Ooijpolder and Duffelt)

The most important sources for this area are the manuscript of sheet 40 W of the Geological Map of the Netherlands and a geological map of the Duffelt.[78] The latter is, however, only a geological interpretation of a soil map. Because all three stream-ridges which run from north-east to south-west are covered with a thick deposit of basin-clay, they are not indicated on this map. The most eastern ridge on German territory had to be constructed from a few incidental borings of the Netherlands Geological Survey. The German geological overview-map (1 : 100,000)[79] proved to be of little use for this

68 See Pons 1953, 24-8 and Harbers/Mulder 1981, 416 and fig. 9.
69 See above, p. 34.
70 Hulst/Lehmann 1974.
71 Pons 1966, Appendix 1. See above, p. 34.
72 Pons 1957, Appendix 7 and 1966, Appendix 1; Soil map 45O (STIBOKA 1976).
73 These are the data from the *boorraai Lunen*, made available by Drs A. Verbraeck and J. van der Staay.
74 Cf. Pons 1954, esp. figs. 1 and 2.
75 Peddemors 1978, sites 144–148.
76 Pons 1957, 42–4.
77 Pons 1957, 44; Van der Voort 1973.
78 Paas/Teunissen 1978, *Abb.* 2.
79 Braun 1968.

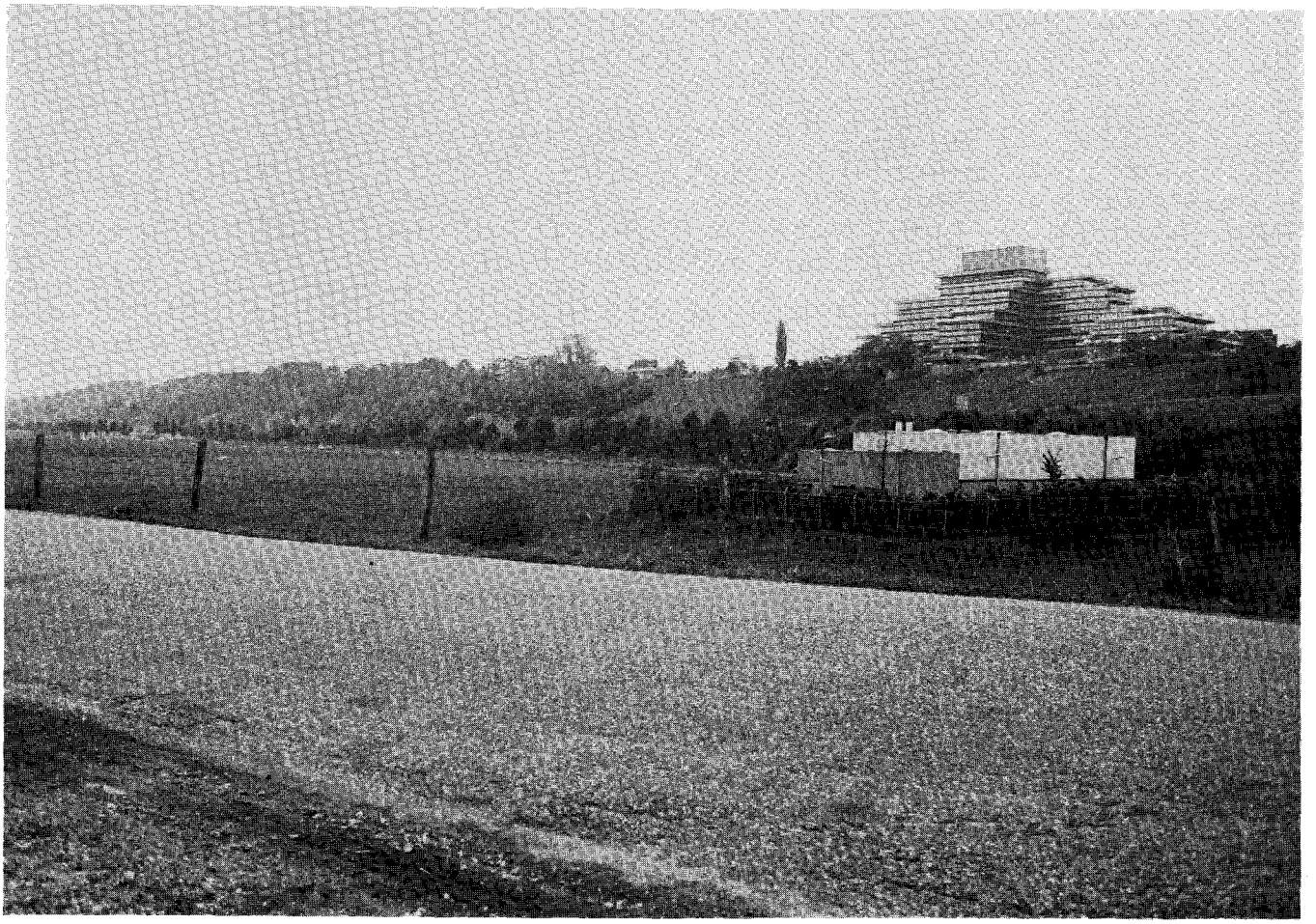


Fig. 12 View from the Ooijpolder at the steep slope of the ice-pushed ridge in Nijmegen. The large office-building on top of it is located at site 407, which was excavated before the new construction. The Kops plateau (site 417) is visible on the extreme left, and in between those sites lies the Hunerberg, where the successive fortresses (site 412) were built.

area.[80] The Pleistocene fluvial deposits still at the surface have been included because they can presumably not be considered a part of the flood-basin. As is evident from a historical map of 1667/8,[81] the high lying deposits north of Mehr were used as arable land, in the same way as the stream-ridges and the river dune of Zyfflich. The extent of the relatively high channel zone deposits from Kleve to Millingen was derived from the map by Paas/Teunissen and the German geological map. They are undoubtedly related to the main channel of the Roman Rhine.[82] It was not possible to distinguish clearly between these and the post-Roman deposits to the east. The area east of the fossil channel between Kleve and Düffelward has been interpreted as post-Roman, although site 451 indicates that at least a part of the earlier deposits may not have been eroded.

80 During a visit to the *Geologisches Landesamt Nordrhein-Westfalen* in Krefeld (BRD) in 1980 (after the retirement of Dr F.J. Braun) all access to more detailed data from this area was denied for security reasons.

81 Gorissen 1975, Appendix.

82 Cf. Braun 1968, 81–1; Teunissen 1975; Paas/Teunissen 1978.

Another problem are the deposits to the north of the Kermisdahl in Kleve. According to several authors,[83] this large and peat-filled fossil channel has to be pre-Roman. The only really decisive argument could be the presence *in situ* of a part of the Roman *limes*-road in Kellen, on the convex side of the meander curve (the *kronkelwaard*). Unfortunately this argument depends on a single statement by Gorissen which could not be verified.[84] In the near future, pollen analysis and C14 dating of samples from a boring in the fossil channel may provide additional evidence.[85] For the moment, it has to be assumed that the channel deposits in the *kronkelwaard* existed during the Roman Period, at least as far north as Kellen.

The area between Nijmegen, Kleve, and Gennep (Rijk van Nijmegen and Reichswald)

This area consists almost entirely of Pleistocene deposits. For the northern part around Nijmegen, the data were derived from the manuscript of sheet 40 W of the geological map and for the German area from the *Geologische Übersichtskarte 1 : 100.000*. The dry valleys have been reconstructed from the contour lines of the topographical maps, using geomorphological overview-maps as a guideline.[86]

For the Dutch part of sheet 46 W, several different sources had to be used. The western flank of the ice-pushed ridge has been adapted from the 'old' Geological Map of the Netherlands, using sheet 46 W of the soil map and the geological overview-map of the Netherlands 1:600,000[87] to obtain an acceptable correspondence with the northern part on sheet 40 W. The geological situation of the eastern flank was deduced from sheet 46 W of the soil map and maps of the municipality of Groesbeek.[88] For the lower area between the ice-pushed ridge and the Meuse, the Soil Map of Northern Limburg[89] was used as a basis, with some additions from the sheet 46 W of the soil map. The German part of this region was adapted from the *Geologische Übersichtskarte 1 : 100.000* and a separate map of the Pleistocene fluvial deposits.[90]

With the exception of some erosion and redeposition in post-Roman times, this region has remained basically the same since the Roman Period. The relief is, of course, determined by the ice-pushed ridge Nijmegen-Kleve, which has very steep slopes at both these places (fig. 12). The eolian sand and loess deposits are cross-cut by numerous dry valleys, some of which are very deep. The low lying fluvial deposits, which may originally have contained more peat-filled channels, are comparable to a Holocene flood-basin. The Niers is still a functioning river, but its channel has been artificially straightened both in the Netherlands and in Germany.

The area south of the Meuse

No recent geological maps are available for this part of Brabant and so various soil maps had to be used. For the Maaskant, the only area with Holocene deposits, the geogenetic soil map of Van Diepen[91] was used. Because of the inadequate depth of the borings, the stream-ridges could not be reconstructed in a completely satisfactory way. Small areas covered with basin-clay may not have been indicated as channel but as flood-basin deposits. Others, such as the area around Dieden, had to be indicated as 'post-Roman' for lack of specific data. For the terrain around Ravenstein, sheet 45 O of the soil map was used. The border between soil types Rn 95 and Rn 67C plus Rn 94c has been used to distinguish post-Roman channel zone deposits from (covered) flood-basin. The border of the coversand area near Berghem and Herpen is based on Van Diepen's map, further to the east on sheet 45 O of the soil map. The relief of the coversand area was derived from the Brabant sheet of the Archaeological Map of the Netherlands[92] and is thus an interpretation of the earliest (mid-19th century) topographical map.[93] The fluvial deposits in the 'Land van Cuijk' have been surveyed recently and the resulting map,[94] supplemented with a few other data,[95] provided adequate information.

Most of the low lying Pleistocene fluvial deposits are very wet and comparable to a flood-basin. The higher

83 Gorissen 1952; Hoppe 1970, 25–6, with further references.
84 Gorissen 1952, 49 and fig. 37.
85 The research is part of the cooperation between the RGD, the ROB and the Department for Biogeology of the Nijmegen University.
86 These are Pons 1957, fig. 3 and STIBOKA 1976, fig. 15.
87 Zagwijn/Van Staalduinen 1975.
88 Schelling 1949.
89 Schelling 1951, Appendix 1.
90 Schelling 1951, Appendix 8.
91 Van Diepen 1952, Appendix 2.
92 Stoepker 1977.
93 The Topographic and Military Map of the Kingdom of the Netherlands. See Stoepker 1977, 220–1 for details.
94 Kleinsman a.o. 1972, Appendix 1.
95 Schelling 1951, Appendix 9 and the old geological map.

fluvial deposits are often more suitable as arable land. The area is bordered to the east by the Graafsche Raam, which is joined by the Hooge Raam south of Escharen. The northern part of the extensive deposits of these brooks is visible on the map.

The rolling landscape of the coversands slopes down rapidly from c. 20 m in the south to about 7.50 m + NAP in the north, where it is covered by the basin-clay of the Maaskant area. Part of the channel zone deposits there may have been stream-ridges during the Roman Period, but those belonging to the fossil channel from Dieden over Macharen, which continues into the Ossermeer,[96] are probably related to a branch of the Meuse which was functioning during the Roman Period.[97]

3.3 ROMAN RIVER-COURSES

3.3.1 Reconstruction and Dating

From the foregoing discussion, it is clear that it should be possible, in principle, to reconstruct the courses of the rivers during the Roman Period in a fairly detailed way. 'River-courses' do not, of course, refer to the actual Roman Period channels but to the (eroded and surviving parts of) meander-belts. Such a reconstruction has not been attempted on the Appendices, because it would have involved various interpretations, some of which are open to doubt. The Archaeological Map of the Netherlands aims at presenting the surviving parts of a landscape during a specific period in an archaeologically meaningful way. From the preceding paragraphs it is clear that this approach already produced quite a number of – sometimes debatable – interpretations. To increase this situation even further by adding eroded areas, where the true extent of deposits is unknown, is not desirable. The map would require an exceedingly complex legend or otherwise lose its relevance regarding the distribution of archaeological phenomena. Moreover, the total picture would be unbalanced and outdated much sooner.

The reconstruction of Roman river-courses in the Netherlands has a long history, often stirring up fervent debate. This is primarily due to the human interference with rivers during the Roman Period, which is reported by several classical authors, but an important second reason is the long-standing interest of Dutch archaeologists in data from the geo-sciences. Research by geologists and soil scientists has provided and still provides[98] new information, both at theoretical and practical levels. It has changed viewpoints in the past and will do so in the future. It is not necessary to describe in detail the history of the entire discussion on Roman river-courses. Particular issues will be mentioned only as far as they are relevant to the present state of affairs in which there are still numerous questions which cannot be answered or on which there is no consensus.

Fig. 13 is based primarily on the various data which were discussed in the preceding paragraph and the archaeological information. It is a simplification of the geological background on the Appendices, with a reconstruction of the areas left blank there. This implies that large parts of the Roman channel zone of especially the Rhine and its branches, the Waal and IJssel, are reconstructions. Most of the channel zone of the Meuse and of pre-Roman channel zones (i.e. the stream-ridges during the Roman Period) could be derived directly from the Appendices, because the boundaries of the deposits have not been eroded.

A thorough discussion of the advantages and pitfalls of the different methods for dating channel zones is provided by Berendsen.[99] Of particular interest is his use of C14-data[100] to unravel the chronology of the different channel zones in the central Dutch river area. C14 dates have also been used as additional evidence in the construction of fig. 13, but the situation in the eastern river area is somewhat different from that further to the west, because convenient occurrences of peat are not always available or easy to locate. This is part of the reason why there are fewer C14 datings. Before the start of the present study, the Groningen C14 laboratory had dated samples submitted by various institutions, but mainly by the Netherlands Geological Survey (RGD).[101] Co-operation between the ROB and RGD[102] resulted in an additional series of datings.

96 Situated immediately to the west of the area covered by the map. For recent finds there, see Verwers/Beex 1978, 32 and *afb.* 47.

97 Van Diepen 1952, 114–6; Pons 1957, 43 and Appendix 7.

98 The most recent examples of this continuing interest in the river area are Van der Woude 1981, Harbers/Mulder 1981, and Berendsen 1982.

99 Berendsen 1982, chapter 7.3.

100 *Op. cit.*, chapter 4.3. and 7.2.

101 I thank Ing J. de Jong for his permission to use these datings.

102 Directly involved were A. Verbraeck, J. van der Staay, and J. de Jong from the RGD, and J.F. van Regteren Altena from the ROB.

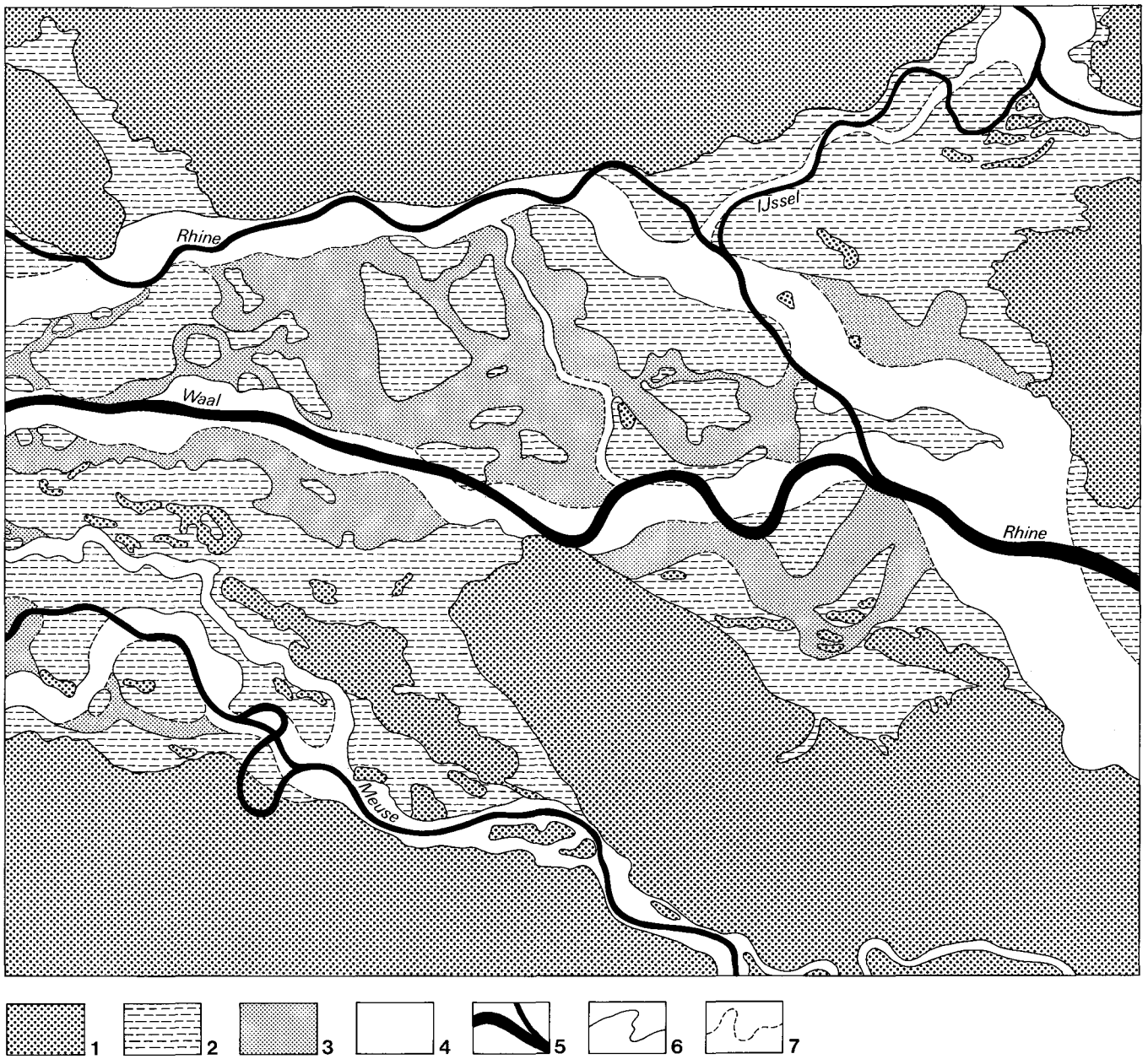


Fig. 13 A reconstruction of river-courses during the Roman Period: 1 Pleistocene deposits, 2 flood-basin deposits and peat, 3 pre-Roman channel zone deposits, 4 Roman channel zone deposits, 5 present-day river-channels, 6 boundaries of deposits, 7 reconstructed boundaries of deposits. Scale 1:250,000.

Although the available datings provide *termini post et ante quem* that can serve purposes comparable to those of Berendsen, they are used here mainly as additional evidence in the construction of an essentially static picture, namely, the situation during the Roman Period. In contrast to the more sophisticated approach of Berendsen,[103] the C14 datings from the eastern river area have been divided simply into those resulting in a *terminus*

103 Berendsen 1982, chapter 7.2.

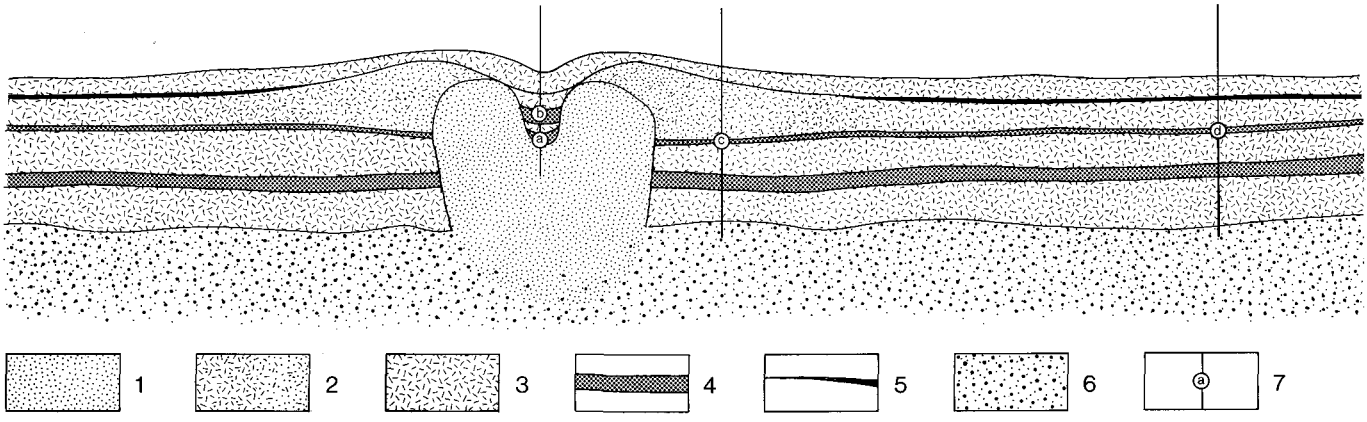


Fig. 14 Typical locations for C14 samples which are relevant to the dating of Holocene river deposits: 1–6 see fig. 7; 7 boring and location of sample. The significance of the samples a–d is discussed in the text.

post quem and those which yield a *terminus ante quem*, each category being composed of two sorts of datings that resulted from the different positions of the samples. These are indicated on fig. 14.

A boring in a fossil channel may result in two kinds of samples (fig. 14, a and b), the dating of which gives *termini ante quem* for the period in which the stream-ridge was being formed. Sample a, which is peat or other organic material situated directly on the underlying sandy clay, yields an end-phase dating in the sense as discussed by Berendsen. It dates the period in which the channel stopped functioning and the sedimentation by the river came to an end.

This is not true for sample b, which only results in a dating some time after the end-phase. Because the length of the intermediate period is unknown, this is a dating which could be of little use in Berendsen's analysis. In the eastern river area, where datings of type a are often impossible to obtain, a b-type dating may, however, serve our purposes quite well. As long as it turns out to be pre-Roman, the associated stream-ridge is certainly pre-Roman also. When the organic material proves to be of Roman age, we may be dealing with a pre-Roman fossil river (a stream-ridge) or with a river which became fossil during the Roman Period, but in any case its deposits existed during that period. Only a post-Roman date presents problems, because the associated deposits may be post-Roman also.

There are two other locations of borings indicated on fig. 14, namely, those for samples c and d. They are taken from the top of a peat layer, directly underneath the oldest sediments from the river, and therefore give a *terminus post quem*, at least in principle. In practice, it is necessary to examine carefully whether the deposition of sediments from the river caused erosion of the underlying peat. In such a situation, the significance of the dating as a *terminus post quem* may be low, because it will certainly be too old. According to Berendsen,[104] detailed profiles, with borings at 5 to 10 metre intervals, are necessary for a correct evaluation of this type of dating.

Although samples c and d are stratigraphically in the same position, there may be a difference in the results of the C14 analysis. The boring for sample c is close to the meander-belt of the river, where the peat has been covered quickly by fluvial deposits. The boring for sample d is further away, in the flood-basin, and peat may have continued to grow there for a longer period. The datings of type c and d are, of course, somewhat idealized here, but in general it can be said that an actual dating is more suitable as a *terminus post quem* for the beginning of a river when it is more of type c. Datings resembling type d are more a *terminus post quem* for the phase of greatest activity of the river.[105]

104 Berendsen 1982, 118.

105 Note that a sample from the fossil vegetation horizon higher in the same boring would provide a *terminus ante quem* for this phase. See Berendsen 1982, 116 *(bloeifase-* or *flank-dateringen)*.

Fig. 15 List of C14 datings relevant to the dating of stream-ridges in the eastern river area

	Name	GrN	Age in C14 Years BP	Type	Depth in cm below surface	Substance	Co-ordinates	References
1	Herveld	7269	4755 ± 60	c	323–330	peaty clay	179.18/434.13	De Jong/Zagwijn 1976
2	Valburg-De Leygraaf	8653	7635 ± 50	d	469–474	peat	181.50/435.03	De Jong 1979
3	Oude Zeeg	8654	3150 ± 55	a	268–278	humic clay	182.35/434.69	De Jong 1979; De Jong 1983b
4	Valburg-De Hoogebrug	8652	2645 ± 30	a	207–212	humic clay-gyttja with shell-fragments	182.50/434.43	De Jong 1979
5	Slijk-Ewijk Veens	8655	3040 ± 35	a	237–242	humic clay-gyttja with shell-fragments	182.53/433.60	De Jong 1979
6	Slijk-Ewijk	7268	3665 ± 55	d	210–220	very humic clay	183.20/432.78	De Jong/Zagwijn 1976
7	Oosterhout 1	10598	3225 ± 50	b	400–402	gyttja, clayey	185.14/431.61	De Jong 1983a
	Oosterhout 2	10599	3745 ± 45	b	420–422	gyttja, clayey		
8	Ressen 1	10601	4920 ± 100	b	156–161	peat and wood	188.55/434.44	De Jong 1983a
	Ressen 2	10602	2370 ± 100	a	177–182	clay-gyttja		
9	Baal	10600	3110 ± 190	a	181–188	clay-gyttja with shell-fragments	191.74/434.26	De Jong 1983a
10	Homoet	10603	3575 ± 35	b	140–145	clay-gyttja	183.28/437.90	De Jong 1983a
11	Driel	10604	460 ± 90	b	75– 77	peat	185.58/441.23	De Jong 1983a
12	Rijn bij Elden	7266	1585 ± 50	d	125–130	humic clay	186.93/442.30	De Jong/Zagwijn 1976
13	Meinerswijk I/1	10605	2675 ± 30	c	284–287	peat with clay and gyttja	188.21/442.25	De Jong 1983a
	Meinerswijk I/2[106]	10606	7300 ± 40		343–345	peat, clayey		
14	Meinerswijk II/1	10607	1350 ± 30	b	279–282	clay-gyttja	188.43/442.66	De Jong 1983a
	Meinerswijk II/2	10608	1500 ± 35	b	325–328	clay-gyttja		
15	Lathum II	7525	1720 ± 25	d	102–110	peat	199.10/443.50	Teunissen 1980a, 328
16	Lathum I	5491	2000 ± 65	c		peat	200.41/443.96	Lanting/Mook 1977; Poelman 1981a
17	Altforst III	6999	3765 ± 55	a	270–280	clay-gyttja	166.28/429.61	De Jong/Zagwijn 1974
18	Ubbergen	10597	1800 ± 30	b	221–224	peat, partly clayey	190.99/428.98	De Jong 1983a
19	Leuth 1	10595	2030 ± 35	b	146–149	peat, partly clayey	197.16/427.37	De Jong 1983a
	Leuth 2	10596	2170 ± 40	b	165–168	gyttja		
20	Bosse Wässerung	10276	3680 ± 60	b	195–210	humic clay to clayey peat	196.91/426.07	Teunissen 1982, 27, fig. 11

The available C14 datings from the area covered by this study and relevant to the present subject are listed in fig. 15. Their location is indicated on fig. 16. The list contains several additional data, but these are, of course, not at all adequate for a complete and correct evaluation. The type of dating (a–d) is indicated to give an approximate idea of its significance, but there are, in each case, other circumstances which could be important. These are, however, only discussed in those instances where they are of special relevance or when such a discussion is not available in other reports or publications. The results of C14 datings are not always reliable, as is evident from GrN-10601 (Ressen 1) which is far too old, presumably because the peat contained washed-in pieces of very old wood. In this case, a mistake cannot be made because GrN-10602 already indicates that something is wrong, but such a convenient aid is not always present.[107] It is fortunate that in several instances there are already pollen diagrams which may be used as a check on the C14 datings.

106 See discussion and fig. 18 on p. 54-5.

107 A high calcium content, which occurs in some samples, may also cause a dating to turn out too old. In a recent evaluation of the datings GrN–10595 to 10608, De Jong (1983a) concluded that especially datings of gyttja, and notably those containing calcium, tend to be too old. Both datings in Oosterhout (fig. 15,7) and the dating in Homoet (fig. 15,10) are far too old (see also note 109). The same is suspected for Baal (fig. 15,9), where no pollen analysis is available. It could also be demonstrated for Oude Zeeg (fig. 15,3), where pollen analysis has shown that the gyttja should be dated to c. 2000 BP (De Jong 1983b).

The activities of the Department for Biogeology of the Nijmegen University over a longer period have opened the possibility of making fairly reliable chronological interpretations of characteristic fluctuations in pollen diagrams in the eastern river area, covering the late-Glacial and Holocene period.[108] Sometimes, these can also be used to extend the C14 evidence, for example in the case of datings of type b, where the base of the channel-fill deposits does not contain organic material, but can still be studied by means of pollen analysis. This is especially important in cases where the chronological interpretation of the pollen diagram and the C14 dating are in agreement and both indicate a post-Roman date of sample b. In those cases, the results of the pollen analysis may indicate whether the channel started to fill up before, during, or after the Roman Period.

3.3.2 Pre-Roman Stream-Ridges

Although fig. 13 is intended to be a static picture of the situation during the Roman Period, the combined geological, archaeological, C14, and palynological data allow a brief discussion of the chronology of river-courses during earlier periods. The oldest surviving parts of Rhine branches were suitable for habitation during the late-Neolithic, possibly already during the middle-Neolithic, either as natural levees of functioning rivers or as stream-ridges of fossil rivers.

The easternmost branch of the oldest Rhine system is the Bosse Wässerung stream-ridge (Keeken-Niel) in the Duffelt (fig. 16, no. 20). Further to the west, the ridge in the Overbetuwe from Lijnden over Homoet to Heteren probably belongs to the same system. There are archaeological data to support this conclusion, in addition to the C14 dating (*terminus ante quem*) in Homoet (fig. 16, no. 10). On the other hand, the archaeological support for the C14 dating is surprising, because recent pollen analysis has shown that the entire channel-fill deposit has to be post-Roman.[109]

This curious phenomenon is as yet inexplicable; it might be due to rejuvenation of an older ridge, anthropogenetic influence (keeping a channel open), or something else. In any case, the archaeological data show the presence of an old stream-ridge. The same is true for the ridge around Ewijk in the Land van Maas en Waal, as well as for the stream-ridges and crevasse-ridges around Wely-Dodewaard and minimal remains of the later eroded (rejuvenated) Rhine-branch Herveld-Zetten-Randwijk in the Overbetuwe. For all of these, very detailed soil surveys provide indispensable information.[110]

As far as the Meuse is concerned, the branch of the Wijchense Maasje also existed during the same period: the evidence is provided by C14 datings (fig. 16, no. 17[111])

108 For a concise overview, with a.o. a curve of the changing frequencies of tree pollen v. non-tree pollen and the range of several diagrams, see Teunissen 1982, 27, fig. 11.

109 Stratigraphical observations in Heteren have shown a settlement soil with neolithic sherds covered by later deposits with Iron Age and Roman occupation traces (unpublished; pers. comm. Drs R.S. Hulst). The analysis of the boring in Homoet showed that *secale*-pollen is present from the bottom to the top of the channel-fill deposit (unpublished; pers. comm. Dr D. Teunissen).

110 Havinga 1969 and Van der Voort 1973.

111 Datings further to the west in De Jong/Zagwijn 1974.

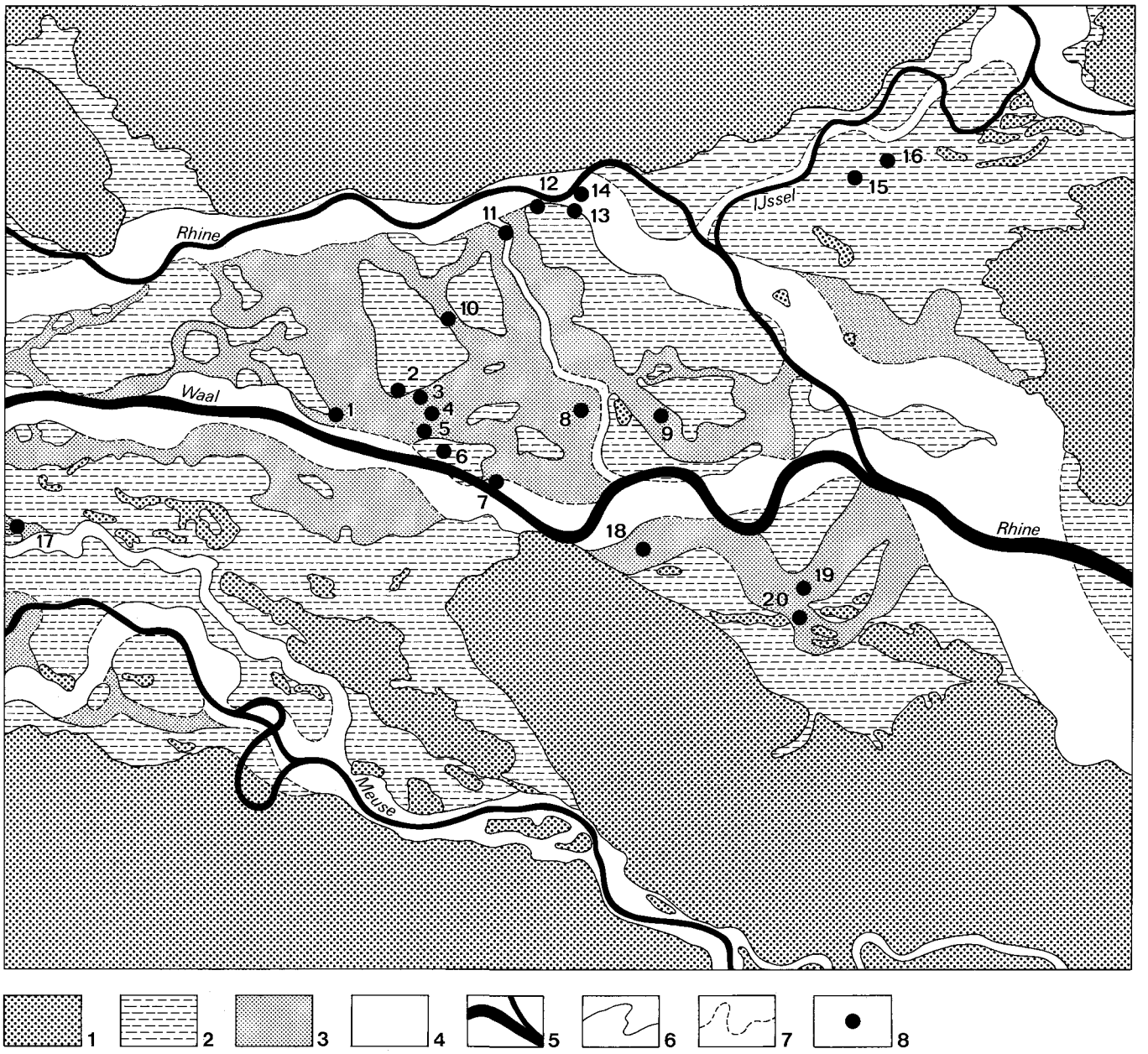


Fig. 16 The location of borings for C14 samples in relation to the reconstructed landscape during the Roman Period: 1–7 see fig. 13; 8 location of boring. Scale 1:250,000.

and finds of late-Neolithic (Vlaardingen) pottery at site 316.

Somewhat younger than these oldest stream-ridges is the rejuvenated large stream-ridge through the Overbetuwe (Herveld-Zetten-Randwijk), with many traces of Bronze Age occupation. It seems to be a continuation of the ridge from Haalderen to Elst (fig. 16, no. 9). It is possible that the stream-ridge from Millingen to Leuth

belongs to the same period, but it might just as well be younger. In that case, it could belong to the stream-ridge from Boerenhoek (c. 195/345) over Bredelaar and Elst to Driel, where the oldest traces of habitation date to the Early-Iron Age. The channel on this ridge can be assumed to have remained open during the entire Roman Period, as is indicated by the C14- and pollen-analysis which are in agreement here.

The stream-ridge from Ooij to Nijmegen, which presumably cut off the ridge from Millingen to Leuth, could be the immediate predecessor of the Roman Waal. The C14 date (fig. 16, no. 18) indicates that the channel started to fill up before the 2nd century AD. The period in which the river was active here must have been rather short, possibly only a few centuries between the end of the activity of the Millingen-Leuth branch and the beginning of the Roman Waal which followed approximately the same course as the present-day river.

This may be an explanation for the total lack of Roman finds from this stream-ridge. It is covered by thick deposits of basin-clay which may have prevented the discovery of finds. Such a thick cover of basin-clay over a stream-ridge is rather unusual. It implies that the ridge is not very well developed, which is understandable if it were formed over a relatively short period. It is, of course, also possible that the natural levees were not very suitable for habitation. Pons indicates extremely clayey channel zone deposits in this area and the only ancient settlement soils discovered so far are medieval settlements.[112]

In principle, it is possible to reconstruct the pre-Roman stream-systems in more detail. This is not attempted here, because the number of available C14 dates is not large enough, and also because important geological and palynological information will be published in the near future.[113] There is, however, one recently published study on the geological development of the eastern river area during the Holocene which deals with the same issue.[114] The authors have introduced a chronostratigraphical classification of various deposits belonging to the Betuwe Formation. They distinguish the deposits of Ressen and Gendt, which are synchronized with the deposits of Gorkum and Tiel in the perimarine area[115] and the marine deposits of Calais and Duinkerke. The criteria for the division of the Ressen and Gendt deposits are varied. They range from lithological properties (grain-size, calcium content) to geomorphological and archaeological aspects. On the basis of this division, the authors arrive at a reconstruction of the chronology of the Holocene fluvial systems in the eastern river area.

Although their study contains many stimulating ideas, further use of its conclusions has to await detailed scrutiny from the point of view of the various geo-sciences. This cannot be done here,[116] but a few general criticisms indicate that the interpretations of Harbers and Mulder should be treated with caution.

At a practical level, the fact that their data depend on soil surveys implies that most of the borings are only 1.20 m deep, which is not always adequate for the purposes of their research. An overall depth of at least 2 m seems to be an acceptable minimum.[117] At a theoretical level, at least two of the authors' basic assumptions are questionable. First, the synchronization of the deposits with those in the perimarine and coastal areas is not a straightforward but a highly complex matter. As Berendsen demonstrated,[118] the stratigraphy of the Holocene deposits in the river area has only a local significance. Second, it is a dubious procedure to distinguish Holocene fluvial deposits lithologically. A similar or identical lithological composition of particular channel zone deposits does not necessarily mean that these deposits date from the same period. To the contrary, the presence of chronologically significant and recognizable lithological differences in channel deposits has been explicitly denied by other investigations.[119] Therefore, the chronostratigraphical division in Ressen and Gendt deposits can only be considered valid where it is based on chronostatigraphical criteria, such as archaeological, pollenanalytical, and C14 datings, or on (related) lithological differences, such as a covering layer of basin-clay

112 See Pons 1951 (soil map) and Pons/Modderman 1951.

113 This includes sheets 39O and 40W of the geological map (Drs A. Verbraeck), the geological development of part of the Betuwe (Dr A. J. Havinga) and several pollen diagrams by Dr D. Teunissen.

114 Harbers/Mulder 1981.

115 See e.g. Verbraeck 1970; Zagwijn/Van Staalduinen 1975, 47–8.

116 This discussion has only just started, with a first review by Van der Schrier (1982).

117 See e.g. Havinga 1969, 6, Van der Voort 1973, 5, and Berendsen 1982, 17.

118 Berendsen 1982, chapter 4.

119 See e.g. Verbraeck 1970, 79 and Berendsen 1982, 68 (sub 4).

over channel deposits. Nevertheless, Harbers and Mulder's distinctions do also follow older distinctions made by authoritative soil-scientists[120] and cannot be considered entirely irrelevant.

3.3.3 *The Rivers*

The Rhine (*Rhenus*)

A discussion of the course of the Rhine during the Roman Period is necessarily a mixture of geological, archaeological, and also historical arguments. In the classical literature, there are four well-known references to interference with the river system. Suetonius (*Claudius* I, 2) mentions the construction of canals as branches of the Rhine (to the north) by the army under Drusus in 12 BC. According to Tacitus (*Ann.* II, 8), there was only one canal, which was used again by the army of Germanicus (AD 15–16) to sail north. The same writer (*Ann.* XIII, 53) also refers to a dam (*agger*) which was constructed by Drusus' troops 'to control the Rhine' and which was completed in AD 55 by the army under Paulinus Pompeius. He describes what is presumably the same construction, but this time called *moles*, in another work (*Hist.* V, 19), because it was destroyed at the orders of Civilis (September AD 70) with the consequence that 'by demolishing the barriers that checked it, he let the Rhine pour in full flow into Gaul along an unencumbered channel. Thus the Rhine was virtually drawn off, and the shallow channel that was left between the island and Germany made the lands seem uninterrupted'.[121]

The first two statements, and the way in which they are probably related to the last two, will be dealt with below in a discussion of the IJssel. These last two references are important, because they mention a *moles* or *agger* which can only have had one function, namely to divert more water to the Rhine which would otherwise have flown into the Waal. As has long been recognized, the *moles/agger* must therefore have been situated at the fork of Rhine and Waal. The discovery, in 1938, of a tombstone of the soldier M(arcus) Mallius (fig. 17) at site 182, has led to a more or less precise localization of the *moles*. The inscription mentions that the deceased *Carvio ad molem sepultus est*, lies buried in Carvium, at the mole.

The tombstone itself was found between the later eroded and washed-down remains of the fort Carvium, at a depth of some 10 to 12 m. It has been dated to before AD 50, although other finds from this site indicate occupation from AD 70 onwards. It presumably belonged to the cemetery of an as yet unknown early camp (the hypothetical site 183), which may have existed from Drusus'



Fig. 17 The famous tombstone from Herwen-De Bijland (site 182); its findspot indicates the approximate location of the *moles*. The text should be read (cf. *ER* III, 231–2 and Jongkees 1959) as follows: M(arcus) MALLIVS / M(arci) F(ilius) GALER (ia tribu) GENVA / MILE(s) LEG(ionis) I (centuriae) RVSONIS / ANNO(rum) XXXV STIP(endiorum) XVI / CARVIO AD MOLEM / SEPVLTVS EST EX TEST(amento) / HEREDES DVO F(aciendum) C(uraverunt): 'Marcus Mallius, son of Marcus, of the tribus Galeria, from Genua, soldier of the 1st legion, of the centuria of Ruso, aged 35, of 16 years' service, lies buried in Carvium at the mole. According to his will, his two heirs had this set up.' (Photo Gemeentemuseum Arnhem).

120 See e.g. Pons 1957, 48–9 for an example.

121 Translation by C.H. Moore, Loeb Classical Library no. 249, 1979.

times onwards until AD 70.[122] This camp may have been located close to the actual fork, but it is very unlikely that the cemetery and certainly not the permanent *limes*-fort of the Middle-Roman Period were also located there. A higher, drier, and more stable location somewhat downstream ought to have been preferred, and the fork of Rhine and Waal can therefore be situated somewhere in the neighbourhood of the present-day village of Tolkamer or even a little further south.

When the geological situation is taken into account, it is also evident that the *moles* is not likely to have been situated at site 182. The Roman Waal must have passed at almost the same spot as the present Bijlandsch Kanaal, through the two-kilometres-wide space between the pre-Roman channel deposits south of Millingen aan de Rijn, and the flood-basin deposits north of Pannerden. The Roman Rhine can only have passed to the east of these flood-basin deposits and west of the Pleistocene coversands, also a space of around two kilometres. Site 182 is located almost in the middle, but so far north that a fork there would be almost T-shaped and so unnatural that it would have been difficult to maintain.

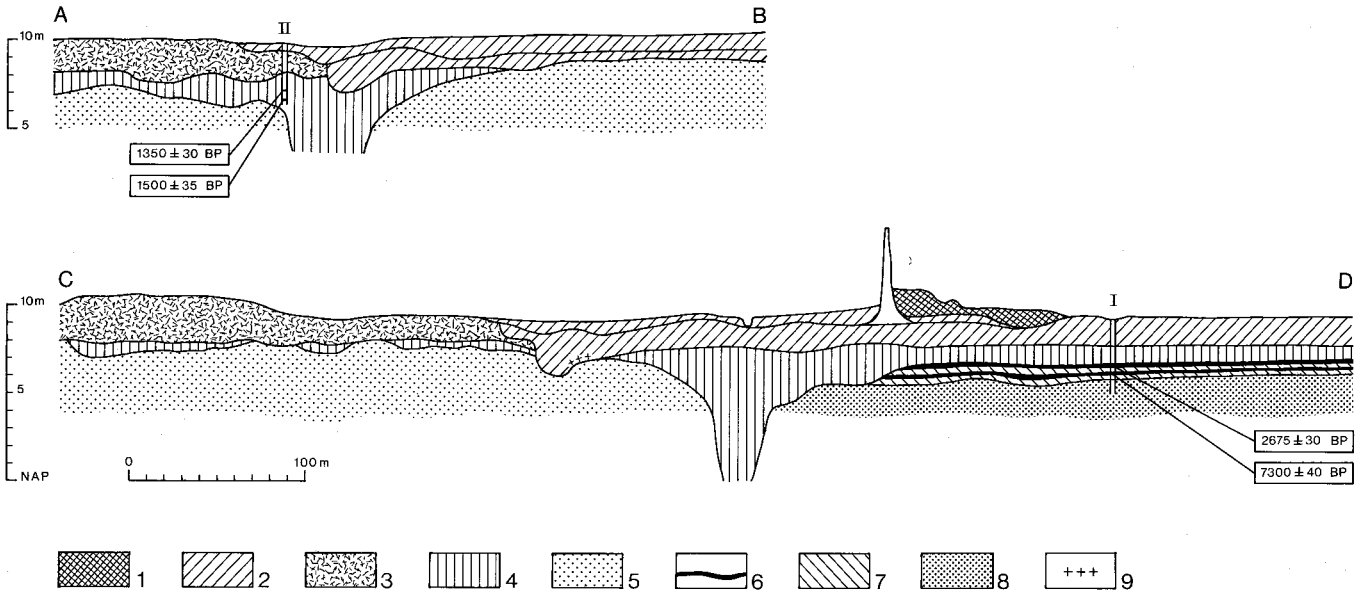
As far as the Roman construction at the fork is concerned, the fact that two different words were used by Tacitus may be completely accidental and insignificant. Nevertheless, a *moles* often is a structure built out into the water and should be translated as groyne or jetty (*strekdam, schephoofd, krib*), while an *agger* is built on land and normally translatable as dam or dike.[123] This has often led to the assumption that Drusus built a jetty as well as a dike, although this is not very likely. It is not improbable that the technical means available in 12 BC, although in our area they certainly did not include stones for construction purposes, allowed the construction of a groyne which actually functioned in the desired way. The first moles and jetties from the Velsen harbour[124] show that it may not have been impossible. The presence of a dike is, however, harder to believe, especially because it is supposed to have been built to keep the water in the Rhine. The natural levees were quite sufficient for that during the summer, and in the winter and spring there was enough water anyway. Besides, the construction of many kilometres of dikes presupposes too much, if not about technical capabilities and available manpower then at least about availability of materials and knowledge of the area and the fluvial system.

An alternative interpretation of the construction is to view it as a dam or series of dams at the fork across the high-water bed of the Waal, leaving open only the entire, or part of its, main channel. During times of high water, such a construction would be described as a *moles*, while in the summer it would be an *agger*. If the construction is interpreted this way, its completion under Paulinus Pompeius in AD 55 also makes sense. Although Tacitus states that the work was undertaken to give the troops something to do, it has to be realized that the situation in AD 55 was completely different from that in 12 BC. Drusus ordered the construction of the dam because it was necessary for his offensive campaign. Paulinus, however, faced a completely different situation. In AD 55, the *limes* had been fixed at the Rhine, and work on the necessary supporting infrastructure had already begun. A vital element therein was the military *limes*-road along the Rhine, which had to cross the Waal. The *agger* built under Paulinus should therefore be seen as part of this *limes*-road, because the dam, interrupted by a bridge or ferry, would provide a safe Waal-crossing during all seasons, as well as a continuation of the diversion of extra water to the Rhine. When seen in this way, Paulinus' initiative becomes a very useful, and from a military point of view even essential, enterprise. For the same reason, and although this has not been recorded in the surviving sources, it can also be taken for granted that the damage done by Civilis was repaired again very soon after AD 70. The fork of Rhine and Waal, and Drusus' dam, can thus be situated somewhere in the neighbourhood of Tolkamer, and it is not likely to be a coincidence that this is approximately midway between the probable location of the fort Harenatium (site 450) at the undivided Rhine, and the fort Carvium (site 182) at the Lower Rhine. The course of the latter through the area of the Liemers is partly the same as that of the so-called 'Oude Rijn' between Herwen and Babberich, but from approximately square 198/436 onwards it was situated more to the north. The remains of the probable fort at site 194, which were also washed down in post-Roman times, indicate its original location which can only have been on the southern levee of the Lower Rhine. From there, the

122 Cf. Bogaers in Bogaers/Rüger 1974, 90.

123 See e.g. Sprey 1953, 99–100, but especially Von Petrikovits 1983, 216–7 and 220–2.

124 Morel/De Weerd 1980.



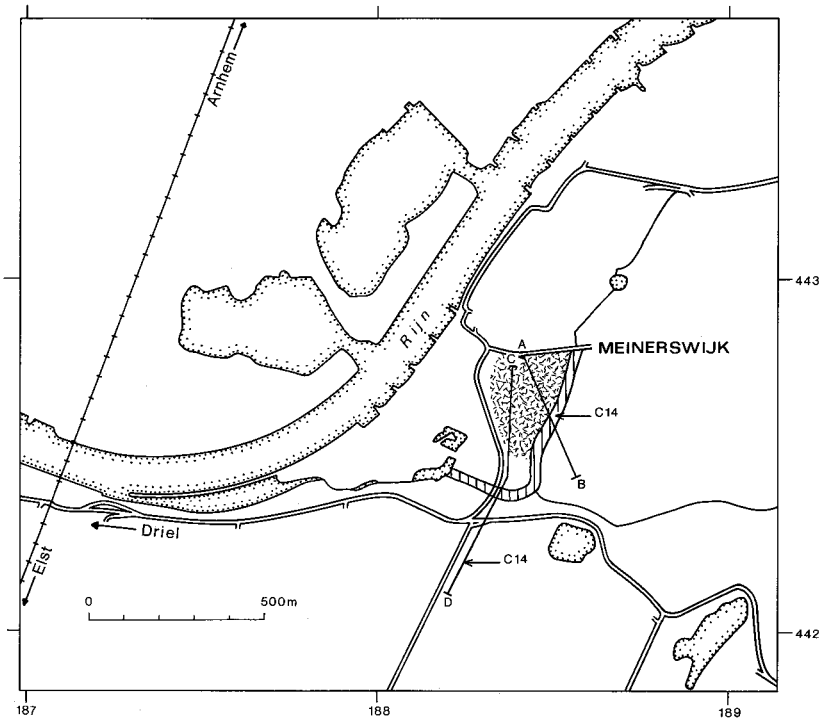


Fig. 18 The site of the Roman fort in Meinerswijk: situation of the ancient settlement soil and sections (borings by the RGD, Drs A. Verbraeck and J. van der Staay). The borings used for C14-anaysis are indicated separately: 1 artificially raised soil, 2 channel-fill, bank, and flood-basin deposits (clay and sandy clay), 3 ancient settlement soil, 4 channel zone - flood-basin deposits (clay, sandy clay, and fine sand), 5 channel deposits (sand and gravel), 6 peat, 7 flood-basin deposits (clay), 8 late-Pleistocene or early-Holocene fluvial deposits (coase sand and gravel), 9 washed-down sherds and brick.

river continued its course in a northwestern direction towards Meinerswijk, sites 130–135 (see Appendix 3) also being located on its southern levee.

As indicated on figs. 15 and 16, there are several C14 datings available in Meinerswijk and Elden. The location and stratigraphical position of nos. 13 and 14 is presented in more detail on fig. 18, which also indicates the extent of the southern part of site 126.[125] Site 126 is located north of a fossil channel. It is evident from the profiles that it was situated at the high, convex bank on the inside of a meander-curve. This curve cannot have been part of the Rhine channel during the Roman Period, because that would imply that the fort at site 126 was located north of the river which, apart from being totally unprecedented, would be very disadvantageous from a military point of view. The obvious interpretation of the geological situation is, therefore, that the fort was located on the inside of a pre-Roman meander-curve, which had been cut off and provided a high and protected site very close to the Roman channel.

The meander-curve did not silt up for a long time after it was cut off, and may have been used as a natural defence and/or harbour. The site itself, which was repeatedly flooded during the earlier part of its occupation, was artificially raised[126] and eventually extended over part of the fossil channel (see fig. 18, profile AB). The GrN-dating 10607(1350 ± 30 BP) provides a *terminus post quem* for this process which must, therefore, have taken place during but more probably after the 7th century AD.[127] It is meaningless as a *terminus ante quem* for the Rhine channel involved.

The datings in profile CD are very important, because they give a clear picture of the chronology of river deposits in this area. The material for GrN-10606 was taken from the base of the lowest organic deposit, that for GrN-10605 from the top of the highest, directly underneath the first sediments from the river. Both datings could be too old because of a high calcium-content, but the outcome of dating 10605, 2675 ± 30 BP, suggests that this effect may not have been very important. It indicates that the Lower Rhine started depositing sediments here after approximately the second half of the 8th century BC.[128] In archaeological terms, this branch of the Rhine can therefore be said to have developed during the Early-Iron Age, a fact that agrees well with the absence of any traces of occupation on the natural levees dating to before the Late-Iron Age. It is acceptable that it took several centuries before the natural levees were permanently habitable.

The distance between the samples for the datings GrN-10606 and 10605 is only 56 cm and the trajectory in the boring consists largely of organic material with some basin clay in between. The difference in dating is, however, in the order of magnitude of five millennia. There is no indication that anything should be seriously wrong with GrN-10606 (7300 ± 40 BP), and the conclusion must therefore be that for a very long period before the 8th century BC, no river was present in this area. The thin deposit of basin clay between the two samples can presumably be regarded as a deposit of the chronologically preceding branch of the Rhine between Elst and Driel, which was already inhabited during the Early-Iron Age.

From what has been said so far, it is perfectly clear that the reconstructions of Harbers and Mulder for this area[129] cannot possibly be correct. From Meinerswijk to Driel, the Lower Rhine must have followed the same course from its origin to the present day. A radiocarbon dating in the flood basin at Elderveld (fig. 16, 12) provided a *terminus post quem* for the channel zone deposits at that spot of 1585 ± 50 BP. This is considerably later than the comparable dating in Meinerswijk, which is probably due to the fact that the actual channel was for a long time located close to the ice-pushed ridge in Oos-

125 The area to the north is covered by a brick-factory and cannot be investigated.

126 See Willems 1980a for a provisional report on the results of the excavation.

127 This matter will be returned to in chapter 9, devoted to the excavation at site 126. The datings, as well as small sherds from the borings, confirm a continued occupation of the site into the Early-Middle Ages. They also imply that profile AB is somewhat misleading: there should be a stratigraphical differentiation between Roman and post-Roman layers in and under the settlement soil, which cannot be detected by geological borings.

128 This is, however, only the dating in conventional C14 years. The real date may be later, because of the calcium content, as well as older, because calibration-curves for this period indicate real ages which are a century older, or even more (see e.g. Mook 1978, fig. 2).

129 Harbers/Mulder 1981, figs. 4, 5, and 6. See also below, pp. 57-8.

terbeek, in the northern part of the Rosande Polder.[130] This is further supported by the location of site 126 and the (eroded) site 23.

In conclusion, it can be said that the entire branch of the Lower Rhine between approximately the Loowaard (site 194) and Driel was formed and gradually became more important during the Iron Age, and was therefore chosen as a frontier in the Roman Period. Somewhere in the area of the Loowaard or Pannerdensche Waard it branched off the previous main course, which it rejoined near Driel, forced by the barrier of the ice-pushed ridge. This does not imply, however, that this previous course was no longer active. The pollen analysis of the channel-fill deposits,[131] at one point of the trajectory confirmed by a C14 date (fig. 16, 11), indicates that the fossil channel between Elst and Driel started to silt up only after the Roman Period, in the 6th or 7th centuries AD. The course of this branch to the southeast of Elst is, however, unknown. The course indicated on fig. 13 is nothing but a guess, for lack of alternatives: at the present state of the evidence it is the only available continuous fossil channel which is not ruled out by pollenanalytical or C14 data, such as those in Ressen and Baal (fig. 16, 8 and 9). It is, however, altogether possible that one or both of these datings will prove to be too old (as did nos. 7 and 10) or that one of the other stream-ridges in the area between Angeren, Bemmel, and Gendt was still an active stream in the Roman Period.

In view of the importance of Elst, the possibility that one or more [132] channels were kept open artificially should not be ruled out. Some of them are part of the natural drainage pattern of the Overbetuwe even today,[133] and certainly during the Roman Period some channels may have been kept open with relatively little effort if the process was not entirely natural. In fact, this possibility is the only positive argument for the reconstruction on fig. 13. A waterway along the axis Nijmegen-Elst-Driel would provide means for easy transportation and a very considerable short cut of a route over water from Nijmegen to the settlements on the Lower Rhine.[134]

The course of the Rhine from Driel to the west was approximately the same as that of the pre-Roman branches and, of course, of later periods until the present day. Only beyond the Grebbenberg (169/440), where the ice-pushed ridge of the Utrechtse Heuvelrug turns to the north, could different stream-ridges develop again. The post-Roman meander-belt of the Rhine does, however, widen from Driel onwards, constricted only by the ice-pushed ridges. It is less than 1 km wide at the Drielsche Veer, and over 3 km at the western boundary of the map. This is undoubtedly due to the presence of earlier sandy river deposits in which the river-bed could change its course easily, and has probably led to, for example, the erosion of forts near Randwijk (18b) and Kesteren (37).

The IJssel (*Fossa Drusiana*)

The course of the IJssel in the area of the map is not a problem. From the point where it leaves the Rhine, it flows towards Doesburg and joins the Oude IJssel which is a functioning river ever since the Pleistocene[135] when it was a, during some periods even the, major Rhine branch. The channel widens to the northeast, because of the Pleistocene sands there. The problem with the IJssel between Westervoort and Doesburg is the time of its origin. Apart from the geological point of view, which simply states that it originated in the Subatlantic (after c. 900 BC), [136] there are currently three hypotheses on this subject.

The first claims that the IJssel already existed from the Late-Subboreal onwards, albeit at first as a rather small

130 A dating of a fossil channel there (the deposits are not Pleistocene, as indicated by Harbers/Mulder 1981, fig. 8, but regular Holocene river deposits) will be attempted in the near future. Essentially, the difference between the radiocarbon datings Elden and Meinerswijk I/1 is that between a 'type d' and 'type c' dating (see fig. 14), which is also visible in the considerable difference in the depth below surface of both samples (see fig. 15, fifth column).

131 I thank Dr Teunissen for his permission to use these unpublished data. The diagram is included in a doctoral paper by T. Kortbeek and P. van Rooy.

132 The branch from Elst over Lijnden and Homoet to Heteren is a second possibility (see p. 49, note 109), although it has not been indicated on fig. 16.

133 See for example fig. 8. Today, the artificial Linge channel is of primary importance for the drainage of the Overbetuwe (Egberts 1950, 63). It was dug during the Middle Ages (Hol 1957, 46 ff).

134 If this is true, it would be an early predecessor of the Grift, a canal from Nijmegen (through Gendt, Elst, and Elden) to Arnhem, that existed in AD 1571 and was used until AD 1740 (Hol 1957, 64–5).

135 See e.g. Van de Meene/Zagwijn 1978, *Abb.* 2.

136 Van de Meene 1977, 62.

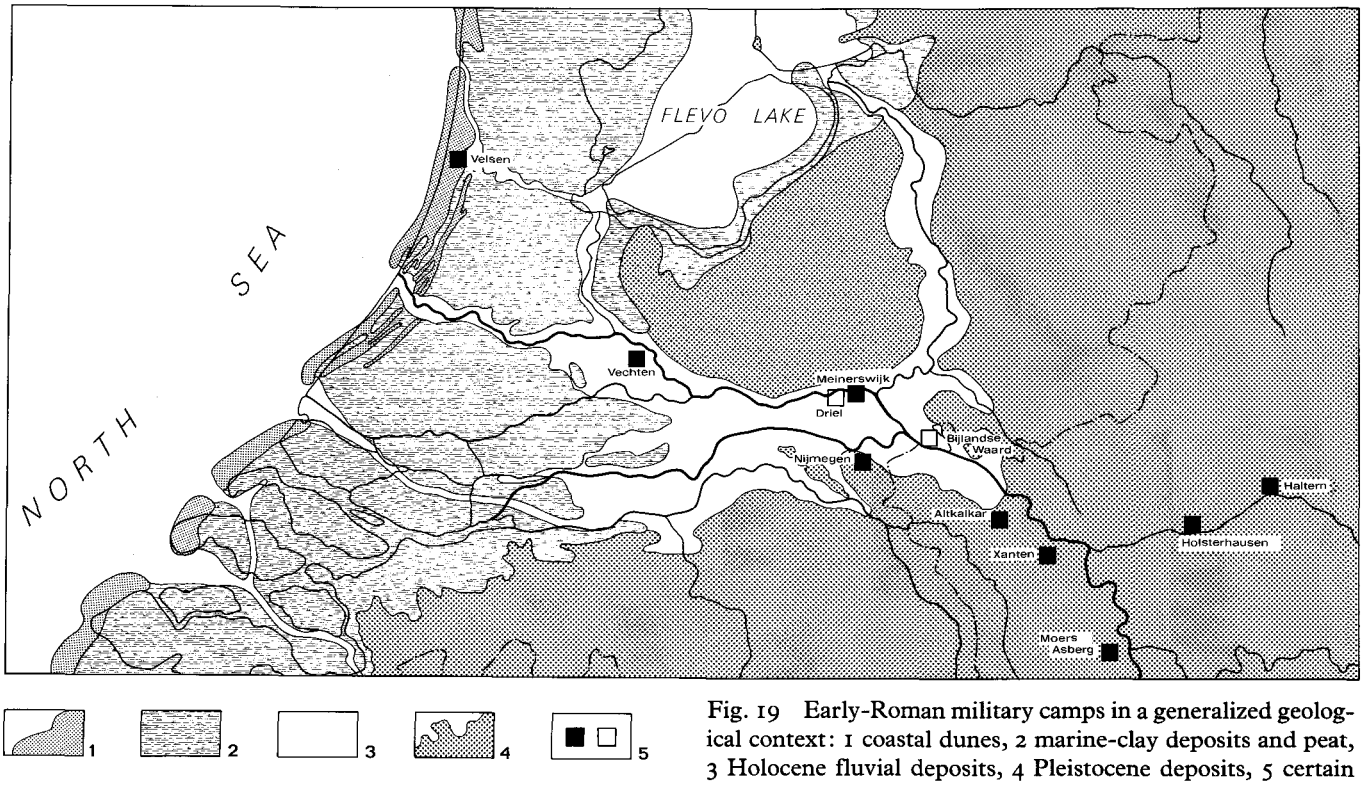


Fig. 19 Early-Roman military camps in a generalized geological context: 1 coastal dunes, 2 marine-clay deposits and peat, 3 Holocene fluvial deposits, 4 Pleistocene deposits, 5 certain and probable camps.

branch. The second relates the origin of the IJssel to the activities of Drusus' troops, while the third argues for a post-Roman dating. As is already evident from the preceding paragraphs and fig. 13, the second hypothesis is preferred here. There are several arguments for this preference, and for the rejection of the other hypotheses. The first, which was introduced by Harbers and Mulder,[137] is based on doubtful assumptions. Pre-Roman IJssel deposits belonging to the phases Gendt O (c. 1500-1000 BC) and Gendt I (c. 600-100 BC) are identified solely by their structure and composition, which are considered to be typical for those periods. Although the IJssel is supposed to have been of minor importance in its earlier phases,[138] the indicated deposits of the Gendt I phase[139] contradict this. For the – in this view – upper course of the IJssel between the Pannerdensche Waard and Meinerswijk, this is no problem because, as shown above, this was actually a branch of the Rhine which did indeed originate at least approximately during the period covered by their Gendt I phase. Both the C14 and archaeological evidence support that. For the rest of the supposed course of the IJssel, which turns eastward again from Meinerswijk to Westervoort, the other evidence contradicts a pre-Roman dating, as will be shown below.

The reconstruction of a 'Drusus' canal between Meinerswijk and Driel, which became necessary because of the sharp curve eastwards of the 'IJssel', cannot be correct either.[140] First, as is shown by the profiles on fig. 18, site 126 is located on the convex side of a meander-curve and the interpretation of its situation on the concave bank of the 'IJssel' is not correct. Second, the fact that the fort would be north of the Rhine does not just 'seem to be rather unfavourable' as was noted by Harbers and

137 Harbers/Mulder 1981.
138 *Op. cit.*, 408-9.
139 *Op. cit.*, fig. 5.
140 *Op. cit.*, figs. 5–7, but see especially fig. 6.

Mulder. It was, and even very much so. No military commander in his right mind would ever build a frontier fort with the river at its rear and it is no coincidence that there is not a single example for such a situation in the entire Roman empire.

To attribute the origin of the IJssel to the activity of Drusus' troops, and thus to a historical event, will be impossible to prove conclusively, unless an inscription commemorating the event is discovered in the surroundings of Westervoort. Nevertheless, there are a number of circumstances which do support this hypothesis which is by no means a new one.[141]

The entire hypothesis is, of course, based upon the references by Suetonius and Tacitus mentioned above (p. 52). The works of Drusus included the *agger/moles* and one (Tacitus, *Ann.* II, 8: *fossa Drusiana*) or more (Suetonius, *Claudius* I, 2: *fossae Drusinae*) canals. In these references, there is no direct connection between these works. Nevertheless, as already noticed by Ramaer,[142] such a connection is almost inevitable. The function of the *agger/moles* is indisputable: it had to divert more water to the Rhine. The context of its construction is also very clear: it was ordered by Drusus because it was apparently necessary for his plans, which included the creation of a navigable route north, to Lake Flevo and from there to the German coast. There cannot be much doubt concerning the relation between the two. The *agger/moles* was necessary to reach the *fossa(e)*, or to enable it/them to function, or both.

From a geological point of view, there are several possible locations for a Drusian fosse, but especially the (Utrechtse) Vecht and the upper course of the IJssel (see fig. 19). The identification of the Vecht with the *Fossa Drusiana* is primarily inspired by the early-Roman camp and harbour of Vechten-Fectio,[143] located a few kilometres from the Rhine-Vecht fork. Before the discovery of site 126, this was a very strong argument, because an early-Roman camp near the Rhine-IJssel fork was lacking. Nevertheless, several authors continued to reject it, because the relation between the Drusus works would be lost. The additional water provided by the *agger/moles* would have no noticeable effect on the Vecht, which leaves the Rhine at well over 90 km from the location of the dam. This argument, however, may not be entirely correct because the extra water may only have been necessary to reach the Vecht, not to allow it to function. This is because the Vecht may to some extent have an independent source of water, provided by drainage water from the ice-pushed ridges (Utrechtse Heuvelrug) and the large peat area at its foot.

New evidence on the geology of the Vecht has recently been provided by Berendsen.[144] He showed that the origin of the river can be dated to the second part of the 5th millennium BP and that it was an important branch from 3700 BP to between 1700 and 750 BP in conventional C14 years. Thus, the river did not need to be created because it already existed, and 'canalization' is not very likely: fewer windings would have made little difference and the draught of Roman river-boats is known to have been shallow.[145]

With the additional argument of the location of Fectio and Velsen (Flevum??) it can be taken for granted that the Vecht was available and indeed used as a waterway.[146] It is possible to conclude, therefore, that the *moles* was only intended to provide the necessary water to reach the Vecht during the summer. This, however, presupposes additional works to prevent that water from draining into other branches before reaching the Vecht[147] and does not necessitate digging a fosse, unless there was indeed no natural connection between the Vecht/Oer-IJ and the Flevo lake.

Although these possibilities cannot be disregarded, they are difficult to relate to the reported waterworks. It is more likely that a convenient connection with Lake Flevo existed and the location of Fectio indicates that it was used as such. But even if this is accepted, the apparent need for the Drusus works remains unexplained.

It is possible that Suetonius' phrasing is correct in that there were indeed two routes north,[148] so that a usable connection over water was ensured at all times. The cre-

141 For an overview of the literature pro and contra, and the eminent scholars involved, see Teunissen 1980a, 324–5 and, more extensively, Teunissen 1980b, 9–15.

142 Ramaer 1928.

143 See Bogaers, in Bogaers/Rüger 1974, 62–5.

144 Berendsen 1982, 169 and 188–91, fig. 8.10–8.16.

145 Often not more than c. 50 cm. See also De Weerd 1978, 20 with further references.

146 Velsen could be reached by sailing from the Vecht into the so-called Oer-IJ. It is possible that fig. 19 is not correct in the sense that in Drusus' time the Vecht was not connected to the Flevo-lake but only to the Oer-IJ.

147 Especially important is the L 14/K 1 branch (the Lek) indicated by Berendsen 1982, figs. 8.14 and 8.15.

148 As mentioned e.g. by Wells (1972, 115–6), the use of the plural in itself is not at all significant.

ation of a second route makes sense in view of the tactical and logistic demands resulting from the gigantic scale of the operations of Drusus and Germanicus, who used the Betuwe as an assembly area (*Bereitstellungsraum*) for their troops. In any case, the largest and presumably also the majority of early military camps are in the eastern part of the Dutch river area: Nijmegen (403?, 412, 417), Meinerswijk (126), somewhere near the Rhine/Waal fork (183), and probably Driel (117). From the east, the shortest and most advantageous line north would definitely have been along the IJssel. The construction of the *moles*, in combination with a cut through the natural levee of the Lower Rhine near Westervoort, would provide that connection.

This line of reasoning provides a good motive for an artificially created *Fossa Drusiana* in the east. It also eliminates the difficulties arising from the assumption that the additional water was directly necessary for the Vecht route and accounts for the observed concentration of troops in the east.[149] It is not, however, incontrovertible evidence. Therefore, the origin of the IJssel as the Drusus canal needs to be supported by more direct arguments than those mentioned so far.

It should be realized that the very nature and also the time of origin of the IJssel make it difficult to provide those arguments. The river is not only located beyond the *limes* but material evidence for occupation on its natural levees should be obtainable to the same degree as in the rest of the river area. This means that it should be material from the Middle-Roman Period, which is the most abundant. Precisely during this period, however, it is not very likely that the IJssel had already developed natural levees suitable for dense habitation. During the Late-Roman Period and Early-Middle Ages, when this may have been possible, both the number of sites in general and datable material are far fewer. Also, C14 and pollen analysis of samples from fossil channels will not provide significant *termini ante quem*, unless by sheer luck a very early channel is found, one which was also rapidly cut off again.

At a practical level, there is an additional disadvantage in this area, because archaeological activities in the Liemers after World War II have been minimal.[150] The intensive collecting of finds by amateur archaeologists, which took place in the Betuwe and the Land van Maas en Waal, did not occur in the Liemers.

Notwithstanding the many unfavourable circumstances, there are still a number of Roman finds along the IJssel. The reports on these finds are usually old and rather vague, or with doubtful find-circumstances. Nevertheless, they are not without value. Particularly interesting is Janssen's account of his discovery of what is unmistakably an ancient settlement soil in Westervoort at a place called *Dalems boomgaard* (Dalems orchard), containing finds datable to the Roman Period.[151] Janssen is the primary source for this report; he checked the situation himself, and, because his reports on similar sites in the Betuwe have proved to be accurate, there is no reason whatsoever to doubt his conclusions in this case. The site has not been included in the catalogue only because it could not be located precisely.[152]

Another reference given by Janssen, relating to seven wooden wells and Roman coins discovered in IJsseloord (on the other side of the IJssel opposite Westervoort) cannot be considered reliable.[153] Janssen is only a secondary source in this case, and the finds were made in the second half of the 18th century. The dating of the finds is therefore unconfirmed and doubtful, the precise location unknown. There are various similar reports or rumours of possible Roman finds along the IJssel or of Roman finds, possibly found along the IJssel.[154] Except for contributing to a general feeling that there may be some truth in those which are not controllably wrong, they are not very useful for our purposes.

The only other possible settlement-site worth noting is the findspot of some Roman material at the *Grote en kleine Durk*, mentioned by Pleyte.[155] Because it could be located precisely, it has been included in the catalogue as site 32. The finds are, however, rather extraordinary. Attempts to get more information on this findspot have

149 Even though many questions about the nature and true extent of the camps in Nijmegen (see Bloemers a.o. 1979, chapter IA, IB, and IC) are still unanswered, and it is possible to reject the evidence for sites 117 and 183 as early camps, the archaeological data from Nijmegen alone are sufficient proof of such a concentration.

150 See above, p. 20.

151 Janssen 1844, 225–7.

152 Janssen indicates that it was left (= north) of the old road from Arnhem to Germany. It is probably in the centre of present-day Westervoort.

153 Janssen 1844, 227–8.

154 See e.g. Zadoks-Josephus Jitta 1961 (a medal of Trebonianus Gallus). The references by Wells 1972, 115, note 3, are based on a misreading of Ramaer; although they remind us of the IJsseloord finds, this refers to 'finds' from elsewhere.

155 Pleyte 1887–1903, 37–9 (Gelderland).

failed, because the site no longer exists due to gravel-dredging operations.[156]

Although the evidence of early occupation along the IJssel is not overwhelming, which is in accordance with expectations, at least the sites at *Dalems boomgaard* and *Grote en kleine Durk* are indications which cannot be ignored. Even more significant are the stray finds from site 199 in the Lathumse Waard. These are nine weights manufactured from Roman tegulae, found in 1971 as a result of suction-dredging for sand.[157] Comparable finds are discussed in paragraph 6.6.4. Their interpretation as net-sinkers or, to be completely on the safe side, as fishing-implements is beyond doubt. Their dating as Roman is not unquestionable, but especially weights made of secondarily used brick are very unlikely to be post-Roman, as shown by many finds from other sites.

They are therefore considered here an important indication that the river was used for fishing, and thus existed, during the Roman Period. A small stretch of the fossil channel from which the finds may have originated still exists.[158] Although a C14 or pollen analysis of the channel-fill deposit is not likely to be very informative,[159] there are two datings of peat directly underneath the channel deposits in the same area (fig. 16, 15 and 16). These provide a *terminus post quem* for the beginning of the sedimentation.

Lathum I, a sample that was taken close (c. 600 m) to the river, turned out to be 2000 ± 65 BP; Lathum II, which was taken further into the flood-basin(c. 2 km from the river), yielded a date of 1720 ± 25 BP. On the basis of these datings, it is possible to conclude that sedimentation by the IJssel started around 2000 BP and that its influence had already extended far into the flood-basin in the 3rd century.[160] If the IJssel was artificially created by Drusus' troops, a better result is hardly imaginable.

Nevertheless, the interpretation of the datings is not undisputed. The geologists who submitted both samples agree that the dated peat is definitly not eroded and that its stratigraphical position indicates that it was indeed the last peat growing in the flood basin before the first sediments were deposited there by the IJssel.[161] Poelman, however, attaches great importance to the nature of the top of the peat-layer which is dark, weathered, and mixed with clay (*veraard*) and concludes that there must therefore have been a considerable hiatus in the form of a dry period between the last peat-growth and the first deposition of clay.[162] This allows him to conclude that the origin of the IJssel should be dated to post-Roman times, preferably the period after AD 500.

This, however, is contradicted by the palynological evidence from the Lathumse Broek which indicates no such hiatus. Moreover, the fluctuations in that pollen diagram conform to the characteristic fluctuations elsewhere in the eastern river area[163] and the chronological significance of these fluctuations has been checked by other C14-datings in addition to GrN-7525.[164] There is no reason to accept the alterations in the chronological interpretation as proposed by Poelman,[165] but this does not invalidate his dry period 'hiatus'.

Teunissen[166] explains the dark upper part of the peat in a different way, namely, as the result of increased biological activity, due to an increased supply of nutrients in the soil (eutrophication) by the first deposits of clay. This is a reasonable alternative to Poelman's view, but even if the 'dry' explanation is true,[167] there is still no indication of the period of time involved. The dry hiatus may have been due to an amelioration of the drainage of the flood-basin after the origin of the IJssel (which, by the way, may have followed an already somewhat improved drainage by the Rhine branch from the Loowaard to Meinerswijk several centuries earlier). The fact that the dark peat-layer increases in thickness further away from the river[168] is in agreement with such a view.

In conclusion, it can be said that the origin of the IJssel

156 The large dredging-lake, which is still visible on topographic maps of the 1960s, has now been filled in again.

157 These are in the Gemeentemuseum Arnhem, collection of the Gelderse Archeologische Stichting, nos. 1971-12-194 to 202. They are identical to weights from Wijchen (fig. 53, 1-5).

158 STIBOKA 1975, sheet 40 W, Arnhem, from 198.4/444.9 to 198.9/444.7.

159 It should, and hopefully will, be done anyway in the near future, because it is the only case where a dating within the Roman Period is at least possible.

160 C14 and real ages do not differ very much in this period, see Mook 1978, fig. 2.

161 Poelman 1981a, 21; Teunissen 1980b, 33.

162 Poelman 1981a, 1981b.

163 Teunissen 1980b.

164 See Teunissen 1982, fig. 11.

165 Poelman 1981b, 173-4.

166 Teunissen 1981, 168.

167 These are thus indeed two possible explanations as Poelman so pointedly observed (1981b, 175).

168 Poelman 1981b, 174.

as *Fossa Drusiana* cannot as yet be considered proven beyond doubt. The need for a fosse at this spot can be argued, but that does not constitute definite evidence for its existence. More direct evidence is available, but it is not incontrovertible. Proof of Roman Period habitation on the natural levees, which is slight as was to be expected, has to rely on older evidence. Use of the river (for fishing) is based on artifacts whose dating to the Roman Period has been shown but which is not absolute and can (and thus predictably will) be disregarded, even if it were only in this particular case. The interpretation of C14 and pollen-analytical data is apparently open to dispute and even the presence of Roman troops close to the bifurcation (site 126, probably 117, and maybe even 194) has already been rejected as evidence. The arguments were faulty,[169] but nevertheless, the presence of one or more camps is only circumstantial, not direct evidence.

These arguments or considerations indicate that an identification of the upper course of the IJssel with the *Fossa Drusiana* is not completely certain. There are, however, so many basically different but mutually supporting pieces of evidence that the connection of a historical to a geological event, in itself a difficult feat, need not be rejected. On the contrary, the available evidence is highly significant and, short of the find of a commemorative inscription or a sunken barge in a fossil channel,[170] the best that was to be expected, although it needs further support.

The arguments for rejecting a post-Roman dating of the IJssel have already been largely incorporated in the foregoing discussion. It is useful, however, to point to a few other arguments in this respect. In his study of the Liemers area and the IJssel deposits, Pons[171] concluded that in the large flood-basin between Driel, Doesburg, and Zevenaar, new branches (part of the Rhine and the IJssel) were formed at the beginning of the Roman Period. There are many geomorphological and lithological similarities between these branches,[172] but the Rhine branch is definitely – although relatively shortly – pre-Roman, as shown above. His conclusion that both branches were initially of limited importance and comparatively young holds good. It is also confirmed by Van der Schrier and Ente,[173] who have shown that sedimentation of the IJssel at its lower course became important only in the 12th century, according to Ente because of floods and the construction of dikes, as well as a better connection between the Almere (the medieval enlarged successor of the Flevo lake) and the North Sea. Although an earlier existence of the IJssel is recognized, it is dated to the 8th century at the earliest, mainly on historical and climatological grounds. Use of the latter does, however, present problems,[174] while the former only gives a *terminus ante quem* which is very crude indeed. Ente indicates that while, in his opinion, the origin of the IJssel is post-Roman it is not precisely datable and, even more importantly, that the origin of the upper course of the IJssel did not lead to recognizable changes below Doesburg and in the formation of the IJssel delta, which occurred only from the 12th century onwards. As was thus already observed by Ente and more explicitly by Teunissen, followed by Lanting and Mook,[175] the formation of the IJssel delta is therefore of no consequence for any dating of the upper-IJssel course.

The Waal (*Vahalis*)

In contrast to the Lower Rhine and IJssel, a discussion of the course of the Waal during the Roman Period does not present any problems or new aspects. After the bifurcation of Rhine and Waal south of the Bijlandsche Waard, the Waal must have followed more or less the same course as today. North of Nijmegen, the Roman channel must have been further north than the present one, as shown by the post-Roman erosion of part of site 399. In Druten it was somewhat more to the south, as shown by the find of a Roman barge (site 211). A few

169 Poelman (1981b, 175) based his reasoning on the situation during the Middle-Roman Period, when the entire military situation, and thus the reasons for establishing forts, were completely different. It can be shown clearly (see e.g. Gechter 1979, esp. p. 114) that in general the location of camps for the offensive campaigns were determined by river courses.

170 Even a future discovery of some sort of settlement along the IJssel will not be sufficient, because inevitably someone will provide an argument to explain its presence there without the need to presuppose IJssel-deposits.

171 Pons 1953, 23–4 and fig. 3.

172 See also Pons 1957, 62, Van der Schrier 1968, 244, and Harbers/Mulder 1981.

173 Van der Schrier 1968; Ente 1973–74.

174 See Berendsen 1982, 83–5.

175 Teunissen 1975, 92–3, note 3 and Lanting/Mook 1977, 171.

others parts of the meander-belt of the Waal may have survived, such as those around Gendt, south of Wely and Dodewaard, possibly also at a few points under the present water-meadows.
In general, the Waal must have become the most important branch again after, possibly even during the end of, the Roman Period. The large extent of post-Roman channel and bank deposits as well as basin-clay (covering all but the highest parts of older stream-ridges) testify to that.[176] Especially the earliest deposits, which are dated by Pons from the end of the 3rd until well into the 5th centuries[177] and included as the Gendt II deposits (AD 250-600) by Harbers and Mulder, are relevant in the present context. Although, as Berendsen[178] has shown, there are no reasons to assume such a precise synchronization with the Duinkerke II transgression phase, at least at some places a considerable sedimentation must have occurred between the Middle-Roman and Merovingian Period. Clear examples are the thick bank deposits between the Roman settlement (158) and Merovingian cemetery (159) in Lent, and between the Roman and Carolingian occupation layers recently excavated on the Waalkade in Nijmegen (403).[179]

The Meuse (*Mosa*)
Down to Grave/Nederasselt, the course of the Meuse remained more or less the same during the Holocene. At least during the entire Roman Period, the river had two branches from there onwards (which probably explains the large post-Roman meander-curves at this spot). The northern branch, the Wijchense Maasje, already existed in the Late-Neolithic and continued to function in the Roman Period. At site 316, a lot of Roman material (from site 315) was found in the remains of the fossil channel, the Wijchense Meer. These included also the net-sinkers from fig. 53. Pons' assumption that the Wijchense Maasje ceased to function after the Roman Period has been confirmed by pollen analysis of the c. 4-m-deep channel-fill deposit.[180] This has shown that the final silting-up of the channel probably started during the 5th century and was definitely in full progress during the Merovingian Period.
The reconstruction and dating of the southern branch present more difficulties. According to Pons,[181] its origin is later than the Wijchense Maasje. The reconstruction of its course is more or less the same as that proposed by Pons: it goes from Lienden over Batenburg/Dieden and Haren to Macharen, and from there to the Ossermeer, which is an oxbow lake and was definitely part of a functioning river during the Roman Period as demonstrated already by Van Diepen.[182] More recent finds, again including net-sinkers, are also available.[183]
Because of lack of precise information, it is not entirely certain whether the Meuse began to follow its present course during the Late-Roman or Merovingian Period, or even later. The continuing habitation on the channel zone deposits cannot be used as an argument for or against. In any case, the results of Van Diepen's survey indicate substantial post-Roman deposits along the Meuse and there are no compelling reasons to disagree with the chronological interpretation of these.[184]

From the foregoing discussion, it is clear that the reconstruction of the river system on fig. 13 is only valid for the Late-Iron Age and the Roman Period. The course of the Meuse in particular changed considerably from the 5th century onwards, but also the still functioning old branches of the Rhine in the Betuwe (from ? to Elst and from there to Driel as well as, possibly, to Homoet and Heteren) were no longer active by then.
Other changes, which do not affect the reconstruction greatly, are the increased importance of the Waal at the expense of the Lower Rhine, which may quite conceivably have included alterations in the location of the bifurcation of both branches. The importance of the IJssel also increased, or, should our arguments concerning its origin be incorrect, it originated around this time. In any case, there is no dispute about its existence during the Early-Middle Ages.

176 See e.g. Pons 1957, 48–9, 55–64 and Harbers/Mulder 1981.
177 Pons 1957, 57.
178 Berendsen 1982, 81.
179 For Lent, see *JROB* 1972, 24 and 1975, 21; for Nijmegen, *JROB* 1983, chapter II D (in press).
180 I thank Dr Teunissen for putting the results at my disposal. The analysis was done under his direction by H. Cuppen and P. van Loenhoud.
181 Pons 1957, 43–4.
182 Van Diepen 1952, 114–6. The Ossermeer is an oxbow lake located just outside the area studied, to the southwest of site 470 (in square 165/422).
183 See Verwers/Beex 1978, 32–3.
184 See Pons 1957, Appendices 8 and 9.

3.4 ROADS AND ROUTES

The literature on Roman roads in the Dutch river area is comparable to or even exceeds in volume that on the rivers. In both cases, the causes are similar: there are few real clues, various historical sources, intriguing problems, and enough people with a vivid imagination. There is also a difference. The discussion on Roman rivers is necessarily based to some degree on data from the geo-sciences. In contrast, even though the course of roads is evidently directly related to the landscape, this rather basic fact has often been ignored or overlooked.

Not long after the last war, Modderman[185] published a short article which provided the first, and as far as the eastern river area is concerned the only, sensible discussion not just of the possible courses of Roman roads but also of the preconditions for studying them. Subsequent literature or maps which ignore Modderman's main outlines should not be taken seriously,[186] although later research has of course provided new or additional data with regard to his concrete proposals within the general framework.

In a recent article, Bakker[187] provided an overview of theoretical principles and practical applications of road-research in northwestern Europe. Although he is concerned primarily with the very old and continuous long-distance connections on Pleistocene deposits, the general principles can also be applied in the river area and for the Roman Period, and confirm Modderman's earlier considerations. As a rule, one can expect roads in the (eastern) river area to be situated on pre-Roman stream-ridges and on natural levees of Roman rivers. They will also be situated in such a way that the connections are as short as possible under the existing circumstances in each case. The direction of some roads will also be influenced by optimal connections into the adjoining Pleistocene area and their further course in these areas will be determined by the natural relief and local soil conditions.

However, in contrast to the prehistoric situation, there are additional circumstances which may have been important during the Roman Period. Roman roads did not just come into being as a kind of optimum paths laid down according to the principle of least effort, as the result of many influences over a long time with the natural conditions as the main determinant. They were constructed on the orders of a central authority which had both general and very specific objectives in mind. Also, although the 'optimum path' and 'least effort' principles continued to be very important, this central authority had both the power and the technical means to overrule them when that was considered desirable for other (military, administrative, political, economic) purposes.

Because they were actually built, there is also a very clear difference between Roman and prehistoric roads: In general, a Roman road is a 'real world' phenomenon, an archaeological feature that can be traced precisely and excavated; Prehistoric roads are often more mental concepts, based on general or specific considerations but not, or rather no longer, actual physical entities.

This difference is, of course, not at all suitable to distinguish Roman from prehistoric roads in practice. Main roads (*viae*) were indeed constructions with metalling, parallel ditches, and the like, but there is a whole range of road types and the simpler side-roads or tracks (*diverticula*) are in no way different from prehistoric roads. Also, prehistoric roads are sometimes indeed observable archaeological entities, such as cart-tracks or corduroy roads.

Nevertheless, in a general way the distinction is valid, but it is mentioned here for a different purpose. Especially during the Roman Period, but in fact during all archaeological periods, there is no terminological clarity. A phenomenon that can hardly be drawn on a map to a scale of 1:100,000 with a very thick pencil is called a road just as well as a precisely known road that requires at least a scale of 1:10,000 or even more for all known details to be depicted. In the present discussion, a distinction will therefore be made between two concepts, namely roads and routes. In principle, a road will be called anything that is actually present, a route anything that is reconstructed and supposed to be or to have been present.

Although seemingly clear-cut, this distinction requires some further discussion. First, routes are more or less the broader framework within which, if correctly reconstructed, further research may eventually lead to the discovery of roads or parts of them. This implies that the

185 Modderman 1952.

186 A recent example is Cowan 1974, who treats the Betuwe almost as a uniform plain where one can draw lines and declare them roads at will.

187 Bakker 1976. See also Bakker 1973, chapter 7.3, esp. 36–46.

transition from route to road is to some extent fluid. Sometimes, the course of a route can be so well-defined that, given the fact that the type of road involved will have left few or no observable traces, it becomes more practical to refer to it as a road. Examples are sections of barrow roads[188] or roads through urnfields.[189] Also, when fragments of roads are precisely known, it is not very useful to designate the intermediate stretches as routes, unless they are too long and there are too many alternative options.

A second aspect of road and route is that these concepts may be used to designate the same phenomenon in different disciplines. An example is the so-called *limes* road, which ran along the Rhine frontier, connecting the various forts. For the student of classical sources or of the Roman defence system, who can argue on the basis of the *Tabula Peutingeriana*, this is indeed a road. For someone discussing in detail a particular region where clear traces of this road are lacking, there is merely the fact that the road is known to have existed there. Only its possible route can be determined and a study of the geological situation may reveal the locations where its remains can be profitably looked for. This is, by the way, exactly what Modderman did in his 1952 article. 'The problem of the Roman roads in the river area' is that they have not been found and that only plausible routes can be proposed.

Nevertheless, several aspects of roads in the eastern river area should first be mentioned, to be followed by a discussion of the main available routes, partly confirming, partly in addition to Modderman's observations.

3.4.1 The Roads

Some traces of Roman roads in the eastern river area are known, although they are few and far apart. The causes of this dearth of traces are difficult to determine. They are probably very diverse. The roads in the Holocene area must have been situated on channel zone deposits or Pleistocene sand dunes, which generally provided reliable connections throughout the year. The possibility of important connections through flood-basins can be ruled out, because the particular configuration of stream-ridges and natural levees would have made such cumbersome and costly constructions unnecessary.

The erosion of former natural levees and the deposition of new sediments after the Roman Period are undoubtedly important causes of the scarcity of recovered traces of roads. A second factor is that only major roads may have been metalled, presumably with gravel from the river area itself. This last circumstance[190] may point to a third factor. Coarse sands with gravel and even gravel deposits to occur in the subsoil of the river area, and accidentally located Roman roads may not have been recognized as artificial gravel deposits. Flooding may also have spread the gravel of roads too widely.

Actual remains of Roman roads have been found at the following sites:

1 *Site 126.* Section through a road, presumably just outside the fort and dating to the Flavian period.[191] The gravel had been partially washed away and deposited in a V-shaped ditch.

2 *Site 242.* Section through a road of which only the two ditches (c. 6 m apart) remain.

3 *Site 407/415.* Parts of metalled roads through the wards (*vici*) of the *canabae legionis*.[192] One of the western roads is probably part of the connection between the fortress (412) and sites 403 and 399. The eastern road is perhaps the beginning of a route to Rindern/Harenatium.

4 *Kellen.* Section through a metalled road (width c. 3 m) bordered by deep ditches, underneath a medieval road;[193] presumably part of the *limes* road from Altkalkar/Burginatium to Rindern/Harenatium.

5 *Site 500.* Stretch of a metalled road (width c. 8 m) through the vicus,[194] probably part of the road from Tongeren/Atuatuca to Nijmegen/Noviomagus. The same road has been identified at several other places immediately to the south.[195] It presumably crossed the Meuse at Cuijk. Part of a road has also been reported from Katwijk-aan-de-Maas, north of Cuijk.[196]

It should be noted that all of these sites, with probably one exception (242), are more or less directly related to the major roads attested in the historical sources. Site 242 is also the only one that was not demonstrably metalled. If one accepts for the moment that the identifica-

188 See e.g. Bakker 1976, figs. 2 and 11.
189 See Kooi 1979, figs. 153–5 and 158.
190 See below, 184.
191 Willems 1980a, 337.
192 For a plan see Bloemers 1979d, *Abb.* 50, nos. 2 and 6.
193 Reported by Gorissen 1952, 49 and fig. 37. The exact location of the profile is not mentioned. The remaining stretch of the road is located in square 207/424.
194 For a plan see Bogaers 1967, *afb.* 8.
195 See Beex 1973, 163, sites 248, 252, 253 and 256. See also Hermans 1865, 16–27.
196 Beex, *op cit.*, site 240, based on Hermans 1865, 19.

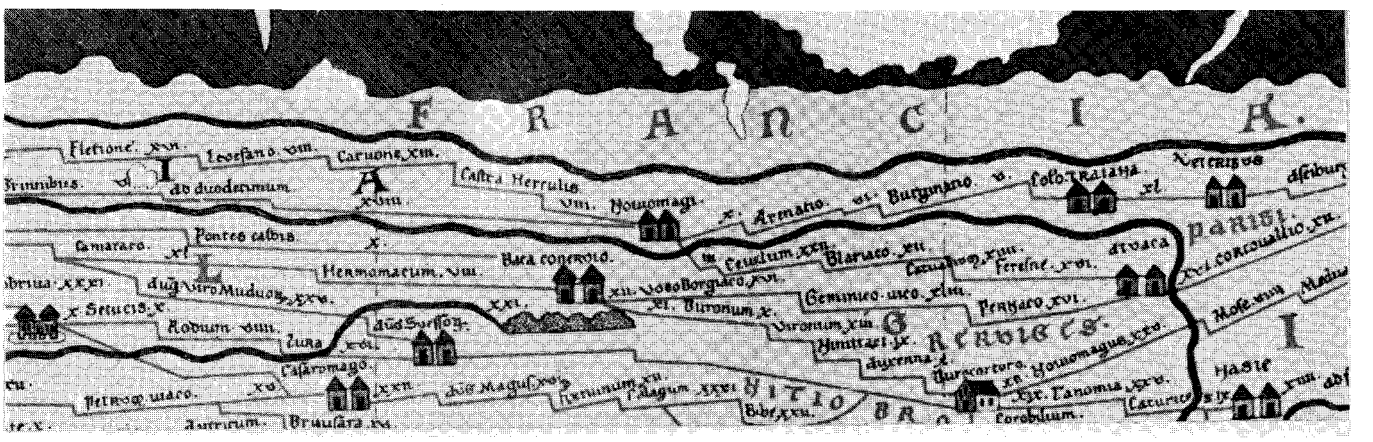
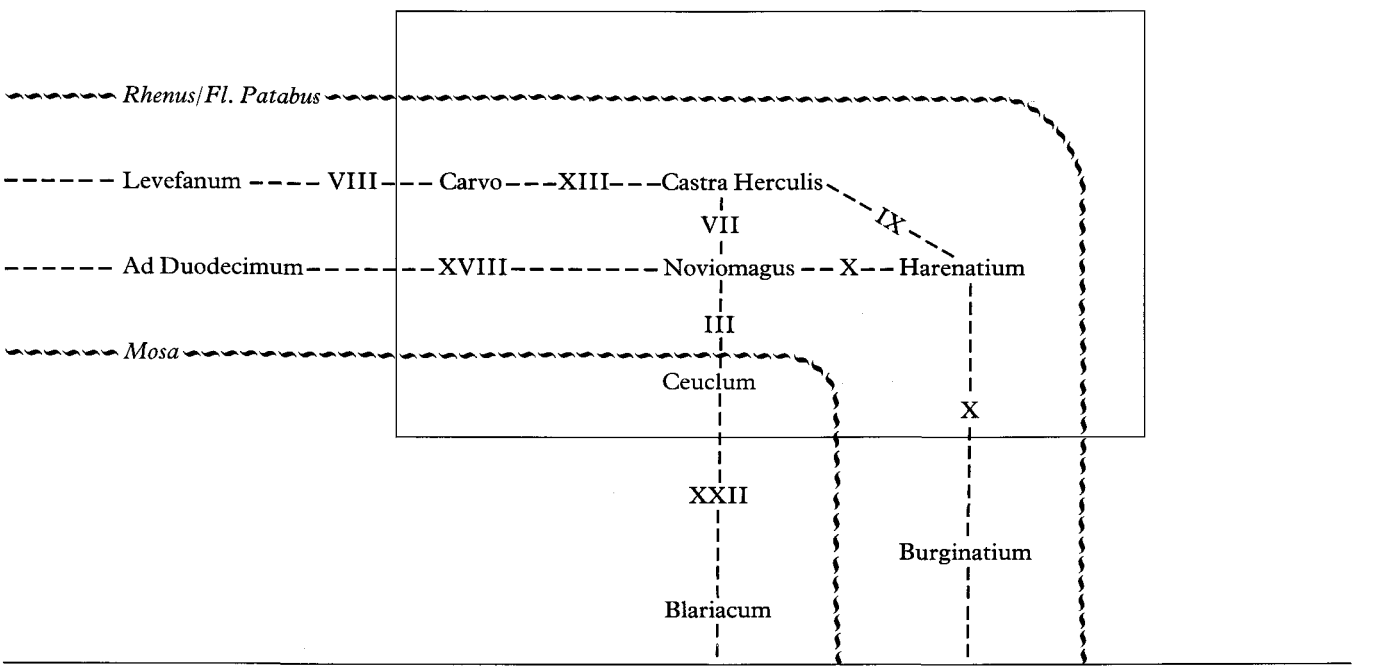


Fig. 20 Fragment of the *Tabula Peutingeriana*, part of segment I (ed. K. Miller).

Fig. 21 An interpretation of the road system from the classical sources



tions of Kesteren/Carvo(ne), Rindern/Harenatium, and Cuijk/Ceuclum are correct, the relevant sources for the area in between should cover the eastern river area. They indicate a total of four roads. First, there is the *limes* road coming from Burginatium. On the *Tabula Peutingeriana* (fig. 20) this road does, however, not follow the *limes* beyond Harenatium. Instead, it turns to Noviomagus and from there back to the Rhine towards Castra Herculis and Carvo. The location of Castra Herculis at the Rhine is confirmed by other sources, namely, Ammianus Marcellinus (XVIII, 2, 4), Libanius (*Oratio* XVIII, 87), and, if the interpretation of the corrupted name is correct,[197] the Anonymus Ravennas (*Cosmographia* IV, 24). This is, therefore, a second road. The presence of a continuation of the *limes* road from Harenatium to Carvo can, however, be deduced from the *Itinerarium Antonini* (368-70).

A third road is indicated on the Peutinger Map as going from Noviomagus westwards to Ad Duodecimum. The fourth is also known from the *Tabula*. It comes from the south along the Meuse over Ceuclum and crosses the Meuse before reaching Nijmegen. Strictly according to the sources, a road system can be deduced such as presented in fig. 21.

The rectangle represents the eastern river area if the three identifications of Carvo, Harenatium, and Ceuclum are correct. The indicated distances are mainly according to those mentioned on the *Tabula Peutingeriana*. The distance between Castra Herculis and Harenatium was calculated by subtracting the XIII between Carvo and Castra Herculis on the *Tabula* from the XXII between Carvo and Harenatium which are mentioned in the *Itinerarium Antonini* (369,4). The distance X between Harenatium and Burginatium is also from the itinerary (256,2) which later (370,1) gives VI for the same distance, confirming to the *Tabula*.

If the identifications of both places are correct and if the distances are indeed indicated in *leugae* of 2.22 km.,[198] then the latter figure is certainly corrupt. In this respect, two additional remarks can be made. If both assumptions also pertain to the road between Noviomagus and Ceuclum, which is very likely, the distance is also corrupt and should be read as VI.[199] If they are valid for the limes-road between Carvo and Harenatium, the figure XXII could be amended to, for example, XXV.[200]

Considerations like this reveal the limited value of the sources when archaeological realities are concerned. A reconstruction such as that presented here, is already open for debate when place-names and distances are introduced,[201] even before the historical reconstruction is confronted with real settlements. The only way in which the historical information on Roman roads can be formulated without immediately triggering all sorts of arguments is probably to state simply that three main roads can be discerned: A northern east-west road along the Rhine (the *limes* road) which does not pass Noviomagus, a southern east-west road, branching off the *limes* road and passing Noviomagus, and a south-north road along the Meuse, crossing the southern road in Noviomagus and reaching the *limes* road at some point along the Lower Rhine.

In order to determine the course of these roads where they have not been found, it is necessary to consider first the network of routes that can be proposed for the eastern river area on the basis of archaeological and geological data.

3.4.2 The Routes

It is obvious that all kinds of roads and tracks on Pleistocene and channel zone deposits will have connected the settlements. On the basis of Appendix 3 especially, a great number of their courses can be drawn approximately. In order to avoid confusing detail, only the main routes through the eastern river area have been indicated on fig. 22, together with the river courses. This was necessary because of the direct relation between the two, but it should be kept in mind that the channels indicated on fig. 22 have no relation to the real channels during the Roman Period. They are merely drawn as straight as possible through the middle of the reconstructed meander-belts of the Roman rivers and thus represent only the system of water-routes next to the system of land-routes. A number of settlement-sites have been added to this picture.

197 See Bogaers 1960–1, 310–1, note 254 and 1968, 152.

198 See e.g. Stolte 1938, 703–4 and 1959, 58–9. The use of *leugae* and not *milia passuum* on this part of the *Tabula* is nowadays universally accepted.

199 Cf. Stolte 1938, 705; Bogaers, in Bogaers/Rüger 1974, 84.

200 Unless one is prepared to consider an absolutely straight road (which cannot be excluded), in which case 22 leugae could just be barely enough.

201 See e.g. Stolte 1938, 714 (slightly different) and Bogaers 1968, 155–6 (very different).

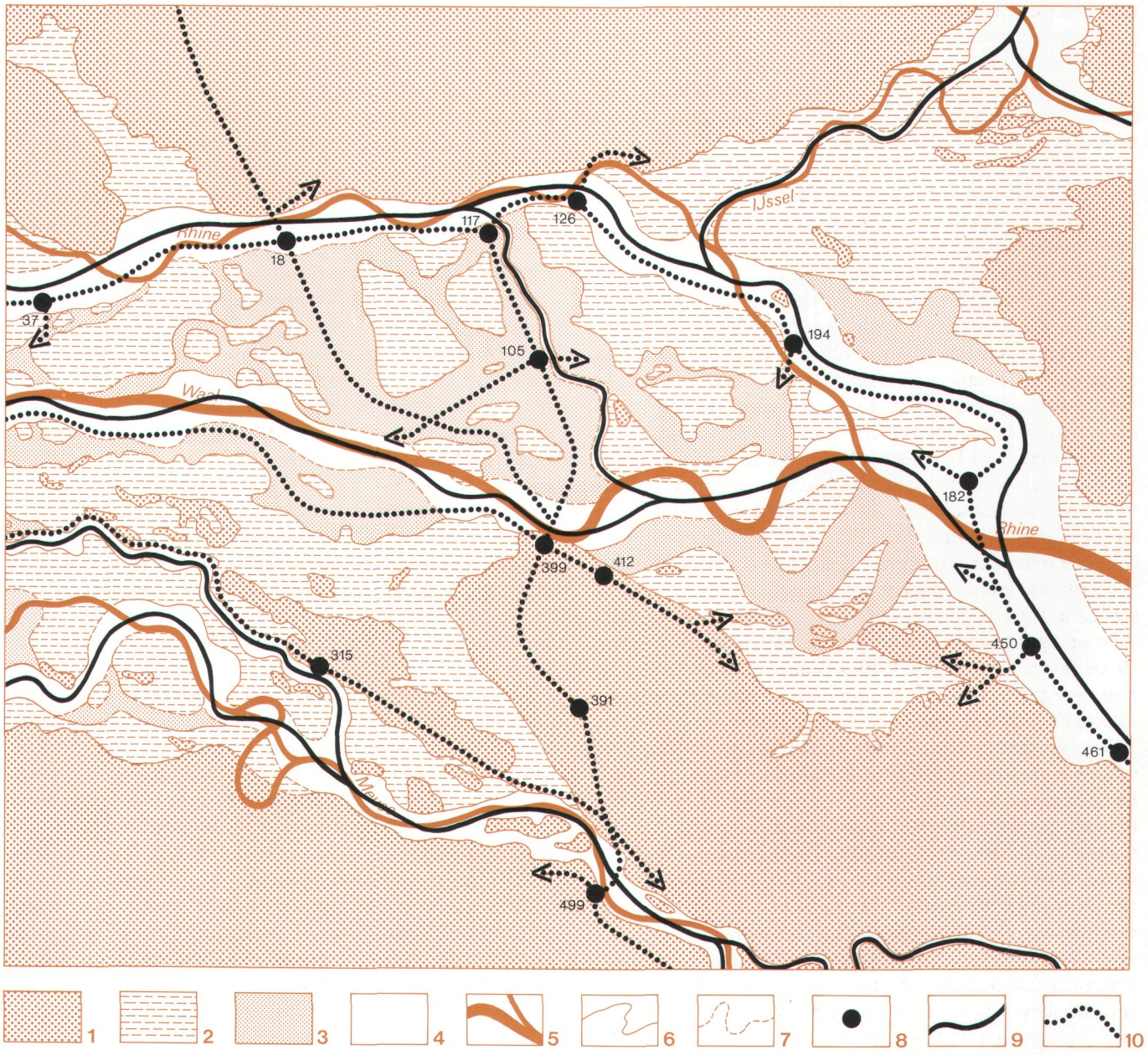


Fig. 22 The system of major routes and (schematic) river-channels in the eastern river area during the Roman Period: 1–7 see fig. 13, 8 settlement sites (mostly military), 9 channel, 10 route. Scale 1:250,000.

The first route to be considered is that along the Rhine. It must have followed the southern natural levee of the river, connecting the various military forts and/or frontier posts, and crossed the Waal fairly close to the point where it left the Rhine. The route must have had considerable military importance and therefore been usable throughout the year. The real road may thus have been somewhat more winding than major Roman roads usual-

ly were, following the highest points of the levee. Because the Roman Rhine was a free meandering stream, this road may have changed its course at some points during its time of existence, causing additional windings. On the other hand, the means to keep it exactly as desired were probably available.

In addition to this east-west route, there is an obviously important south-north route along the axis Cuijk-Nijmegen-Driel. In a general way, this route follows the same course as the railroad over Cuijk, Nijmegen, and Elst to Arnhem.[202] Up to Cuijk, the road is more or less known. It must have crossed the Meuse there, if only because of the presence of site 499/500 at that point. From Cuijk, the route will have continued to Nijmegen, but after site 391 (Heumensoord) its course is not certain. It may have gone quite straight, to site 403 (Valkhof), but for the 2nd and 3rd centuries a course to site 399 (as on fig. 22) and for the 1st century to site 412 is also conceivable. From Nijmegen, where it crossed the Waal, the obvious and most direct route to the Rhine is over Elst (site 105) to Driel (site 117).

The important settlement in Elst was undoubtedly situated at the intersection with another east-west route, with a connection eastwards to the fort at site 194 and westwards back to the Rhine (site 18), as well as along the northern levee of the Waal. A more direct connection between Nijmegen and Randwijk (site 18), partly as an alternative to the route over Elst, is also quite likely. There are thus two important connections between Nijmegen and the Rhine at Driel and at Randwijk.

In both cases, a relation with the traditional river-crossings, at the Drielse Veer and Lexkesveer, and thereby with routes going further north, immediately comes to mind. Site 125 may support the idea of a ford across the river at the Drielse Veer,[203] but, except for the medieval road-system (see note 207), other indications for an actual route north of the Rhine are still lacking in this case. For Lexkesveer, the situation is very different.

There are no fewer than three possible routes to the north from there: one over Wageningen to Ede, along the border of the high lying coversand in the Geldersche Vallei, and two others following the valleys of the Renkumse Beek and the Heelsumse Beek. The earliest evidence for these routes, especially the latter two, dates to the Neolithic. Although the relevant data have to be treated with some caution[204] the impression of an important north-south connection across the Rhine is quite convincing and in agreement with the fact that stream-ridges and/or natural levees between Herveld and Randwijk were already habitable by then. A connection with some of the barrow roads on the Veluwe is also plausible.[205] The presence of these routes during the entire period covered by this study is quite clear from the Appendices. The western route especially, between Wageningen and Ede, can be traced by a variety of sites, and it is also very attractive from a geo(morpho)logical point of view. Its southern part survives to the present day in the so-called Diedenweg.[206]

During the Roman Period, it may have been of military importance. In this respect, a continuation of the route along the eastern side of the Geldersche Vallei towards Ermelo can be proposed.[207] The only known exclusively middle-Roman fort or camp north of the *limes* is situated there. This has been interpreted as a march-camp,[208] but it is much more likely to have been an outpost fort, probably intermittently occupied and part of the early-warning apparatus of the forward defence system.[209] Arguments for this interpretation are the published plan, which indicates that there are almost certainly at least two periods, and the fact that recent finds include 1st-century pottery in addition to the late 2nd- or early 3rd-century material already known.[210]

The tile-stamps from the civil settlements at sites 1 and 7 may also be indicative of a military presence along the road, but the reason why and the way in which they arrived there may just as well be quite different. In any

202 See the topography on the Appendices.

203 See Holwerda 1931 on this subject.

204 See Modderman 1962–3, Map 2, but also the remarks on p. 11.

205 Cf. Bakker 1976, 79.

206 See Van Es 1964, 187 and fig. 33; Blommensteijn a.o. (eds.) 1977, map after p. 168.

207 For Roman finds, see Blommensteijn a.o. (eds.) 1977, maps after p. 182. In contrast to this suggestion, research on the medieval road-system on the Veluwe indicates that a route from the Rhine towards Ermelo is more likely to run along the Heelsumse Beek or, still better, to begin at the Drielse Veer (pers. comm. Drs H.A. Heidinga, 19.1. 1983).

208 Bogaers/Rüger 1974, 33–5. For the location of the fort, see fig. 6.

209 Luttwak 1976, chapter 2.

210 Van der Sanden 1981. It should be remembered, however, that conclusive evidence for Ermelo as a Roman camp has never been obtained. It could conceivably be some kind of native settlement.

case, the presence of a very old north-south route crossing the Rhine and intersecting the *limes* road near Randwijk can be deduced from a variety of different sources.
Apart from the route along the Rhine, there are several other east-west connections: on both sides of the Waal and along the branches of the Meuse. Not all of these possible routes have been indicated on fig. 22, partly because there are sometimes several alternatives for the most important routes which fig. 22 aims at presenting. This is true, for example, for a connection from site 499/500 to the Maaskant area, south of the Meuse, but also for the undoubtedly very important east-west connection over Nijmegen.
From Nijmegen to the west, the route along the Waal can be traced without real problems.[211] To the east, however, there are several options which certainly all existed, but it is not very likely that they were all equally important as the main connection between Nijmegen and Xanten. The obvious route of this historically known road could be the most southern one, along the edge of the Pleistocene deposits. It is usually[212] considered to run from Rindern over Donsbrüggen, Nütterden, Kranenburg, Wijler, and Berg en Dal (site 433) to Nijmegen. There are no real objections to this trajectory, although three points deserve to be mentioned. First, the point where it leaves the route along the *limes* is situated at site 450 because of the identification of Rindern with Harenatium. From a geological point of view, a situation of the fork in Kleve would seem to be more obvious but the historical and archaeological implications (Kleve = Harenatium) definitely less so. Second, the road may have avoided the low and wet area around Kranenburg and run from Nütterden over Frasselt and Groesbeek to Berg en Dal. Third, if the location of the fork in Rindern is accepted, which is the most logical proposal at present, then there is an alternative and very straight route from Rindern over Mehr, Niel, Zyfflich, and Beek to Nijmegen. As far as known findspots of Roman material are concerned, this route is even more positively traceable. This may, however, be accidental and there are no decisive arguments to favour any of the trajectories discussed so far, unless the milestone from Beek (site 427) is considered as such. It would definitely support the trajectory over Zyfflich and not the one over Berg en Dal. The latter may only have been a connection to the tile factory at the Holdeurn (site 433), as was already observed by De Waele.[213]
The other routes from Rindern to Nijmegen are longer and less likely to represent the main road. They branch off the *limes* route north of Rindern and then follow the stream-ridges in the Duffelt towards Zyfflich, over Keeken and Niel as well as over Millingen. Even less plausible as a main road is a route over Millingen and Ooij, on the southern levee of the Roman Waal.
On fig. 22, part of a southern east-west connection has also been indicated. It is the western continuation of a route along the Niers and passing site 466, which must have joined a route along the eastern side of the Meuse somewhere near Gennep, crossed the route from Cuijk to Nijmegen, and then followed the indicated course towards Wijchen (site 315) and along the northern branch of the Meuse.
Although not indicated, it is possible that there was a more direct connection between Nijmegen and Wijchen[214] approximately following the same course as the present-day railroad or to the south of there. This would be the only example of a route across what is essentially a flood-basin, but the underlying and sometimes relatively high Pleistocene fluvial deposits there[215] make if different from the normal flood-basins. Nevertheless, the road would have had to pass through wet and low lying areas and traverse numerous gullies, and it is remarkable that no traces of it have ever been discovered especially at those points. This is, however, not a sufficient reason to reject its existence, particularly when Wijchen was undoubtedly a very important place and a long detour to Nijmegen over Mook or, perhaps, over Bergharen and Ewijk, is the only alternative.

3.4.3 Conclusions

From the preceding paragraph it is clear that the general system of routes in the river area can be reconstructed without too much difficulty. Such a reconstruction also leads to observations which are not immediately obvious

211 This is also the route of the Koningsstraat as described by a.o. Edelman (1951, 320–1), Modderman (1951, 46–7 and the map), or Pons (1957, 47 and Appendix 7). But, although the Koningsstraat can be called a Roman route, it is not very likely to be a Roman road.
212 See e.g. Bogaers 1968, 155.
213 De Waele 1931, 55.
214 Cf. Bogaers 1968, 158 and note 79. The presence of the burial sites 331–332 (cf. p. 90) may also be an indication for a road.
215 See above, p. 42, and Pons 1966, Appendix 1.

from the Appendices, for example, regarding the location of the forts along the Rhine. Virtually all of these forts have a direct connection with the hinterland, over a natural levee or a stream-ridge; in one case (site 18b) this connection is so important that the situation is reversed and a hypothetical fort has been assumed on those grounds. The only exception is site 126, which could only be reached over Driel.[216] Its location can more profitably be explained by a relation to the Drusian fosse as well as a possible route over the coversands north of the IJssel. It is located exactly at the point where the Rhine reaches high ground and turns westwards.

Also, the rather favourable location in relation to traffic of places like Nijmegen and Elst is quite clear. Unfortunately, however, the analysis of the system of routes leads to ambiguous results when compared to the system of major roads which is largely known from the sources. All the elements mentioned on page 66, a *limes* road, a southern east-west road, and a south-north road, are present. Nevertheless, except for the connection over Cuijk as far as Nijmegen and that along the Rhine, all other identifications of roads with routes are open to dispute.

It is very much a matter of what one wishes to prove (and, consequently, which data should be declared corrupt) that determines the proposals advanced by the numerous authors on this subject. The main source of trouble is that most identifications of place-names with sites are not absolutely indisputable and have given rise to some fairly reasonable and to many unreasonable proposals, all of which have been described elsewhere as 'scientific folklore'.[217] In a discussion of roads, these identifications are, in the end, unavoidable even if there is no intention to prove anything.

For the southern road of the *Tabula Peutingeriana* (Harenatium - Noviomagus - Ad Duodecimum) the most obvious choice would be the (straightest) route from Rindern to Nijmegen and from there the route south of the Waal. The implicit identifications of Harenatium with Rindern and Nijmegen with Noviomagus are not problematical, but the location of Ad Duodecimum is unknown[218] and the course of the road leading towards it remains doubtful.

Similar problems arise when the road from Nijmegen to the Rhine has to be identified. It undoubtedly crossed the Waal at Nijmegen and the direct and short connection following the route over Elst to Driel is the first to come to mind, but the direct route to Randwijk is also plausible as a main road to the west. This may all be very clear and obvious, but it implies that either Driel (in this case site 123) or Randwijk are identified with Castra Herculis and then the recorded distances cannot be made to fit. It is possible to 'solve' the problem by locating Castra Herculis elsewhere (that is, away from the Rhine) or by juggling with some category of data in another way. But no solution, including the most recent one,[219] is completely convincing in all respects. All in all, the problem of the actual Roman roads in the river area can be solved only at the level of routes, which is yet another reason to distinguish carefully between the concepts of road and route.

3.5 ANCIENT SETTLEMENT SOILS

The close relation between the large regional soil surveys, which started during World War II,[220] and the veritable boom in the discovery of archaeological findspots in the river area has often been acknowledged. It was the result of an active interest in the habitational aspects of a region by soil surveyors such as Oosting and Edelman and a corresponding interest in the results of these surveys by experts in other disciplines, for archaeology mainly represented by Modderman.[221]

The reason for this close co-operation and the impor-

216 Huissen (site 135) would be another exception if it were indeed a military settlement, which, in view of the presence of sites 126 and 194 as well as the find circumstances, is becoming increasingly unlikely.

217 Willems 1981, 168. Fortunately, the use of this term elicitated the desired response (Bogaers 1981, 172), making clear that there are indeed two opposed views on the use of *this* sort of data from historical sources.

218 But see Stolte 1959, 60–1 (Wamel) and Bogaers 1968, 158–9 (Maasbommel-Berghuizen). Both authors agree that Ad Duodecimum should – because of its name – necessarily be located at 12, not at 18 *leugae* from Nijmegen.

219 See Willems 1980a, 1980b. The identification of Castra Herculis with Meinerswijk (126) remains the most likely in view of the available (archaeological) evidence, but Driel (123) cannot be ruled out.

220 The first survey (in the Bommelerwaard) started in 1943. It is generally considered to be the 'nursery' of the Soil Survey Institute (Buringh 1949, 1; Osse 1959, 6).

221 See e.g. Oosting 1936, esp. chapter 8 and Edelman 1951 (see also e.g. Osse 1959, on both). Modderman 1955 gives a concise overview of his results.

tance of its results is that during the soil surveys a particular type of soil was recorded which only develops at places which have been, or sometimes still are, inhabited. These places were recorded as 'ancient settlement soils' on the soil maps and Modderman visited them to collect finds by which he could date the period of habitation.

The presence of these sites was not a new discovery. Ancient settlement soils are sometimes raised and the dark soil with settlement debris on those high-lying parcels which often, but not always, have names with *woerd* or *hof* suffixes,[222] had long been recognized. Dr L.J.F. Janssen, and in particular his cousin, the Reverend O.G. Heldring[223] recorded a large number of such sites and finds from them in the 19th century, and their observations have retained their value ever since. Nevertheless, only a limited part of the ancient settlement soils had been identified by these characteristics, and the soil survey added many more.

The particular characteristics of ancient settlement soils have been described in detail by various authors.[224] In addition to direct clues such as particles of charcoal, stones, or pottery, the most important properties are high to very high contents of humus and phosphate, which are both identifiable by eye. The soils are recognizable by their colour, which ranges from fairly grey (brown) to very dark and black. As a rule, the phosphate is also visible in the clay underneath, usually as greenish-yellow stains which are sometimes so numerous that they can be recognized even by a completely unexperienced eye. This phenomenon is due to the very high natural phosphate content of the clays in the river area. The enrichment caused by habitation normally results in visibility of the phosphate.[225] Because they are normally situated fairly high, the soils are well drained and because of the humus content their structure is very good. It is not surprising, therefore, that these soils belong to the agriculturally most valuable in the river area[226] which had the convenient side-effect that they were also recorded precisely during later detailed soil-surveys in which archaeology played no part.[227]

For archaeological studies, the presence of this particular type of soil in the river area has two very important and favourable implications. These concern the interpretation of findspots and the completeness of the archaeological record, which both require some further discussion.

3.5.1 The Settlement Soils as Findspots

Although there is undoubtedly still much to be investigated concerning the processes which led to the formation of ancient settlement soils (which are not all identical or, more precisely, equally well developed), one aspect is absolutely clear: if such a soil is present, then we are dealing with a site that was inhabited and thus a settlement during some time in the past.

This may seem a trivial point, but it is not. It provides a very important advantage in comparison with findspots or sites in other regions. Accidental finds, regular finds by amateurs, and full scale archaeological surveys all result in a number of findspots on maps. Findspots may be combined to sites, but unless the density reaches a certain level and/or specific types of artifacts turn up, it is very difficult to decide whether one is dealing with a settlement or not.

The presence of a settlement soil eliminates this problem completely. While the significance of a dozen sherds from a field in a coversand area is often unknown, artifacts from a field which is known to be a settlement soil are only necessary to determine the dating and (special) characteristics of that settlement: one middle-Roman sherd is sufficient evidence for such a site to be reliably interpreted as a middle-Roman settlement, an additional late-Roman sherd proves its occupation at that time, and a third, Merovingian sherd is enough to know that the site was also inhabited during the Early-Middle Ages.

222 On this subject, see Edelman 1949 and Modderman 1949, 72.

223 Especially Heldring 1838–9. For his bibliography, see Brongers 1976b, 35.

224 Fairly recent examples are Pons 1966, 34–5, Havinga 1969, 25–30, and Van der Voort/Poelman/Van Es 1979, 439–56.

225 Pers. comm. C. Blommesteijn, who is investigating settlement soils in the central river area with the geologist R. Steenbeek.

226 In the past, and occasionally again during the last war, they were used to grow tobacco (Egberts 1950, 13; Hol 1957, 155) and the fruits from orchards on settlement soils were known to be bigger and better (Egberts 1950, 19).

227 Such as the surveys for the land reallotment schemes by the STIBOKA (Van der Schans/Steeghs 1957; Zegers/Zandbergen 1958).

Of course such minimal data leave a lot of questions unanswered, for example, those regarding the precise datings or continuity of habitation, but the basic information is present and it is reliable. The latter fact was already accepted by Modderman, but it can nowadays be supported by the find-histories of dozens of settlements. The dating of settlements to a particular period by a few or only one sherd after material was first collected there is always confirmed when more material becomes available later. Particularly striking examples where Modderman's datings were later confirmed by a wealth of new finds are, for example, sites 62, 79, 93, 168, 234, 235, 250, 283, or 341, but the number of good examples of similar find-histories is very large.[228]

Of course, when more sherds or other artifacts become available, not only is the reliability of the datings increased, but there is also a greater chance that occupation during other periods can be demonstrated, especially when the original collection was very small. Because ancient settlement soils are usually not very deeply stratified and rather intensively used for agricultural purposes, surface collections do not always have to be very large in order to obtain datable evidence for all periods.

Although some of the *woerden* resemble settlement mounds like the terps, their depth rarely exceeds c. 1.5 m and most settlement sites are shallow. The find-histories also demonstrate that the so-called *opspit* phenomenon, a term that refers to the upward movement of artifacts through the soil by human or animal activities, is the process whereby at least some material from the oldest strata nearly always comes to lie on the surface. The most striking example of the *opspit* effect is the most deeply stratified site in the region, the fort in Meinerswijk (126). Among the surface finds was at least one piece, a sherd of Arretine terra sigillata, which could only have originated from the lowest excavation level, almost three metres below the surface!

Although, in general, the circumstances for collecting datable material from the surface are thus rather favourable, they do, of course, only obtain when the surface of the settlement soil is exposed. When the area is built over, used as grassland, or covered by later deposits, other methods for collecting artifacts have to be employed. In principle, there is no difference in the quality of the dating evidence in both cases, but the collections of material aquired by, for example, digging a test pit are less reliable than surface collections. The latter are more or less random samples, while the former could be very biased. Digging a series of test pits or small trenches, which would yield a more reliable overview, is the obvious solution, but that cannot always be, and thus, unfortunately, has not often been done. Cunettes for new roads, trenches for pipelines, and the digging of new or cleaning of older ditches are, however, examples of quite satisfactory alternative sources of material.

There is one final aspect of settlement soils as geological phenomena which should be especially mentioned here, because it is not directly related to settlements as archaeological phenomena. That aspect is the extent of the settlement soil. It is, of course, intuïtively obvious that settlements on sites which have been continuously or intermittently inhabited, sometimes for a millennium or longer, cannot be assumed to have all been equal in size. The settlement will have grown, shrunk, disappeared for a while, or moved somewhat. Already for these reasons, the size of a settlement soil is not indicative of the size of a settlement in any particular period. This situation can be remedied by archaeological research strategies,[229] but the geological data themselves cannot be used. Comparisons of settlements on this basis cannot be accepted. For example, Peddemors' conclusions[230] concerning settlements in the Land van Maas en Waal rely entirely on the implicit assumption that the youngest settlement should be the largest, which is an unjustifiable proposition. Even for periods where the growth of settlement-areas can generally be expected, this is not necessarily true. A good example is the settlement at De Horden near Wijk bij Duurstede, where the settlement-area apparently decreased in size from the Late-Iron Age to the Middle-Roman Period.[231]

There are also other reasons why there can be no direct relation between the size of settlements and settlement soils. These derive from the conditions in which a settlement soil was formed and those in which it can be recog-

228 The relevant data have been stored in the CAA at the ROB. A comparison of the data published by Modderman with those included in the catalogue (chapter 5) also reveals other examples.

229 Including anything from carefully recording the distributions of surface finds to a full scale excavation.

230 Peddemors 1978, 26.

231 See Van Es, in Van der Voort/Poelman/Van Es 1979, 439.

nized. The implications are, however, more directly relevant when settlement soils are considered at the regional level.

3.5.2 Settlement Soils and the Completeness of the Archaeological Record

It is obvious that the presence of ancient settlement soils has, in principle, very favourable implications at the regional level. In 1949 Modderman,[232] who is not exactly known for any hasty or ill-founded remarks, wrote that 'for, however it may be, after the soil survey it will not be possible to discover many new settlements. We have more or less reached the limit'. Although he proceeded to specify at least one reason why new settlements might still be discovered (coverage by younger sediments which are thicker than the depth of borings in soil surveys), this is a bold statement. It implies that in the uneroded part of the region most settlements are already known and that, in areas where the borings were deep enough, the archaeological record for settlement sites is complete.

This would, of course, be a situation unparalleled in archaeological studies. Whether it indeed fits the facts depends mainly on two technical conditions. First, as already mentioned above, a settlement soil necessarily implies a settlement, but when the total number of settlements is considered, the reverse also has to be true: no settlement soil, no settlement. Second, the soil surveyors must have done a proper job in their recording of settlement soils. Random errors, caused by incidentally missing one or even a few settlements, would not be a problem, but the systematic bias introduced by overlooking particular sorts of settlements, such as those covered by later deposits of more than 1.20 m thick, cannot be disregarded.

The first of these conditions is the most crucial, because if there are settlements where habitation has not left any settlement soil, then the river area loses its unique quality as a region where all uneroded settlements are easily traceable. At this point, it is important to realize that a settlement is a site which is, at least in principle, permanently inhabited.[233] Consequently, the settlements in question here can be expected to have existed normally at least for decades, but more often considerably longer. It is also important to realize that the phenomenon of an ancient settlement soil should be identifiably by sight. It is, of course, possible to rely on chemical instead of visual data, but all soil surveys in the river area have relied on the visual identification of settlement soils. The possibility that settlement soils which cannot be seen in borings can be identified by analysis of the phosphate content of samples is only relevant in the sense that 'invisible' settlements can theoretically[234] still be traced. The available data on visible settlement soils are inadequate if there should be invisible ones.

The recent chemical, geological, archaeological, and soil studies of the settlement soil at De Horden near Wijk bij Duurstede[235] have provided some insight into the problem. De Horden would not have escaped destruction if it had not been mapped as a settlement soil: it was covered by later sediments of at least 0.5 m thick and the occupation layer was not reached by the plough. The characteristic soil was mapped by three independent surveys with virtually identical results. Chemical analysis proved that the interpretation of the greenish-yellow stains as phosphate stains is correct and even that there are additional reddish stains which also indicate phosphate concentrations. It could, however, also be demonstrated that visual and chemical determinations of the relative amount of phosphate did not coincide and that some phosphate concentrations in the subsoil could not be seen. The excavation results also showed that the inhabited area was definitely larger than the area indicated by the visual interpretation of phosphates. It was concluded that that area could only be seen as a minimum estimate for the total size of the area that was once inhabited and that it indicated especially those parts that had been the most intensively used.

These results are rather discouraging, because apparently the relatively shortly and/or less densely settled parts could not be traced by borings. This could imply that where these conditions prevailed over an entire settlement, such a settlement would not be noticed. There

232 Modderman 1949, 68.

233 Intermittent (e.g. seasonal) occupation could also be included, but single activity sites such as herding, harvesting, or hunting stations, etc. would, if they actually occurred in the region, be considered a category of 'isolated find' (see p. 18).

234 In practice, the time and effort involved and the costs of a complete survey will make this rather difficult. It could be possible to survey smaller areas by cheap and quick methods (see Eidt 1977 and Craddock/Gurney/Hughes/Pryor 1982), providing the high natural phosphate content of river area clays allows their application.

235 Van der Voort/Poelman/Van Es 1979.

are, however, several circumstances which should also be considered in this respect.

First, the 'invisible' parts of the inhabited area at De Horden belong primarily to prehistoric settlements. Second, the invisible parts of the Roman settlement were definitely not those of the houses, but areas where other activities took place.[236] In addition to these significant nuances Van der Voort *c.s.* are, curiously enough, only concerned with the phosphate-stain characteristic of ancient settlement soils which is important, but not the only one. Symptomatic is the geological profile of almost 1 km,[237] in which a fossil habitation layer is indicated along its entire length. This designation is misleading, because the layer consists of a fossil habitation layer (the actual settlement soil) and a fossil vegetation layer (*laklaag*),[238] which gradually change into each other.[239]

Havinga's study of part of the Betuwe is also important in this respect. He remarks[240] that it is sometimes difficult to distinguish fossil vegetation from habitation layers, but that it is still possible to do so by using a variety of indicators. Even more interesting is the fact that his difficulties were restricted to the settlement soils attributed (on stratigraphical as well as archaeological grounds) to the Neolithic and the Bronze Age. These soils are also characterized by the fact that phosphate stains are only rarely visible. It is no coincidence that Havinga mapped a considerable number of settlement soils which had not been noticed during previous surveys,[241] for only a very detailed survey reveals the older settlement soils. Whether their limited visibility is caused by their age, the nature of the occupation, or its duration, is a question which has yet to be answered.[242]

In general it seems, however, that for the Late-Iron Age, Roman Period, and later settlements, there is little risk that they, or at least the location of their core, cannot be seen in borings. Final proof of this can only be obtained after an extremely detailed survey by various means of the region surveyed by Havinga, but on the basis of the present evidence it is the most plausible assumption. Only very short-lived settlements cannot be assumed to be visible, unless by chance. 'Very short' can, however, not be defined. It might mean a generation or less,[243] but virtually all settlements which could be called relatively short-lived (for example, only occupied during the 2nd century or so) still have settlement soils.

There is only one case, site 99, where finds point to a settlement (with stone buildings) which, in principle, may have existed for only half a century or even less and where no settlement soil has been observed. Although in this case, the latter need not be doubted, settlement traces are also absent and the interpretation as a settlement, which was based on the number of finds, could be mistaken. Pending an excavation, nothing can be said with certainty. It is possible that we are dealing here with an unsuccessful settlement, in this case perhaps a failure to establish a villa, but the finds may have been in secondary position, the actual settlement being close by.

There are a few other sites which are considered to be definitely settlements and which cannot be assumed to have been occupied for a short time, but where a settlement soil is nevertheless absent. These are 141, 439, and possibly 516. This list could be augmented by several sites which could be settlements, but they probably all belong to the somewhat larger category of settlements where no survey recorded a settlement soil but where one was definitely shown to be present, either in borings carried out by the present author[244] or in a profile during the discovery of the finds.[245]

These facts lead to the second problem, namely, the conclusion that quite a few settlement soils appear to have been overlooked during the soil surveys, even though most areas have been surveyed two or three times for different purposes. This is undoubtedly caused primarily by the density of the borings, generally 1 or 2 per ha.[246] Only in the western part of the Betuwe (approximately west of the line Randwijk-Andelst),[247] is the den-

236 Van Es, in Van der Voort/Poelman/Van Es 1979, 439.
237 *Op. cit.*, *Abb.* 4.
238 See above, p. 30 and *op. cit*, 447.
239 A general illustration of their relation is provided by Pons 1957, 46, fig. 31.
240 Havinga 1969, 27.
241 Egberts 1950; Van der Schans/Steeghs 1957.
242 See also Craddock/Gurney/Hughes/Pryor 1982, and various remarks on the subject in Zölitz 1980.
243 Van der Voort/Poelman/Van Es 1979, 439.
244 Which implies that they were clearly recognizable.
245 When settlement soils are present but not indicated on any of the available soil maps, this is always mentioned in the catalogue.
246 This should imply an average of 2–3 STIBOKA-borings in each 100 × 100 m square for the entire river area.
247 The area was investigated by the Dept. of Regional Soil Science of the Agricultural University, Wageningen (Havinga 1969, Op 't Hof 1970, Havinga/Op 't Hof 1975).

sity of the borings very much higher, with a minimum of 10 borings per ha. This has led to a considerable increase in the total number of settlement sites compared to those known from previous surveys, and to at least some increase in the number of settlements dating to the Roman Period or later.

A second cause of this increase could be that some of the less well-developed settlement soils were not recognized in the borings, but in general this does not seem to be very important. Otherwise, it is not understandable why Pons, for example, who paid special attention to settlement soils, did not record fairly large and well-developed examples such as site 275 and some others. Fortunately, this means that at least Roman and later settlement soils were not noticed during the surveys for arbitrary reasons. The only systematic error could be that no really large and comparatively many of the very small settlement soils have gone unnoticed. This, however, says little about the size of the settlements involved.

In conclusion, it can be said that Modderman's statement in 1949 was essentially correct, although in some ways too optimistic. He definitely underestimated the number of settlements which remained to be found, and it is clear that in parts of the Betuwe and the Land van Maas en Waal, and also in the Liemers, the Ooijpolder, and the Duffelt, some of them still await discovery. In the last two areas, there is the additional problem of very thick post-Roman deposits. Nevertheless, the number of known settlements on river deposits is much larger than even the most detailed archaeological survey could ever produce, and we have indeed 'more or less reached the limit'.

It is difficult to estimate the percentage of missing settlements – apart from those which have been eroded – without detailed soil and archaeological surveys in control areas. A comparison between Havinga's results and those of the STIBOKA[248] shows that for areas with 1–3 borings per ha. almost 20% of the settlement soils may have been missed. This percentage has been reduced by numerous discoveries of new sites through other means over the past three decades, mainly thanks to the activities of zealous amateur archaeologists and changes in the use of the land. Additional finds are still being reported at the rate of one or two sites per year, but the influence of the law of diminishing returns is becoming noticeable. For the entire area of Holocene river deposits, an average figure of 10 to 15% for undiscovered settlements would seem to be in the correct order of magnitude. In practice, it may vary from approximately 0% in the western Overbetuwe to perhaps as much as 50% in the Duffelt and 100% in the Ooijpolder. An archaeological survey in the central part of the Dutch river area, the Kromme Rijn region, has shown that 40% of the ancient settlement soils had been missed, even though the density of the borings was 5 per ha. The soil survey in that area is, however, not comparable to Havinga's detailed work or to the STIBOKA surveys in the eastern river area.[249] The percentage is, therefore, also incomparable and no reason to reconsider the general percentage of 10 to 15% for the eastern river area.

It clearly shows, however, that the type of soil survey and the geological situation in a particular area are very important for a correct evaluation of the evidence. The geological situation in the Ooijpolder, for example, is such that the region should be investigated in the future by a soil survey with borings to at least 2 m, and preferably at least 3–5 per ha. In addition, as already mentioned in chapter 1, the entire eastern river area should be sampled to test the reliability of the present assumptions and conclusions. This can be done by randomly selecting a small series of areas of e.g. 0.5 km^2 where a detailed boring program with 5–10 borings per ha. is carried out (250–500 borings per area). These should also be checked for phosphate content by the quick and cheap chemical method proposed by Craddock a.o.[250] Only by this or a comparable sampling design[251] will it be possible to asses the reliability of the data on settlement soils. Moreover, if our current notions should prove to be wrong and the data in some way biased by missing some type(s) of sites, such a technique will at least allow a prediction of their incidence in the area of Holocene deposits.

248 Havinga 1969 and the data reported in Egberts 1950, Van der Schans/Steeghs 1957, and STIBOKA 1973.

249 I am grateful to C. Blommesteijn for providing the information. The Kromme Rijn survey was carried out by students and not by experienced STIBOKA field technicians and pedologists. It is significant that of the total number of sites originally reported, 30% later proved to be no settlement soil. This situation has never been encountered in the eastern river area.

250 See note 234.

251 See for example Flannery (ed.) 1976, 131–60.

4 THE SITES: CHRONOLOGY AND RELATION TO THE DISTRIBUTION MAPS

4.1 CHRONOLOGY OF THE SITES

Fig. 23 is primarily intended to present the chronological data concerning the sites. In order to facilitate the use of this table, several other data have been added. These are, first, the topographical co-ordinates defining the 1 x 1 km square in which the site is located on Appendices 1–5. Second, fig. 23 provides a general evaluation of the datable finds, collected or excavated at the settlement sites. This is intended to give an impression of the quality and quantity of the material on which, among others, the chronology is based. Unfortunately, it proved impossible to devise a wholly unambiguous indicator. A choice was made therefore, to indicate either excavation or, in the other cases, the variability of the material. The latter is both an abstraction from and an addition to the information provided by the catalogue.[1]

Generally, the symbols '– –' or '–' imply that in these cases the chronology is uncertain to a larger degree than in others. However, settlements with an abundance of native material or with a large number of sherds from only a few types of wheel-turned pottery will also be marked as '– –' or '–', although their dating is probably more secure. On the other hand, the find-indicators do incorporate undeterminable but still datable finds. For example, a wall-sherd of 4th-century Mayen ware or a 6-ribbed Augustan flagon-handle will not be mentioned separately in the catalogue because they cannot be described typologically. But, when no rimsherds of the same wares have been found, they have been counted as separate types and, of course, contribute to the dating of the settlement. The same applies to other cases when not only the type but also the ware provide useful information (e.g. terra sigillata, colour-coated ware, etc.).

In general, except for settlements with only native material, the indicators provided give a fairly accurate assessment of the chronological information contained in the finds. From the find-history of a number of settlements[2] it may be concluded that only settlements in the '+' categories permit completely reliable conclusions to be drawn. In fig. 23, the type of site is also indicated, in more or less general terms. For detailed information concerning the character of a settlement in each of the periods discussed, it is necessary to consult Appendices 1–5 and paragraph 4.2, as well as the (forthcoming) chapter 8 on the typology of the settlements in the second part of this study. Obviously, settlements which (are supposed to) have certain characteristics in one period may (appear to) have changed in another. For this reason, a distinction is only made between civil and military occupation, which automatically results in a third category, namely, settlements which are probably or possibly military camps of some sort or which were so during part of their occupation history.

For the other sites, only the basic types have been differentiated. These are cemeteries (including single graves), hoards, and isolated finds.[3]

The chronology of the sites is indicated by horizontal black bars, interrupted bars, and open bars. This is partly intended to express explicitly – as far as possible – the degree of certainty in the dating of each site, partly what sort of finds it is based on, and also to convey a sense of continuity or discontinuity in each case. A period covered by the wheel-turned and more (Roman) or less (Merovingian) precisely datable pottery is always represented by a black bar. The same applies to other datable finds, such as fibulae, coins (except in settlements), stamped tiles, glass, etc., but their contribution is a relatively minor one.

Interrupted bars indicate uncertainty, which may be of four different kinds: doubtful attribution of some finds to a site in a few cases, sometimes an uncertain dating of the finds[4], a questionable determination of the material, and, finally, self-imposed restrictions on the use of uninterrupted bars.

The latter uncertainty is caused by the dating of some comparatively long-lived types of pottery; for example, the coarse ware cooking-pots Stuart 201 or 210 date from Augustan times to well into the 2nd century or even later. Our study has shown, however, that relatively short-lived wheel-turned pottery types dated to the Early-Roman Period (12 BC-C.AD 50) occur only very rarely in ordinary sites in the area. Even from AD 50-100

1 Unless they have been excavated, this could not be done for the sites in Brabant (see p. 129, note 25).

2 See chapter 3, 72.

3 These are not just stray finds, but also include deliberately deposited materials. See chapter 1, 18.

4 Especially in the case of '– –' settlements, where not or hardly suitable material had to be used for dating purposes in many cases.

this sort of pottery is still unusual. Therefore, it would be unwise to cover the period from 12 BC-AD 100 by an uninterrupted bar when a site has only yielded Stuart's types 201 and 210 and no other more positive indicators for that period. In cases like this, the Early-Roman Period is not covered by a bar at all, or by an open bar when native pottery is present, while the trajectory from AD 50-100 is indicated by an interrupted bar. As a result, the Early-Roman Period is only indicated by a black bar when definitely appropriate finds are present. Comparable problems arise when the end of the occupation of a settlement has to be determined. In many cases this is fairly easy. When Niederbieber types occur, the end is arbitrarily fixed at AD 270. When they do not, the decision depends on the other finds. In cases where there is a lot of pottery and/or a fair amount of variability (certainly the '□' and '+' categories, sometimes the '–' one also), the occupation is assumed to have ended earlier than AD 270. In other cases this cannot be assumed and therefore an interrupted bar will indicate that occupation may have continued until AD 270.

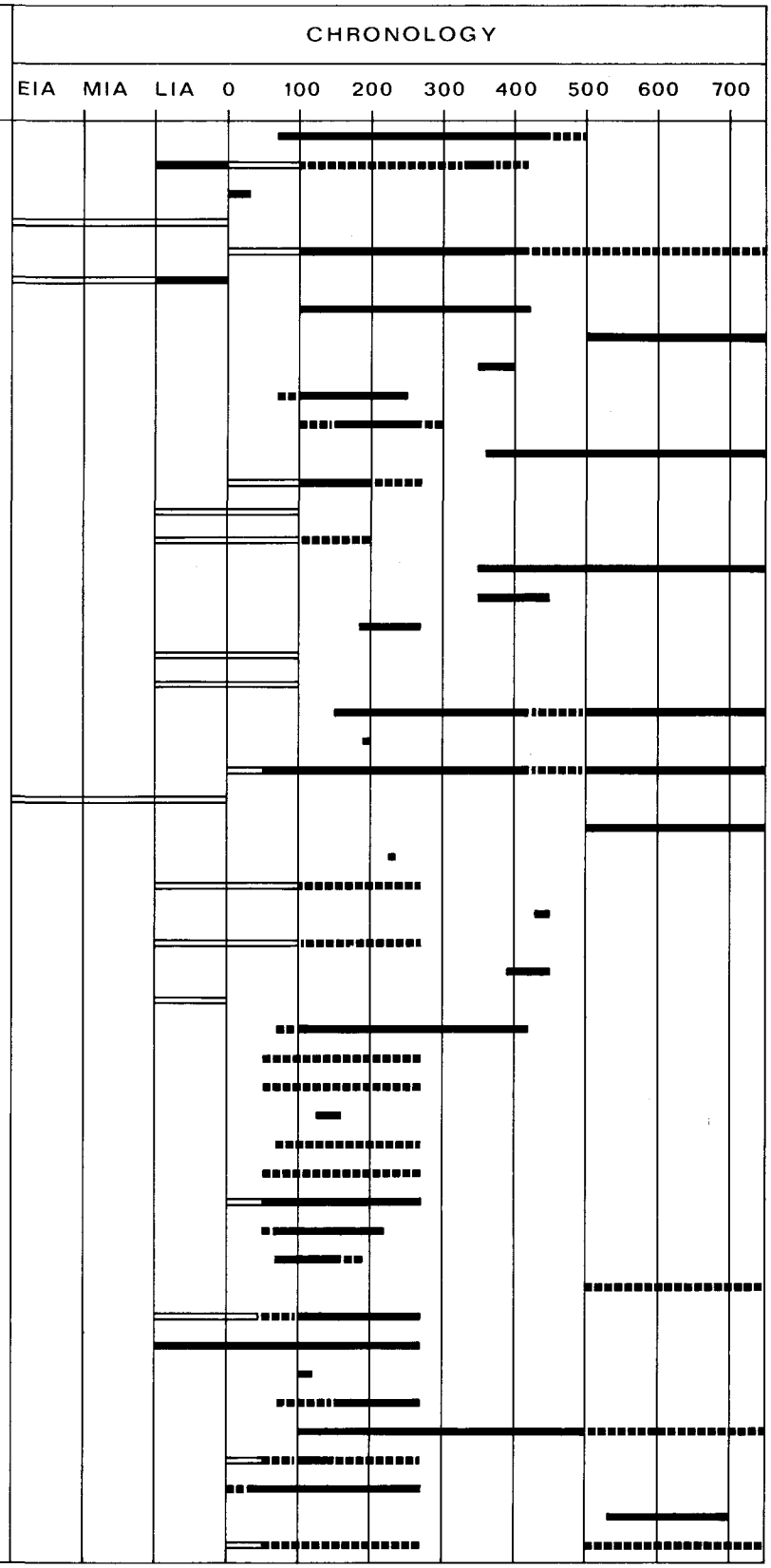
The Late-Roman Period lasts, by definition, from AD 270-c.425. Because there is as yet no firm typological 'grip' on late-3rd-century material, the period between 270 and the beginning of the 4th century is not indicated by an interrupted bar but by a continuous one in cases when 4th-century material occurs on the site. The same could be done for the earliest Middle Ages, the 5th century, when Merovingian finds are present. Because it turned out that quite a number of finds in the eastern river area could be dated to the 5th century, a continuous bar is only used when they are present. In fact, this situation resembles that of the Early-Roman Period which is also only indicated by a continuous bar when appropriate finds are available.

All remarks so far apply to wheel-turned pottery and other datable finds. In addition to that, we also have the native pottery, which is always indicated by an open bar. As is discussed in chapter 6.3, it is extremely difficult and often impossible to distinguish between late-Iron Age and later native wares. Consequently, in the latter case an open bar will often cover the Late-Iron Age and continue into the Roman Period in so far as no other bar is present, resulting in a probable early-Roman settlement on Appendix 2. When no other material is present at all, this bar will stop at the more or less arbitrarily chosen point of AD 100[5] unless, of course, the native ware has been ascribed exclusively to the Late-Iron Age. Strictly speaking, such sites should have appeared as possible Middle-Roman Period settlements on Appendix 3, but because the absence of the all-pervading wheel-turned pottery in this period is judged to be more important, they do not. It should be noted that, in quite a few instances, the use of an open bar for datings based on native pottery does not at all mean a less reliable dating. Many determiniations as late-Iron Age pottery have been confirmed by finds of La Tène glass bracelets and, occasionally, by other late-Iron Age artifacts from the same site. When such items occur, this period is, of course, no longer covered by an open but by a black bar.

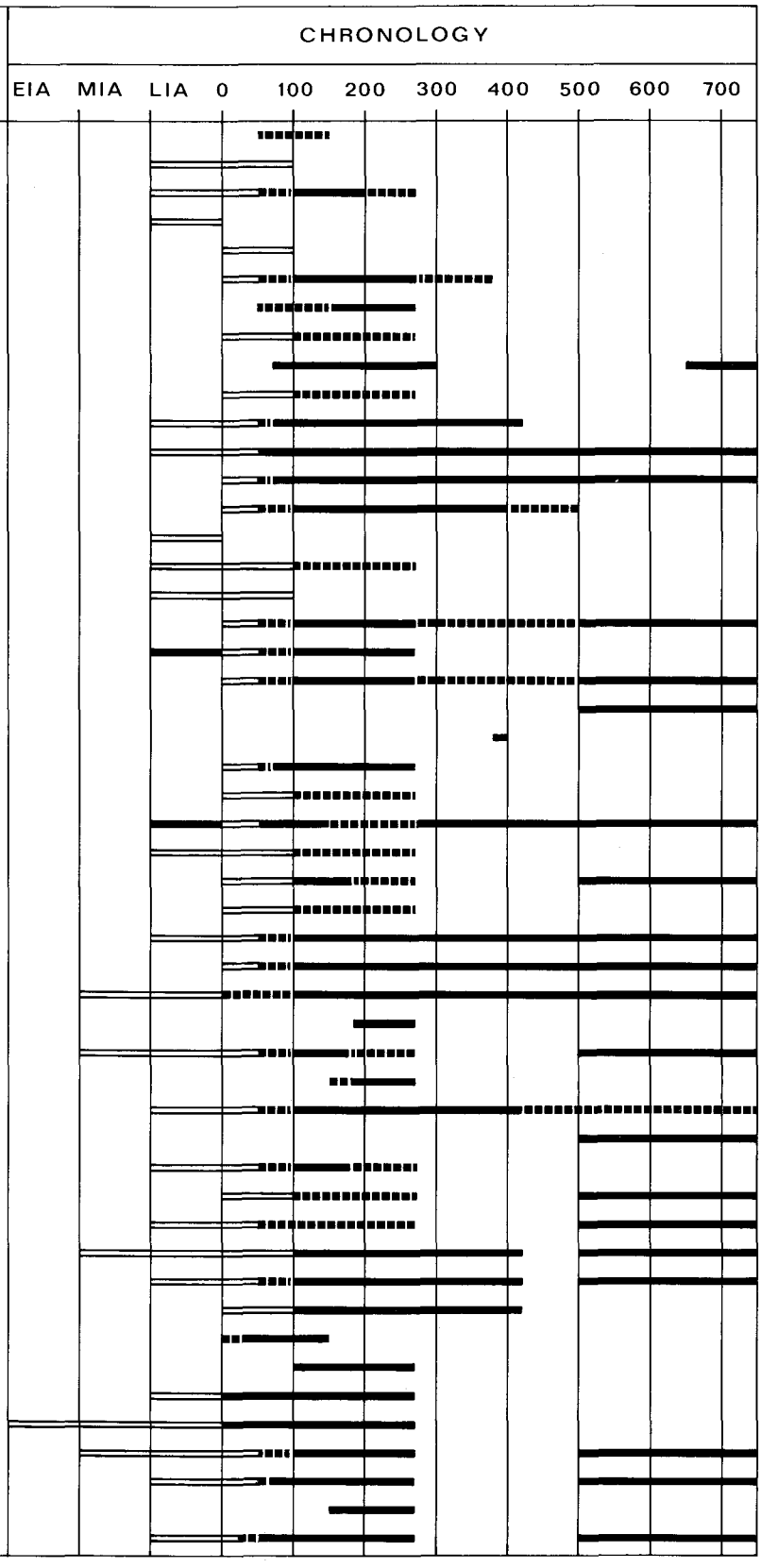
5 For a motivation, see chapter 6, note 202.

Fig. 23 Chronology of the sites. On the tables, the following symbols are employed for the finds from *settlement* sites:
– – Fewer than 5 sherds of datable pottery of different types or other objects except coins
– 5–10 sherds of datable pottery of different types or other objects except coins
□ 10–15 sherds of datable pottery of different types or other objects except coins
+ 15 or more sherds of datable pottery of different types or other objects except coins
+ + Excavation
These symbols are, of course, omitted for sites which are not settlements. The chronology is indicated as follows:
■ Period covered by datable material: wheel-turned pottery or other objects
■ ■ Idem, uncertain
▭ Period covered by hand-made native pottery

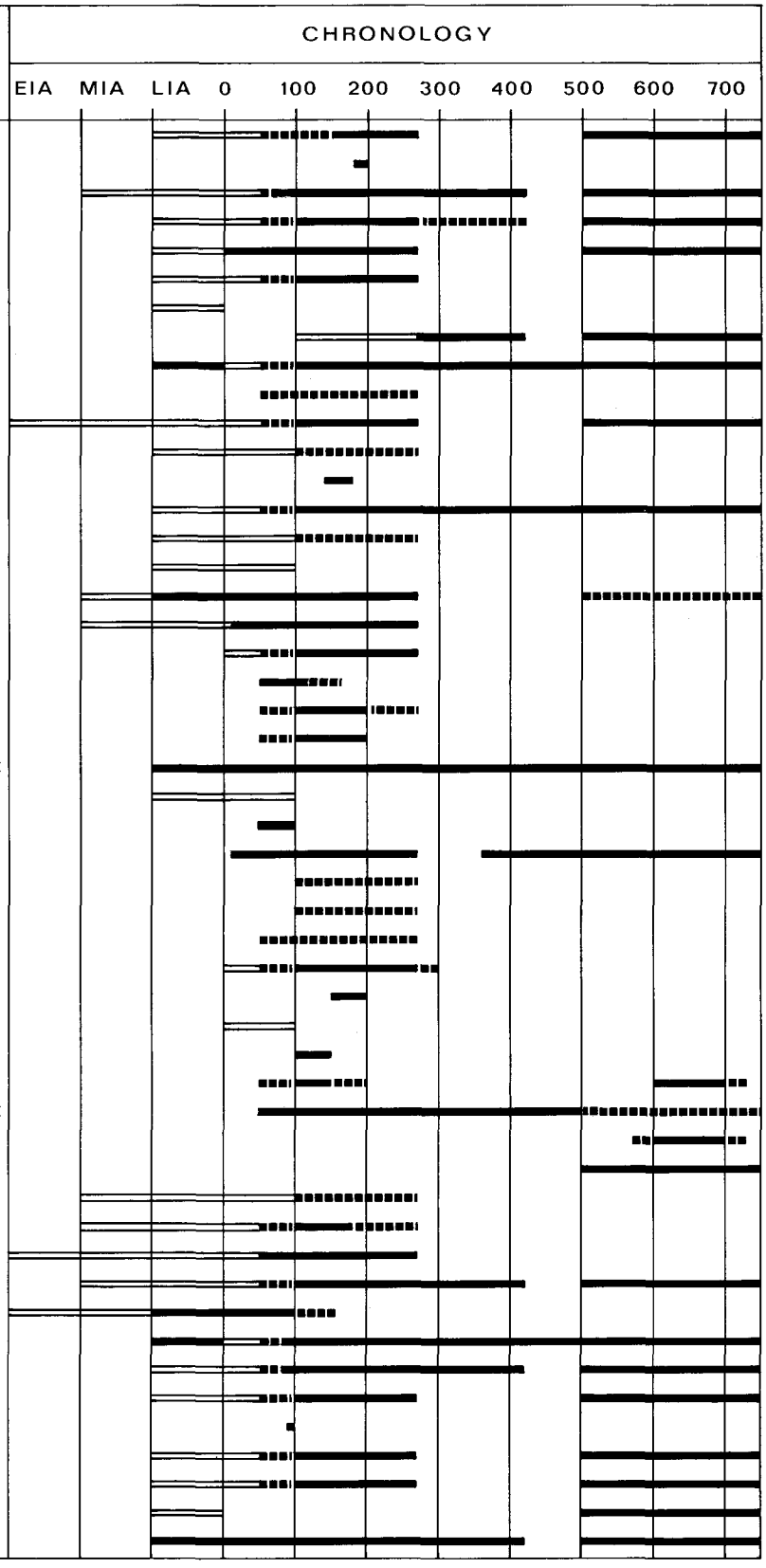
NR.	GRID	FINDS	TYPE OF SITE
1	172/450	+ +	settlement
2	172/449	– –	settlement (LIA only)
3	172/448	– –	settlement
4	172/448	+ +	settlement
5	173/448	□	settlement
6	170/446	– –	settlement
7	174/445	+ +	settlement
8	174/445	– –	settlement
9	169/443		hoard
10	166/441		cemetery?
11	166/441	–	settlement
12	166/441		cemetery
13	166/442	– –	settlement?
14	169/441	– –	settlement?
15	169/441	– –	settlement?
16	175/443		cemetery
17	175/443	+ +	settlement
18a	175/441		isolated find
19	174/442	– –	settlement
20	176/444	– –	settlement
21	178/445	+ +	settlement
22	183/445		hoard
23	188/443	+	settlement
24	188/444	– –	settlement
25	189/445		cemetery?
26	190/443		hoard
27	190/443	– –	settlement?
28	194/445		hoard
29	195/444	– –	settlement
30	195/444		hoard
31	195/445	– –	settlement
32	195/443		isolated find
33	198/446		cemetery?
34	166/438	– –	settlement
35	166/438		cemetery
36	167/438		isolated find
37	167/439	NO	military settlement?
38	167/438	+ +	settlement
39	167/438		cemetery
40	167/438		cemetery
41	167/438		isolated find
42	168/437	–	settlement
43	167/436	+	settlement
44	166/436		cemetery
45	166/436	– –	settlement
46	167/435	–	settlement
47	169/438	–	settlement
48	170/438	+	settlement
49	170/438		cemetery?
50	171/438	– –	settlement



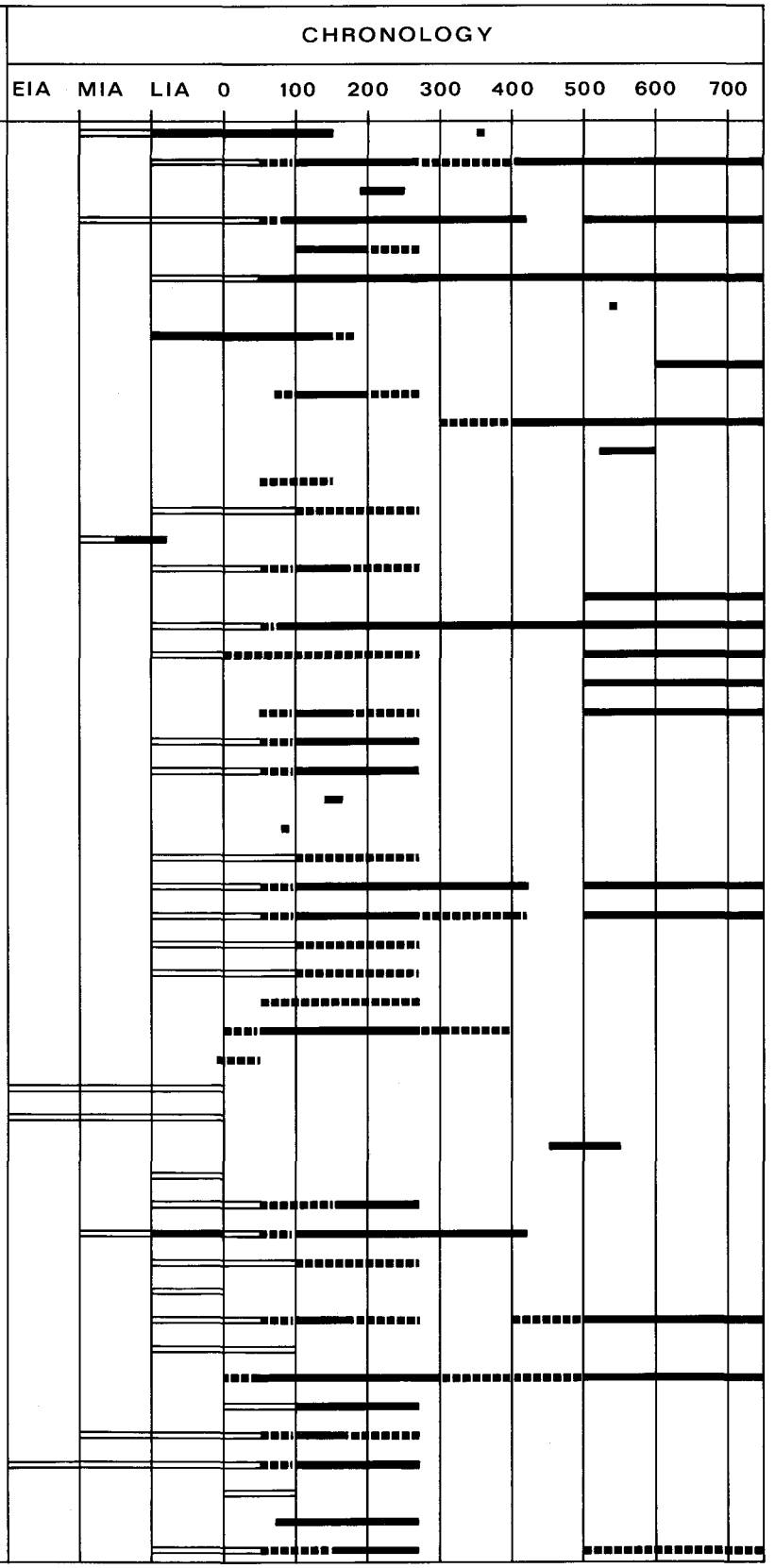
NR.	GRID	FINDS	TYPE OF SITE
51	171/437		isolated find
52	172/437	– –	settlement
53	173/437	–	settlement
54	173/437	– –	settlement
55	173/438	– –	settlement
56	171/435	–	settlement
57	173/435	– –	settlement?
58	174/435	– –	settlement?
59	174/436	+	settlement (RA only)
60	175/434	– –	settlement?
61	175/435	–	settlement
62	174/438	+	settlement
63	175/438	□	settlement
64	175/437	–	settlement
65	175/440	– –	settlement
66	175/438	– –	settlement
67	175/438	– –	settlement
68	176/438	– –	settlement
69	176/438	– –	settlement
70	177/438	–	settlement
71	177/440	– –	settlement
72	178/440		isolated find
73	178/440	□	settlement
74	178/438	– –	settlement
75	178/438	+ +	settlement
76	178/437	– –	settlement
77	177/436	–	settlement
78	179/436	– –	settlement
79	179/436	+	settlement
80	179/435	□	settlement
81	178/435	+	settlement
82	177/435		cemetery?
83	179/434	–	settlement
84	180/434		cemetery?
85	180/435	□	settlement
86	180/435		cemetery?
87	181/434	– –	settlement
88	182/434	– –	settlement
89	182/433	– –	settlement
90	183/433	+	settlement
91	184/435	–	settlement
92	182/436	– –	settlement
93	180/439	+ +	settlement
94	180/439		isolated find
95	181/439	□	settlement
96	182/438	+	settlement
97	182/438	□	settlement
98	184/436	□	settlement
99	185/436	□	settlement
100	185/434	–	settlement



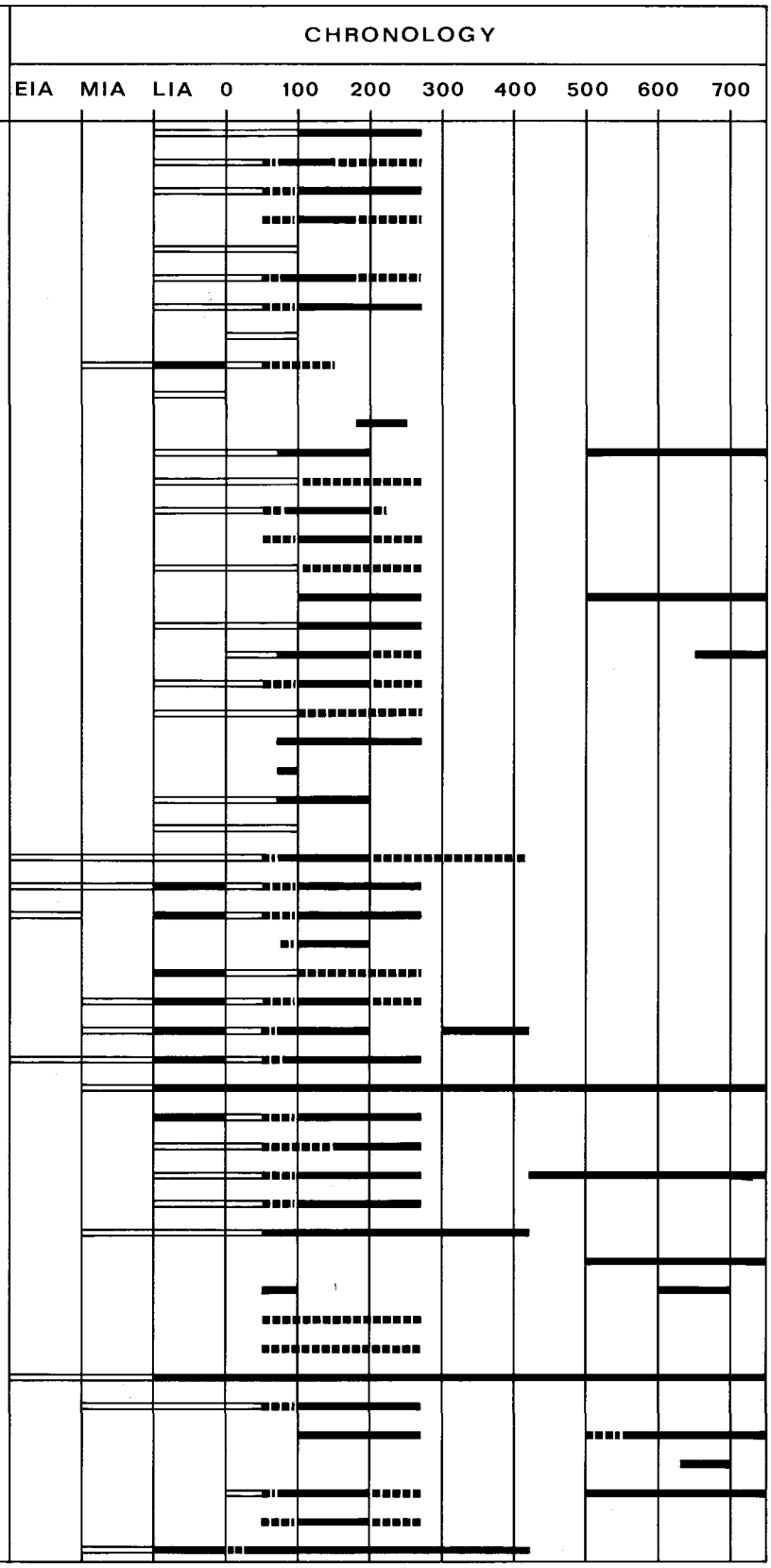
NR.	GRID	FINDS	TYPE OF SITE
101	185/436	– –	settlement
102	185/436		isolated find
103	186/436	–	settlement
104	186/436	–	settlement
105	186/436	+ +	settlement
106	187/436	–	settlement
107	187/437	– –	settlement
108	186/437	– –	settlement
109	185/438	+ +	settlement
110	186/438		cemetery?
111	187/438	□	settlement
112	185/439	– –	settlement
113	186/439		isolated find
114	186/439	□	settlement
115	187/439	– –	settlement
116	185/440	– –	settlement
117	185/441	+	military settlement?
118	184/441	+	settlement
119	184/441	□	settlement
120	183/441		isolated find
121	182/441		isolated find
122	182/441	– –	settlement
123	184/441	+	(military?) settlement
124	184/441	– –	settlement?
125	185/442		isolated find
126	188/442	+ +	military settlement
127	188/441		isolated find
128	192/441		isolated find
129	191/440		isolated find
130	191/439	–	settlement
131	191/439		cemetery
132	191/439	– –	settlement
133	192/439	– –	settlement?
134	192/438		cemetery
135	193/438	+ +	(military?) settlement
136	194/436		cemetery
137	194/436	– –	settlement
138	193/435	– –	settlement
139	192/435	– –	settlement
140	192/436	□	settlement
141	191/436	–	settlement
142	190/436	–	settlement
143	190/435	+ +	settlement
144	189/435	□	settlement
145	188/436	–	settlement
146	188/436		isolated find
147	188/436	□	settlement
148	187/434	–	settlement
149	188/433	–	settlement
150	188/433	+ +	settlement



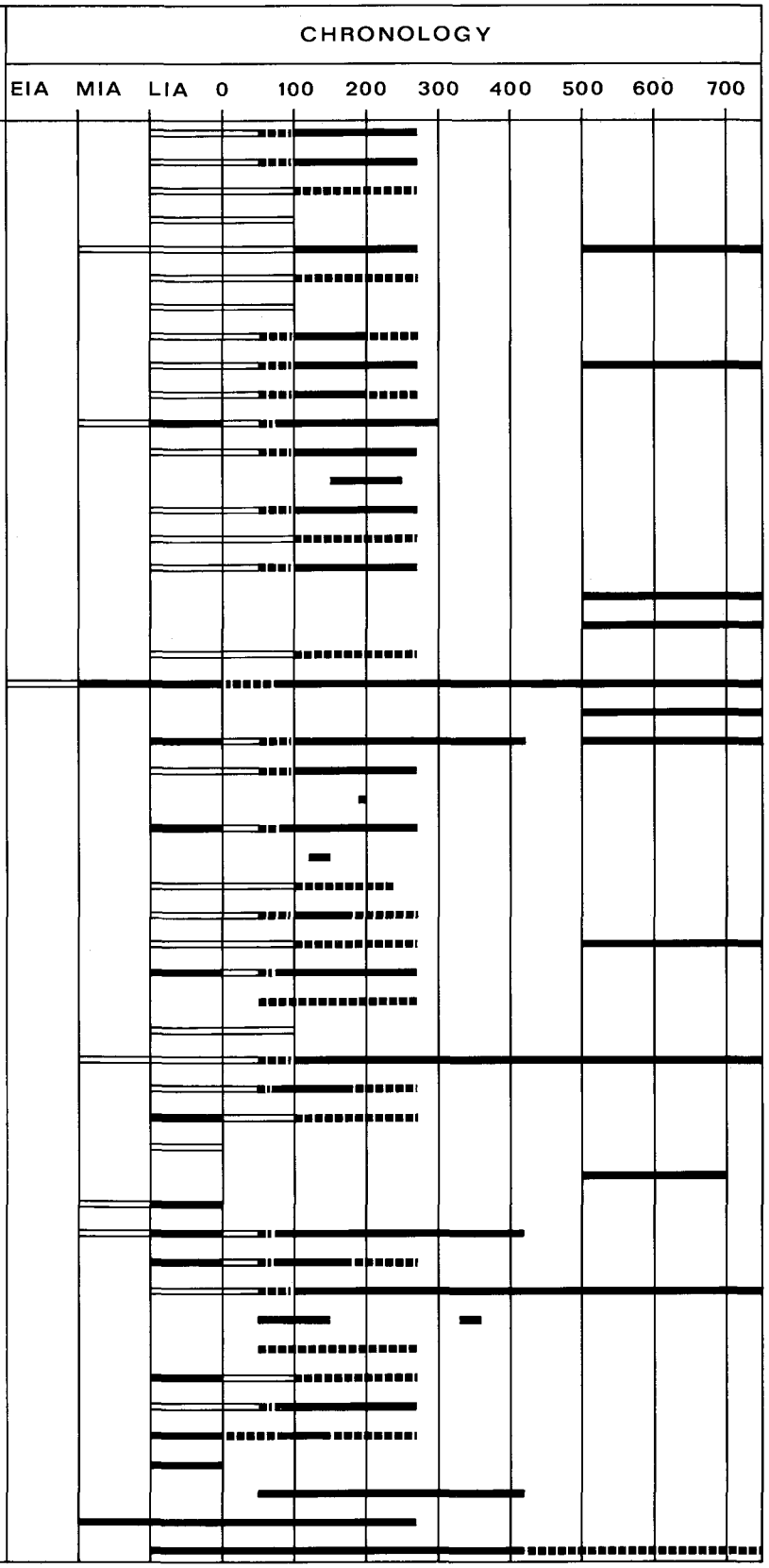
NR.	GRID	FINDS	TYPE OF SITE
151	188/433	+ +	settlement
152	186/432	–	settlement
153	186/432		cemetery
154	185/432	□	settlement
155	186/431		cemetery?
156	187/430	– –	settlement
157	188/430		isolated find
158	188/430	+ +	settlement
159	188/430		cemetery
160	188/429	– –	settlement?
161	189/433	– –	settlement
162	190/433		cemetery
163	189/434		cemetery
164	189/434	– –	settlement
165	189/434	+ +	kiln-site
166	190/434	– –	settlement
167	181/434	– –	settlement
168	191/434	+	settlement
169	192/434	– –	settlement
170	194/433	– –	settlement
171	194/433	– –	settlement
172	195/435	–	settlement
173	195/434	–	settlement
174	195/433		cemetery
175	197/434		isolated find
176	197/433	– –	settlement
177	196/433	–	settlement
178	195/432	–	settlement
179	195/431	– –	settlement
180	199/434	– –	settlement
181	200/432		isolated find
182	203/431	+ +	military settlement
183	204/429	NO	military settlement?
184	207/435	– –	settlement
185	204/438	– –	settlement
186	206/440		cemetery
187	206/440	– –	settlement
188	204/435	– –	settlement
189	202/437	–	settlement
190	202/439	– –	settlement
191	200/438	– –	settlement
192	199/438	□	settlement
193	198/437	– –	settlement
194	196/436	+	military settlement?
195	195/439	– –	settlement
196	197/439	□	settlement
197	198/440	–	settlement
198	197/441	– –	settlement
199	199/444		isolated find
200	166/432	–	settlement



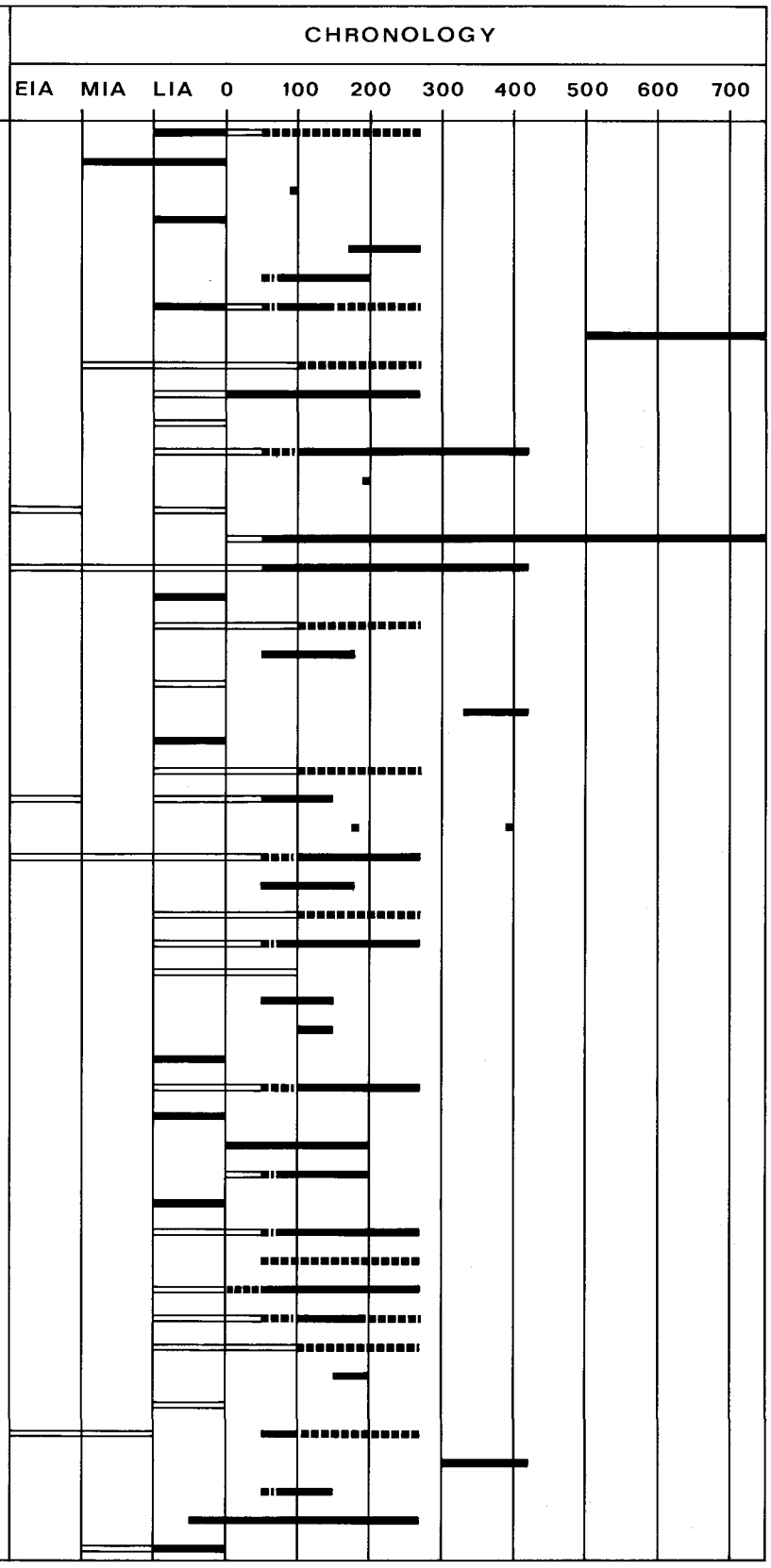
NR.	GRID	FINDS	TYPE OF SITE
201	166/432	– –	settlement
202	168/432	– –	settlement
203	167/431	– –	settlement
204	167/431	– –	settlement
205	168/431	– –	settlement
206	168/431	–	settlement
207	168/432	– –	settlement
208	169/432		cemetery?
209	168/433	–	settlement
210	169/433	– –	settlement
211	169/433		isolated find
212	170/433	+	settlement
213	170/433	– –	settlement
214	170/433	+ +	settlement
215	169/432	– –	settlement
216	169/432	– –	settlement
217	170/432		cemetery
218	171/432	– –	settlement
219	174/433	□	settlement
220	173/433	–	settlement
221	174/433	– –	settlement
222	176/433	+ +	settlement
223	176/433	– –	settlement
224	176/432	– –	settlement
225	176/431	– –	settlement
226	177/431	–	settlement
227	177/430	+	settlement
228	177/430	– –	settlement
229	177/430	– –	settlement?
230	177/430	– –	settlement
231	177/430		isolated find
232	178/430	+ +	settlement
233	178/429	□	settlement
234	178/429	+	settlement
235	179/429	+	settlement
236	179/430	– –	settlement
237	179/430	–	settlement
238	179/430	– –	settlement
239	180/431	+	settlement
240	179/431	–	settlement
241	179/431		cemetery?
242	180/431		isolated find
243	180/431		isolated find
244	180/429	+	settlement
245	181/430	+	settlement
246	181/430	+ +	settlement
247	181/430		cemetery
248	181/430	□	settlement
249	181/430	– –	settlement
250	182/429	+	settlement



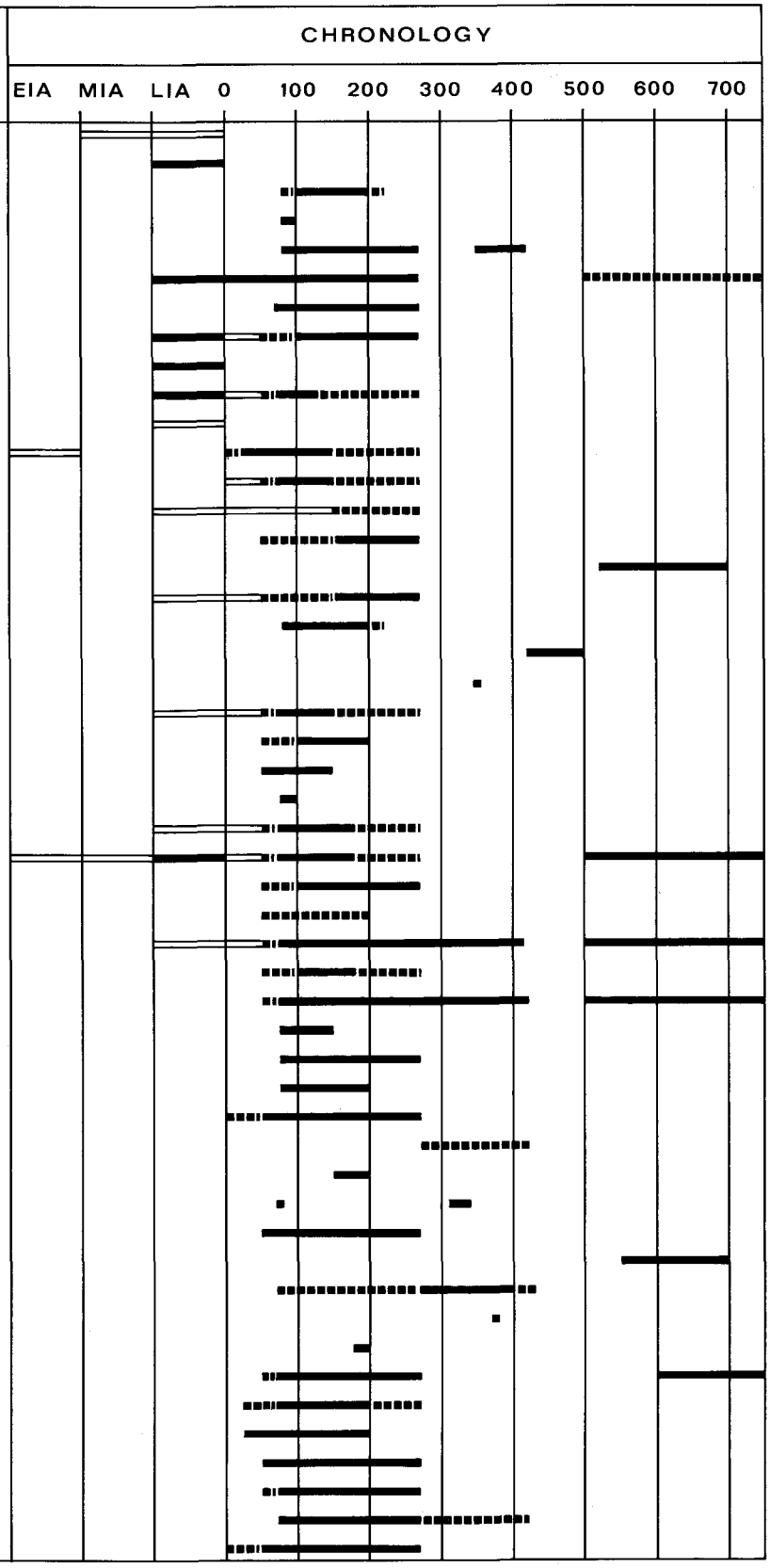
NR.	GRID	FINDS	TYPE OF SITE
251	166/428	–	settlement
252	166/428	–	settlement
253	167/428	– –	settlement
254	167/429	– –	settlement
255	168/428	– –	settlement
256	169/428	– –	settlement
257	170/427	– –	settlement
258	170/430	– –	settlement
259	170/429	–	settlement
260	171/429	– –	settlement
261	172/429	□	settlement
262	170/431	– –	settlement
263	170/430		cemetery
264	171/430	– –	settlement
265	172/431	– –	settlement
266	172/430	– –	settlement
267	172/430		cemetery
268	173/430	– –	settlement
269	174/430	– –	settlement
270	174/429	+	settlement
271	174/429	–	settlement
272	172/428	+	settlement
273	173/427	– –	settlement
274	173/427		isolated find
275	173/426	+	settlement
276	173/426		isolated find
277	171/426	– –	settlement
278	174/425	– –	settlement
279	174/424	– –	settlement
280	174/426	+	settlement
281	174/426	– –	settlement
282	174/425	– –	settlement
283	174/425	+	settlement
284	176/425	– –	settlement
285	176/425	– –	settlement
286	175/427	– –	settlement
287	175/427		cemetery
288	175/427	– –	settlement
289	175/427	+	settlement
290	175/427	–	settlement
291	175/426	–	settlement
292	177/425		cemetery
293	178/426		isolated find
294	178/427	– –	settlement
295	178/427	□	settlement
296	177/425	–	settlement
297	177/425		isolated find
298	177/425	+	settlement
299	177/425	+	settlement
300	177/425	+	settlement



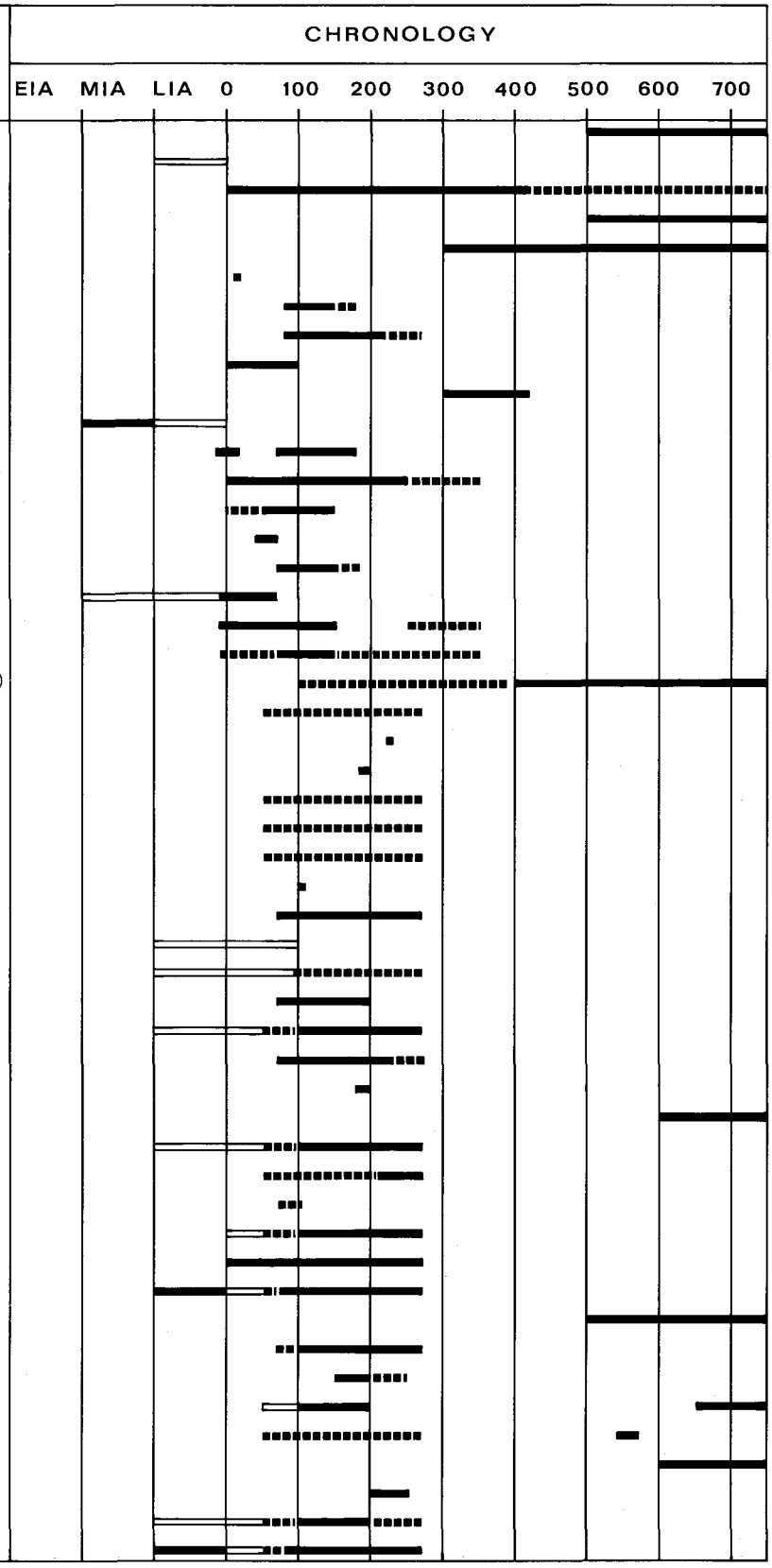
NR.	GRID	FINDS	TYPE OF SITE
301	178/425	–	settlement
302	177/425	+ +	settlement
303	178/425		isolated find
304	178/425		isolated find
305	178/425		cemetery
306	178/425		cemetery
307	178/425	–	settlement
308	177/425		isolated find
309	177/424	– –	settlement
310	176/423	–	settlement
311	177/423	– –	settlement
312	177/424	□	settlement
313	177/424		isolated find
314	177/426		isolated find
315	178/424	+	settlement
316	178/424		isolated find
317	178/424		isolated find
318	178/424	– –	settlement
319	178/424		cemetery
320	178/424	– –	settlement
321	178/424		cemetery
322	178/423	– –	settlement
323	178/423	– –	settlement
324	179/424		cemetery
325	179/424		isolated finds
326	180/426	–	settlement
327	180/425		cemetery
328	181/425	– –	settlement
329	181/427	□	settlement
330	183/427	– –	settlement
331	180/425		cemetery
332	180/425		cemetery
333	181/424	+ +	settlement
334	179/423	– –	settlement
335	179/423	– –	settlement
336	180/423		cemetery
337	180/423	□	settlement
338	180/423		isolated find
339	180/422	□	settlement
340	180/422	– –	settlement?
341	179/421	+	settlement
342	179/420	–	settlement
343	180/420	– –	settlement?
344	180/420		cemetery
345	182/419	– –	settlement?
346	181/423		cemetery?
347	181/423		isolated find
348	181/422		cemetery
349	182/423		isolated find
350	182/423	– –	settlement



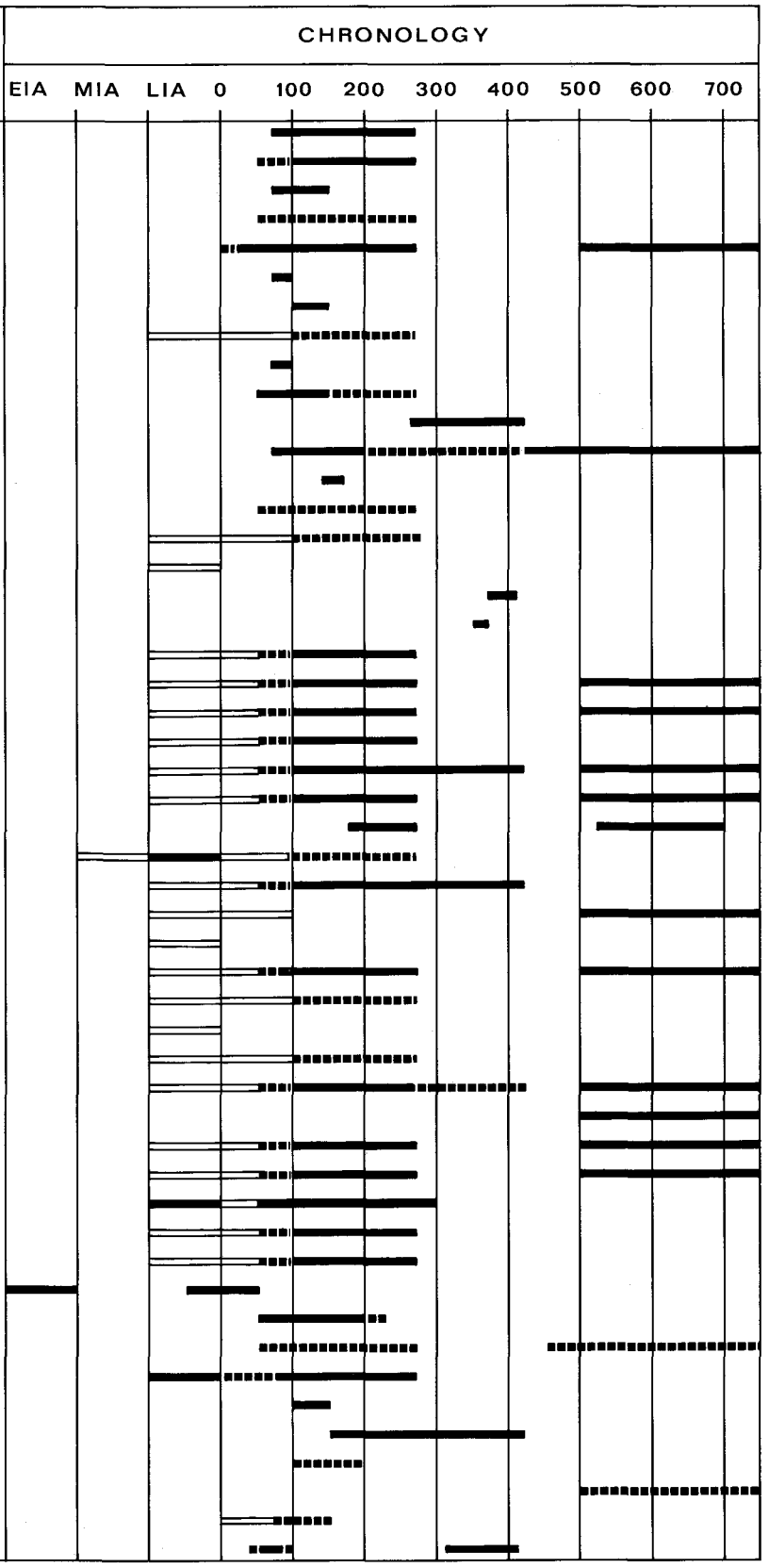
NR.	GRID	FINDS	TYPE OF SITE
351	181/423	+ +	settlement
352	181/422	– –	settlement
353	182/422		cemetery
354	182/422		cemetery
355	182/422	+ +	settlement
356	182/421	+	settlement
357	182/421		cemetery
358	182/421	– –	settlement
359	183/421		isolated find
360	184/421	–	settlement
361	185/421	– –	settlement
362	183/420	–	settlement
363	183/419	–	settlement
364	185/419	– –	settlement
365	185/419	– –	settlement?
366	186/419		cemetery
367	186/419	– –	settlement
368	195/412		cemetery
369	194/412		isolated find
370	194/413		isolated find
371	192/414	– –	settlement
372	192/415		cemetery
373	191/415		cemetery
374	192/415		isolated find
375	192/416	– –	settlement
376	193/416	–	settlement
377	192/416	+ +	settlement
378	192/416		cemetery?
379	190/415	+	settlement
380	190/415	– –	settlement
381	190/416	□	settlement
382	190/416		cemetery
383	190/416		cemetery
384	189/417		cemetery
385	188/418		cemetery
386	188/419		cemetery?
387	189/419		cemetery
388	189/420		isolated finds
389	193/421		cemetery
390	187/422		cemetery
391	188/422	+ +	military settlement
392	188/424		isolated find
393	185/423		cemetery
394	185/423	– –	settlement?
395	184/425	+ +	settlement
396	184/425		cemetery
397	186/427		cemetery
398	186/428		cemetery
399	186/429	+ +	settlement (city)
400	187/428		cemetery



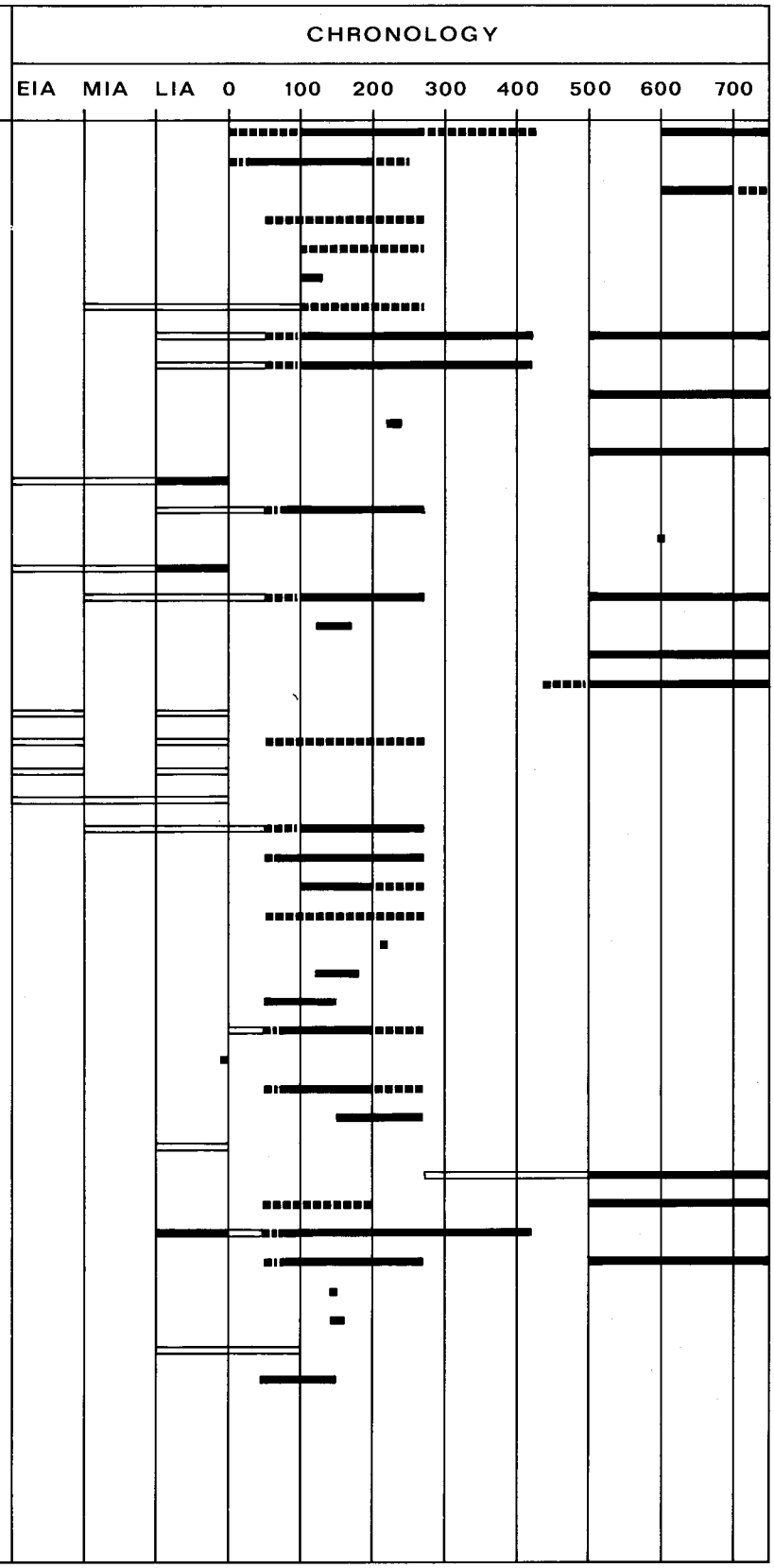
NR.	GRID	FINDS	TYPE OF SITE
401	187/429	–	settlement
402	187/429	– –	settlement
403	188/428	+ +	(military) settlement
404	188/428		cemetery
405	188/428		cemetery
406	188/428		military settlement
407	188/428	+ +	settlement (canabae)
408	188/427	+ +	settlement (canabae)
409	188/428		cemetery
410	188/428		cemetery
411	188/428		cemetery
412	189/429	+ +	military settlement
413	188/427		cemetery (fortress)
414	189/427		cemetery
415	189/427		cemetery
416	189/427	+ +	settlement (canabae)
417	189/427	+ +	(military) settlement
418	190/427		cemetery
419	190/427		cemetery
420	190/427	+	kiln-site (after AD 700)
421	189/425		isolated find
422	191/424		isolated find
423	191/425		isolated find
424	191/426	– –	settlement
425	191/426	– –	settlement
426	192/426	– –	settlement
427	192/426		isolated find
428	192/426		cemetery
429	193/426	– –	settlement
430	191/428	– –	settlement
431	191/428		cemetery
432	194/426	– –	settlement
433	192/425	+ +	kiln-site
434	192/425		cemetery
435	194/425		cemetery
436	199/425	– –	settlement
437	198/428		isolated find
438	196/429		isolated find
439	198/428	–	settlement
440	199/429		cemetery
441	199/429	+ +	settlement
441	199/429		cemetery
442	199/431		cemetery
443	199/431		cemetery
444	200/430	+ +	settlement
445	200/430		isolated find
446	201/430		cemetery
447	201/430		cemetery
448	201/428	–	settlement
449	201/427	–	settlement



NR.	GRID	FINDS	TYPE OF SITE
450	206/425	+	military settlement?
451	207/425	– –	settlement
452	206/423		isolated find
453	206/423	– –	settlement
454	203/423		cemetery
455	203/423		cemetery
456	204/423		cemetery?
457	204/423	– –	settlement
458	206/421		cemetery
459	206/421	–	settlement
460	209/421	+ +	military settlement
461	209/421	+ +	settlement
462	209/420		cemetery
463	208/415	– –	settlement?
464	206/416	– –	settlement
465	206/415	– –	settlement?
466	203/414	+ +	military settlement
467	203/413		hoard
468	202/415	–	settlement
469	166/425	?	settlement
470	166/423	?	settlement
471	166/423	?	settlement
472	166/422	?	settlement
473	168/422	+ +	settlement
474	167/422		cemetery
475	168/422	+ +	settlement
476	168/423	?	settlement
477	168/423	?	settlement
478	169/422	?	settlement
479	170/422	+ +	settlement
480	170/423	?	settlement
481	169/424	?	settlement
482	170/423	?	settlement
483	170/423	?	settlement
484	171/423		cemetery
485	171/423	?	settlement
486	170/425	?	settlement
487	166/420	+ +	settlement
488	173/420	?	settlement
489	167/415	?	settlement
490	170/412		cemetery
491	174/416		cemetery
492	179/416	?	settlement
493	180/417	?	settlement
494	180/417		cemetery
495	181/416	?	settlement
496	186/418		cemetery?
497	187/418		cemetery?
498	185/416		isolated find
499	189/415	+ +	military settlement



NR.	GRID	FINDS	TYPE OF SITE
500	189/415	+ +	settlement
501	189/415		cemetery
502	189/415		cemetery
503	191/412		cemetery?
504	171/439		isolated find
505	169/429		cemetery
506	166/429	–	settlement
509	191/415	–	settlement
508	191/415	□	settlement
509	190/416		cemetery
510	192/444		isolated find
511	188/436		cemetery
512	178/424		isolated find
513	168/437	+	settlement
514	179/417		hoard
515	189/414	+ +	settlement
516	178/435	–	settlement
517	189/435		cemetery
518	180/430		isolated find
519	178/424		cemetery
520	179/424		isolated finds
521	179/423		isolated finds
522	180/424		isolated finds
523	181/422	– –	settlement
524	183/422	–	settlement
525	191/415	–	settlement
526	193/416		cemetery
527	191/416		isolated find
528	190/427		isolated find
529	183/438		cemetery
530	181/439		cemetery
531	184/431	+	settlement
532	169/433		isolated find
533	197/431	□	settlement?
534	186/420	–	settlement
535	168/433	– –	settlement
536	206/445	+ +	settlement
537	206/437	– –	settlement?
538	202/436	–	settlement
539	190/417	□	settlement
540	191/416		isolated find
541	191/419		isolated find
542	171/440	– –	settlement
543	190/427		cemetery



4.2 TYPOLOGY OF THE SITES (THE LEGEND ON APPENDICES 1–5)

An analysis of the different types of sites in the eastern river area will be presented in chapter 8. Although the analysis has already been completed, it will be included in the second part of this study, because it is intimately related to all reconstructions or models of (aspects of) the occupation in the region during each of the periods treated and to any hypothesis on diachronic developments. This paragraph will therefore be restricted to a discussion of the differentiation which is already presented on the distribution maps (Appendices 1–5).

Although the stylized presentation of data requires a certain amount of interpretation, it was decided to present the archaeological data on Appendices 1–5 without too much differentiation. This procedure is both necessary and useful. It is necessary because the distribution maps are not only Appendices to this study; they are also part of the Archaeological Map of the Netherlands, which aims at recording the landscape and occupation of different regions during various archaeological periods. This means that the information given by each map should not be at a very high level of interpretation. This should result on the one hand in the absence of detailed reconstructions of eroded parts of the landscape and on the other in fairly general indications of the archaeological phenomena on that background. In this way, the maps will remain viable for a longer period of time, are adaptable to more and varied sorts of studies, and, as a purpose in itself, they can exist to a certain extent independently of the investigation for which they were first devised.

The presentation of the archaeological data on the maps should, however, only be as general as is absolutely necessary, in order to give a meaningful picture of the variability of the archaeological record of a region. This requires a justification and explanation of the differentiations which have been made.[6]

The basic unit used on the maps is the site.[7] Although many sites are restricted to only one findspot, there are also many that cover some, or numerous findspots. Whenever it was clear that several findspots represented one settlement or burial site only, they were combined. This does not imply that a number of sites which are close together are considered to be definitely separate entities,[8] only that in those cases a possible association needs further proof.

The main reason for this[9] is, of course, that findspots themselves are not meaningful archaeological entities, suitable for further analysis. They become relevant (that is to say: are recognized as an archaeological site) only after interpretation, a decision as to what they are taken to represent, which may include rejection or combination. On the Holocene river-deposits, findspots indicating settlement have always been combined when they are located on the same ancient settlement soil (*oude woongrond*). Because of the limited size of most of these,[10] this implies that combined findspots are usually within a distance of 100 m of each other, and only very rarely further apart than 200–250 m. This agrees well with excavation data from several settlements. Druten-Klepperhei (214) could be as large as c. 200 x 370 m, but other settlements like Rijswijk, with a maximum size of c. 130 x 140 m, Wijk bij Duurstede - De Horden, which measures c. 180 x 180 m, and Heteren (93) of c. 0.75 ha. are certainly smaller.[11] For the Pleistocene deposits, such convenient aids as ancient settlement soils are lacking, but there is no clear reason why settlements there should be of greatly different size. Therefore, findspots closer together than 200–250 m have generally been combined. The same distance was also employed by Bloemers[12] for the province of South Holland.

As far as burial sites are concerned, the situation is gen-

6 It should be remembered that such explanations are necessary in any case, because no 'natural' differentiations are to be found. Van Giffen's famous motto *Die Tatsachen bleiben, die Interpretation schwankt* represents an untenable epistemological position, as was already observed by Glazema in 1950 (Van Es 1976, 294). For discussion of the consequences of this 'normative' view, see e.g. Van der Leeuw 1974.

7 For a discussion of the terminology, see chapter 1, 17–8.

8 For example, sites 296–303 could well be only one settlement (see p. 117, note 23) and 382–384 and 539 may consitute one large cemetery. The number of such cases is, however, very limited.

9 A second reason is the impossibility to represent all findspots on a map of this scale. A comparison with the map of the Land van Maas en Waal published by Peddemors (1978, *Abb.* 15), which is on twice as big a scale and gives (mainly) findspots, will suffice to illustrate this problem.

10 See chapter 3.5. Note again that the 'real' size of a settlement in any period is not necessarily related to the size of the ancient settlement soil.

11 Druten: Hulst 1978, 134–6; Rijswijk: Bloemers 1978a, *Abb.* 20; De Horden: pers. comm. W.A. van Es/C. Blommesteijn; Heteren: Hulst 1971.

12 Bloemers 1978a, Appendix 8.

erally rather simple, because most of the burials are either isolated or occur in clear groups (cemeteries). In only a few cases could burials indicated as separate sites actually be part of the same cemetery, for then it would have to be very elongated, alongside a road, such as 331–332 (cf. note 214) and 382–384 (cf. p. 89 note 8).
Apart from combining several findspots into one site, there are also cases where one findspot is split up into two sites. This has been done to achieve more clarity, to indicate that a site has been used for different activities in different periods. Examples are findspots such as 131/132, where early-Roman settlement traces were found together with a middle-Roman cemetery, or 499/500, where 499 is a fort and 500 the civil settlement which (at least partly) covers the same area.
From what has been said so far, it is evident that the sites indicated on the map are already, albeit low-level, abstractions. Their existence is determined by findspots, which is in accordance with the requirements of the Archaeological Map of the Netherlands. There are, however, even at this level a few deviations, namely, hypothetical and reconstructed sites. As mentioned before, this study does not as a rule include finds of which the findspot is unknown or uncertain. But there are a few finds which were considered so important, that the available data have been used to reconstruct the original findspot and, therewith, a site. This is always indicated in the catalogue, as, for example, for the isolated finds 22, 28, 30, 125, 128, and 427. In these cases, there are at least the finds, or the descriptions of them and the places where they were found, to justify the 'construction' of a site. But this kind of evidence is not present for hypothetical sites, so that their existence has been assumed on other grounds.[13] It is, of course, inconsistent to depict these on the maps, but it was decided to do so anyway because there are only three hypothetical sites (18b, 37, and 183), all hypothetical military camps, which are important in the evaluation of the military aspects of the Roman presence in the river area. They cannot, however, always serve as a basis for further discussion, because they are themselves in some ways the result of that discussion. The 543 sites included as such in the catalogue have been divided chronologically over five periods, and for each period a separate division into categories with different characteristics is presented on the Appendices. The main division is in each case between settlement-, burial- and isolated-sites, whereby in particular the first category may be further subdivided.

Settlements

The defining of a site as a settlement is not only dependent upon archaeological material. On the Holocene deposits, the presence of an ancient settlement soil proved to be of outstanding importance. On the Pleistocene deposits, except where stratigraphical observations have revealed the presence of an occupation layer or features, characterization as a settlement is dependent solely on archaeological material. As a rule, all sites with a relatively large number of finds have been termed a settlement. 'Relatively large' is of course a vague criterion, but it depends entirely on the quality of the evidence. For example, a total of 10 sherds collected during 4 seasons' walking over an arable field is less definitely indicative of a settlement than the same 10 sherds collected from a few mole-hills in grassland during one visit. Furthermore, clustering of material and the presence of finds such as spindle-whorls, loomweights, slags, querns of basalt-lava, and the like are of special value in this respect.
All in all, the minimum requirements for a site to be called a settlement may vary from one sherd on an ancient settlement soil to at least a dozen or so in a field on Pleistocene deposits of which no other data are available. This last figure is in accordance with criteria employed elsewhere[14] but remains nothing more than an assumption, which has no absolute value. It only represents a point in time in the find-histories of sites in those different areas, after which new finds generally confirm the hypothesis regarding the nature of the site.
For the Late-Iron Age, no subdivision of settlements is provided on Appendix 1. Only site 165 is set apart, because it is a kiln.[15] For the Roman Period, the different settlements have been described in more or less neutral terms, in order to avoid the use of the classical terminol-

13 These will be discussed in chapter 8, for each site separately.
14 For the province of South Holland (Bloemers 1978a, Appendix 8): 5–10 finds; for Overijssel (Van Es/Verlinde 1977, 13): 10 determinable sherds. For Texel (Woltering 1979, 17) the detailed survey allowed reliance on clustering of artifacts as a criterion.
15 In this case it was termed a special purpose site, because the kiln is not clearly associated directly with a settlement, see chapter 6, p. 181.

ogy (with concepts such as *vicus, burgus, villa*) which imply a great deal that may not be appropriate or cannot be substantiated without a detailed discussion. Naturally, various propositions on this matter will be put forward in chapter 8.

On Appendices 2–4, a primary distinction is made between military and civil settlements. This is merely intended to mark the presence of the most important military settlements and not to give an exhaustive account of all the possibilities in this respect, because that would imply even more interpretation than is already present. No attempt, for example, has been made to show small middle-Roman military road posts (*stationes beneficiariorum consularis*). The military settlements are divided into two categories: the legionary fortress (Nijmegen) and other forts for the Early- and Middle-Roman Period, and forts or small forts for the Late-Roman Period.

As far as the civil settlements are concerned, the possibilities for general subdivisions during the three stages of the Roman occupation differ considerably. For the Early-Roman Period, the only differentiation is between large and small settlements, with site 417 (Nijmegen-Kops Plateau) as a special case.

During the Middle-Roman Period, the size of the settlements indicates at least a three-level hierarchy. First, the city *Ulpia Noviomagus*; second, a number of large settlements (always with stone buildings); and third, the smaller settlements. The latter have been divided further into those with and those without stone building(s). A fifth category are the large sites that fulfil a special purpose (trade and industry). For the Late-Roman Period, distinctions are again reduced to a large-small dichotomy, and the same is true for the Merovingian Period, when specific military sites no longer exist. One kiln-site has been indicated separately.

The distinctions which have been made involve rather low-level interpretations, with the exception of some settlements classified as forts. Apart from that, the information used includes well-known interpretations which have already been incorporated in the literature (such as the Nijmegen city and fortress), supplemented mainly with data on the size of settlements and the occurrence of stone buildings, which are also standard indicators of the relative importance of settlements.

Other sites

Apart from settlements, several other types of sites have been indicated. As far as burials are concerned, there are no important problems, with the exception of a few finds of complete vessels. These have, as a rule, been taken to represent grave-goods of burials, unless other considerations made this improbable or doubtful (see the catalogue, for example, for the remarks on site 121). For all periods, except the Late-Iron Age where it was irrelevant,[16] a distinction has been made between isolated or small groups of burials and larger cemeteries, containing dozens or hundreds of graves. This distinction could be misplaced in a few cases, because one or a few burials may be the first indication of a large cemetery, but this situation should be rather exceptional. Usually, after the discovery of a grave, its surroundings are rather thoroughly probed to see if there are more of the 'goodies' around.

Isolated finds may point to a number of different kinds of sites, but only one is indicated separately on the maps. These are the finds which have been interpreted as hoards. All others have been lumped together, because their nature is either debatable or they are stray finds, and a further interpretation would be too detailed for the map. In this way, the lost booty (or hoard, or offering?) of site 125 is indicated by the same symbol as a probable offering (120), dumped refuse (316), a sunken ship (211), or a single coin (374).

Indications of certainty

Any inventory of archaeological material and differentiation of sites ends with a number of cases which cannot be classified in a completely satisfactory way. Nevertheless, because they have to be classified in order to be represented, this introduces a degree of uncertainty into the final assessment. This uncertainty, which is essentially a lack of specific data, may be deduced from the way in which the chronology of the sites is presented in this chapter, and from the information provided in the catalogue (chapter 5). For easy reference, it has also been incorporated in the symbols used on Appendices 1–5. Whenever some information about a site is judged to be not completely reliable, that site is indicated as 'probable'. In cases were there is a strong or a second reason for doubt, sites are downgraded to 'possible'. Potential sites, with even more or very serious uncertainties have been omitted.

Uncertainty, or lack of data, may be due to several different causes. The most important source of doubt is insufficient information regarding the chronology of a site.

16 Evidence on burials from this period is very limited.

This problem arises especially where native ceramics are involved. As is explained in chapter 6.3, it is often impossible to decide whether they should be dated to the Late-Iron Age or to the Roman Period, especially when the total collection is only small. This uncertainty has been expressed in the catalogue by describing the native pottery as LIA/RA. Settlements with this sort of data have been indicated on Appendix 1 as well as on 2, in both cases as a probable settlement. 'Probable' in these cases therefore implies that perhaps the settlement should not be included in one of the two Appendices at all, because it did not exist during one of the periods involved.

From time to time, the same problem is encountered with wheel-turned pottery.[17] In most of these cases, however, the uncertainty does not result from a questionable determination of certain items but from the fact that there may be doubt as to whether the (older) finds were indeed found on the site involved. This is, therefore, a second source of uncertainty, which has resulted in indications as 'probable' as well as 'possible'.

A third source of uncertainty is caused by the classification of sites. Examples of this are the finds of complete pots without further data, which are interpreted as grave-goods of burials. In these cases, the site is classified as a 'probable small cemetery or single grave'. The same problem arises, albeit in very few cases, with classifications of sites as settlements at the basic level: are we indeed dealing with a settlement? It occurs more frequently where settlements are further subdivided. For the probable or possible forts, large settlements, and settlements with stone building(s), there is no doubt that they are indeed settlements (except for the three hypothetical sites). But there are not enough data to substantiate fully the particular characteristics attributed to them.

Apart from the factors which cause sites to be downgraded on the certain-probable-possible(-reject) scale, there are also circumstances which may give more reliability to the proposed interpretations. These are sometimes rather specific, as in the case of the hypothetical site 37. The existence of a fort near Kesteren is not just a possibility. There are so many indications for a military presence in the vicinity that the existence of a fort is at least probable.[18]

More general considerations have led, for example, to the acceptance of a number of settlements as Early-Roman, although finds definitely datable to this period are lacking. The reason for this is the relatively short duration of this period (half a century) when compared to the others. Because of this, and the fact that imported Roman pottery can be shown to have a restricted distribution, well-datable finds should be expected to be scarce. Therefore, when a site is certainly occupied during the Late-Iron Age and the Middle-Roman Period, there is no reason to doubt its existence during the half-century in between, at least as long as the site has yielded material which may be dated to this period: either native or early, but long-lived types of wheel-turned pottery.

It should be clear that, for similar reasons, the same cannot be done for other periods. For example, sites with demonstrable middle-Roman and Merovingian occupation may well have also been occupied during the Late-Roman Period, but it would be very unrealistic to take this for granted. The gap between Roman material datable to III A and Merovingian finds, which could be as late as VII B or even VIII A, is too large to be accepted without further evidence, even though there are mitigating circumstances to do so. It is quite clear that the level of production of wheel-turned pottery, and the quantities which were imported, dropped significantly after the Middle-Roman Period. Also, the number of recognizable types is much lower and typochronologically less diverse. Nevertheless, late-Roman, 5th-century, and later Merovingian pottery recognizable as such does occur in the eastern river area, and its absence on a particular site cannot be disregarded. On the other hand, it must be admitted that this is practically the only category of datable material, because 'late' native wares are unrecognizable as such, at least at the present time. We are left, therefore, with the dilemma that the gap cannot be bridged without further evidence while that evidence, even in the eastern river area, is not without its problems. The choice is, thus, between accepting late-Roman occupation without proof or rejecting it on the basis of arguments which are feeble at best. They are, however, the only ones available and, in view of the period of time which is involved, should therefore be used. That late-Roman sites thereby become underrepresented is unavoidable.

17 See pp. 76–7 and the remarks on the dating of several sorts of finds in chapter 6.

18 Cf. Bogaers, in Bogaers/Rüger 1974, 70.

5 THE SITES: CATALOGUE

A number of basic data concerning the sites have been listed in the catalogue. These data are presented under eight different headings which are repeated for every site but which need not all be present in each case. Headings are omitted when information is lacking or when they are irrelevant, according to the principles adopted in the composition of the catalogue. These principles, and those applied in the enumerations under some of the headings, will be explained here. They may facilitate the use of the catalogue and at the same time point out its limitations. All abbreviations specific to the catalogue are to be found in the list of abbreviations on p. 95.

Number and name
Each description of a site is preceded by its number and name. Site names are composed of two elements. The first is the name of the nearest village or town. Names of municipalities have not been used, because these change over the years and are generally less precise. The second is in most cases a field name attached to the findspot. Where such a convenient label was not available, street names or other indicators have been used. Where appropriate, names correspond as far as possible to the traditional names of findspots used in older literature.

Situation
A number of different data pertaining to the situation of the site may be given. For the geological situation, information is only provided when it cannot be inferred from the maps (Appendices 1 to 5). This is always the case when a site is an ancient settlement soil. Also, information on the topographical situation and find–circumstances may be given here, for example, when the finds are suspected to have been found in a secondary position or when there are doubts about the exact location of the site as is the case with reconstructed findspots of older finds. As already mentioned in chapter 1, sites which are certainly due to secondary deposition of materials and sites which can no longer be located with a reasonable degree of certainty are, as a rule, not included in the catalogue.

Excavation
The excavation of a site is listed by naming the archaeological institute responsible and the year(s) in which it was carried out. When a site has been excavated, the finds will generally not be mentioned in detail because that would fall outside the scope of the entire study.[1] Sometimes, however, a few of the more important finds are included, especially when the information is not (yet) easily accessible.

Finds
The finds have been divided into categories, as far as the Roman pottery is concerned corresponding with established groupings in pottery descriptions. The categories are as follows:
(a) Terra sigillata
(b) Colour-coated ware
(c) Gallo-Belgic ware
(d) Smooth ware
(e) Coarse ware
(f) Other Roman pottery
(g) Coins
(h) Stamps, except those on terra sigillata which are mentioned under (a)
(i) Graffiti
(j) Brick
(k) Other building materials
(l) Other Roman finds
(m) Hand-made (native) pottery
(n) Other native finds
(o) Merovingian (wheel-turned) pottery
(p) Other Merovingian finds
(q) Not precisely datable finds
As far as pottery is concerned, only the different types are given, not their numbers. It was decided to omit this information because, although important in some respects,[2] its inclusion would not provide essential information and create several problems.[3] Only in the case of sherds which could not be exactly determined because

1 See chapter 1, 17.
2 See e.g. the remarks on p. 71–2, 76–7, 90, 163–4, and 179–80.
3 These include the impossibility of counting sherds from one pot as one unit before it was restored or, conversely, to count the original pieces of restored pots, the fact that some finds, which were unavailable for study, were mentioned in the literature or the CAA without frequency of occurrence, and a high risk of error because additions to the catalogue were incorporated up to the very last moment. Rough counts of all items mentioned in the catalogue are, however, indicated on the original site-data-sheets which are filed in the CAA at the ROB.

they were either indeterminable (most wall-sherds), could not be traced, or were not studied for other reasons, is an indication of quantity given as follows:
x = present, quantity unknown
xx = present, fewer than five fragments
xxx = present, five fragments or more
The pottery, some of which is dicussed in more detail in chapter 6, is described in terms of existing typological series. There is, of course, a measure of chronological overlapping between these series, and that fact has to some extent been used in the catalogue. For example, in many instances Stuart's typology[4] for the pottery from Nijmegen has been adopted. The bulk of this material is dated to the Flavian period. Therefore, fragments judged to be significantly earlier or later are not referred to by Stuart's type-number but by one from another, chronologically more appropriate, type-list. For example: not Stuart type 101 (St 101) but Haltern type 47 (Ha 47), and not St 203 but Niederbieber type 89 (Nb 89).[5]
Some types are always cited in one way only. These are the following forms:
Dolia. Large dolia are always labelled Hofheim type 78 (Hofh 78), [6] while their much smaller, smooth-walled counterparts are labelled St 147. *Mortaria* with a vertical rim are cited as Haltern type 59 (Ha 59) for the early variety and Brunsting type 37 (Br 37)[7] for the later one; those with a horizontal rim as Br 36. *Lids* of coarse fabric are always indicated as St 219, smooth-walled *incense cups* as St 145 and the so-called *honey-pots* as St 146. Bowls with inturned, thickened rims, the 'cork-urns' of terra nigra-like fabric, are labelled Br 16C.
'Belgic' ware is generally labelled according to the forms distinguished by Holwerda.[8] Merovingian steep-walled, bucket-shaped pots with everted rims are, in most cases, described in Böhners[9] typology as Bö D 9–12. This general indication is preferred because a more detailed one is almost always dubious or even impossible to determine for small fragments. Hand-made native pottery is not described typologically but chronologically. As far as possible, native ceramics, which are discussed in paragraph 6.3, have been dated to the Early-, Middle-, or Late-Iron or to the Roman Age (EIA, MIA, LIA, RA). Late-Iron Age glass bracelets are described by the type-numbers assigned to them by Haevernick.[10]

Collection
The collections in which the finds of the different sites have been stored are only mentioned when the material is in a public collection of a museum or society, or in (temporary) storage at an archaeological institute, pending final publication. Other collections are grouped under 'private'. The exact whereabouts of these finds, and also museum inventory numbers, and the like, are stored in the CAA.

Literature
The literature is cited according to the following principles. As far as possible, the original publication is cited, obvious duplications of simple find-reports only once. When a recent article referring to older literature exists, this is cited instead, if necessary with additional publications since its time of appearance. For easy reference, sites which are in the catalogues compiled by Modderman and Peddemors[11] are always cited by the site number in those publications. Discussions of special finds from a site in more general publications, for example on terra sigillata or on coins, are not normally incorporated. Simple find-reports in *Archeologisch Nieuws* or annual reports are not cited by author but by the abbreviated name of the magazine, year of publication, and page(s). They are therefore not mentioned in the references at the end of this study.

Other data
When there are other data of special relevance to the site, these are mentioned under this heading. They include C14 dates, pollen spectra, etc.

4 Stuart 1963, 1977.
5 Haltern: Loeschcke 1909; Niederbieber: Oelmann 1914.
6 Ritterling 1912.
7 Brunsting 1937.
8 Holwerda 1941.
9 Böhner 1958.
10 Haevernick 1960. 'Haev ?' means that the exact type is unknown or uncertain.
11 Modderman 1949, 1951; Peddemors 1978.

List of abbreviations used in the catalogue[12]

al.	alinea (paragraph)
Al	Alzei (Unverzagt 1916)
AN	*Archeologisch Nieuws*, in the *Nieuwsbulletin van de Koninklijke Nederlandse Oudheidkundige Bond (NKNOB)*
a.s.s.	ancient settlement soil (see chapter 3.5)
AWNN	(collection of the) Archeologische Werkgemeenschap voor Nederland, afdeling Nijmegen en omstreken; the collection is currently in the MFB
Bö	Böhner 1958
Br	Brunsting 1937
ch.	chapter
Ch	Chenet 1941
Curle	see under Drag
Drag	Dragendorff 1895. In cases where Dragendorff's typology is inadequate, other common type-indications (Curle, Knorr, Lud) are used. See Oswald/Pryce 1920 for the general sigillata typologies
EIA	Early-Iron Age, native pottery
Evelein	Evelein 1928
GAS	(collection of the) Gelderse Archeologische Stichting, currently in the GMA
GMA	Gemeentemuseum Arnhem
Gose	Gose 1950
Ha	Haltern (Loeschcke 1909)
Haev	Haevernick 1960
HBW	Holwerda, Belgische Waar (Holwerda 1941)
HKKO	(collection of the) Historische Kring Kesteren en Omstreken
HNW	Holwerda, Nijmeegse Waar (Holwerda 1944)
Hofh	Hofheim (Ritterling 1912)
Isings	Isings 1957
JAWNN	Jaarverslag van de AWNN (Annual report of the AWNN)
Knorr	See under Drag
LIA	Late-Iron Age, native pottery
Lud	Ludowici, see under Drag
MFB	Museum Frans Bloemen, Wijchen
MIA	Middle-Iron Age, native pottery
Nb	Niederbieber (Oelmann 1914)
Ob	Oberaden (Albrecht 1938)
ON	*Opgravingsnieuws der samenwerkende oudheidkundige instellingen in Nederland*
ORL	*Der obergermanisch-rätische Limes des Römerreiches*, ed. by O. von Sarwey, E. Fabricius and F. Hettner, Berlin/Leipzig 1894–1937
Pirling	Pirling 1966, 1974
RA	Roman Age, native pottery
St	Stuart 1963, 1977
Trier	Hussong/Cüppers 1972
T 1–4	Types 1–4. These are types of pottery discussed separately in chapter 6; T1-T2: 162-64, T3: 168-70, T4: 167.

CATALOGUE

1 Ede – Veldhuizen
Excavation ROB 1968, 1971
Finds *(g)* sestertius Faustina, *(h)* LEG IM (retro) on tegula, *(l)* 2 bronze statuettes
Collection GAS, ROB
Literature AN 1969, 35–36; JROB 1971, 20–21; Van Es 1973; Zadoks/Peters 1976

2 Ede – Peppelensteeg
Finds *(e)* xx, *(g)* aes-4 Constantinus II, *(m)* LIA, RA, *(n)* bracelet Haev 3b
Collection private
Literature AN 1971, 91, 126

3 Ede – Manen
Finds *(a)* Ha 8, *(m)* RA
Collection GAS, private
Literature AN 1970, 68; Van Es 1973

4 Ede – Maanderbuurtweg
Excavation ROB 1971
Finds *(m)* EIA, MIA and LIA
Collection ROB
Literature JROB 1971, 19

5 Ede – Verlengde Parkweg
Finds *(a)* Ch 320 and x, *(c)* Ch 342, T1, *(e)* xxx, *(g)* denarius Domitian, *(l)* late-Roman bronze hairpin, *(m)* RA, (o) x?
Collection GAS, private
Literature AN 1971, 126

6 Bennekom – De Kraats
Finds *(m)* EIA, MIA and LIA, *(n)* bracelets Haev 3a, 3b
Collection private
Literature JROB 1972, 95

7 Bennekom – Achterstraat
Excavation ROB 1970–71
Finds *(g)* as/dupondius (Faustina?), *(h)* EX [GER INF] on tegula, *(l)* bronze statuette
Collection ROB
Literature JROB 1970, 14; AN 1972, 30; Van Es 1973; Zadoks/Peters 1976

8 Bennekom – Kerkhoflaan
Finds *(o)* xxx
Collection Vereniging Oud Bennekom

12 Normal standard-abbreviations of literature are not included here. These are listed on the inside of the cover.

9 Achterberg – Friesesteeg
Finds *(i)* E·R·E·F·-V· γXXII·M· PRO·CLV· on torc *(l)* two gold torcs and fragment of a third
Collection Centraal Museum, Utrecht
Literature Roes 1947, 1951; Waterbolk/Glasbergen 1955

10 Rhenen – Lijstereng
Situation location reconstructed
Finds *(a)* Lud Sa, stamp MII[] IIDOI (see ch. 6.2.1), Drag 37 (see ch. 6.2.1), Drag 33, *(d)* x, *(e)* x, *(h)* CPCT on glass bottle, *(l)* bottle Isings 50, bronze bowl, iron and bronze fragments, pipe-clay figurine, *(m)* RA
Collection RMO
Literature Pleyte 1877–1903 (Utrecht), 3–4 and plate I

11 Rhenen – Utrechtsestraatweg I
Finds *(a)* Drag 37, Drag 44, *(b)* x, *(d)* xxx, *(e)* xxx, *(l)*, bronze, iron fragments, *(m)* RA
Collection Flehite
Literature Van Es 1968

12 Rhenen – Utrechtsestraatweg II
Excavation ROB 1951
Collection RMO
Literature Ypey 1973a, 1978; Böhme 1974, 185

13 Rhenen – Donderberg
Finds *(b)* xx, *(e)* xx, *(m)* RA
Collection private
Literature AN 1970, 154

14 Rhenen – Laareind
Situation possibly secondary location
Finds *(m)* LIA/RA
Collection HKKO

15 Rhenen – Laarse Berg
Situation possibly secondary location
Finds *(e)* xx, *(m)* LIA/RA
Collection GAS

16 Wageningen – Diedenweg
Excavation RMO 1927, BAI 1949, ROB 1980–1
Collection RMO, GAS, Nederlands Landbouw Museum Wageningen, ROB
Literature Van Es 1964, JROB 1980, 58 and 1981, 74–6

17 Wageningen – Diedenweg
Excavation RMO 1927, BAI 1949
Collection RMO, GAS, Nederlands Landbouw Museum Wageningen
Literature Van Es 1964, 256–263, 295, 300–301, 305–307

18A/B Wageningen/Randwijk – Lexkesveer
Situation The finds from Wageningen (Westberg, site 18A) are certainly in a secondary location. They may, however, be related to a supposed castellum near Randwijk (site 18B) of which no remains have been discovered as yet. The location of site 18B is therefore purely hypothetical
Finds *(e)* x?, *(h)* EX GER INF on tegula(e?)
Collection GAS
Literature Janssen 1845; Pleyte 1877–1903 (Gelderland), 48; JRMO 1938, 5; Lonkhuysen/Oosting 1938; Kramer-Clobus 1978, 521–4; Willems 1980a

19 Wageningen – Nijverheidsschool
Finds *(m)* LIA/RA
Collection private

20 Wageningen – Hartenseweg
Finds *(m)* LIA/RA
Collection GAS

21 Wageningen – Quadenoord
Excavation RMO 1936–37
Finds *(a)* x, *(c)* Ch 342, x, *(d)* St 109, *(e)* Nb 89, Al 27, x, *(g)* 'Antonine'?, *(m)* RA, *(o)* Bö B, Bö D 9–12, *(q)* iron slag
Collection RMO, private
Literature Braat 1940, 29–35
Other data Probably round 2000 m ESE of this settlement, near Heelsum, Merovingian burials were discovered before 1881 (Pleyte 1877–1903 (Gelderland), 46). Their location could not be reconstructed precisely enough to be indicated on the map.[13]

22 Wolfheze
Situation location reconstructed
Finds *(g)* 246 denarii, Hadrian-Septimus Severus
Collection Koninklijke Nederlandse Academie van Wetenschappen
Literature JMP 47, 1960, 89–91

23 Arnhem – Rosandepolder
Situation found during dredging activities, c. 75 % of the finds clearly transported by water (rounded off)
Finds *(a)* Drag 37, Knorr 78, Drag 18/31, 31, 24/25, 27, 36, 45, and xxx, *(b)* St 2, Br 5, xx, *(c)* HBW 27, *(d)* St 107, 108, 110A, 110B, Hofh 50A, Br 36, Br 37, xxx, *(e)* Nb 89, Al 27, Al 34, xxx, *(g)* as Nero, *(j)* xxx, *(k)* tuff, *(l)* net-sinkers, *(m)* RA, *(o)* Bö D 9–12

13 In an IPP student-paper, C. Blommesteijn has recently reconstructed the findspot at the co-ordinates 180.63/443.15.

Collection GAS
Literature ON 1953, nov. and dec. The Hallstatt D bracelets mentioned in JROB 1973, 80 are probably not from the same findspot

24 Arnhem – Het Dorp
Finds *(m)* EIA ?, MIA ?, LIA
Collection private

25 Arnhem – De gulden bodem
Finds *(o)* XX
Collection GAS

26 Arnhem
Situation exact findspot unknown, reconstruction fairly secure
Finds *(g)* 7 denarii, Vespasian – Severus Alexander
Literature Pleyte 1877–1903 (Gelderland), 3

27 Arnhem – Gele Rijdersplein
Finds *(d)* x?, *(e)* x?, *(m)* LIA/RA
Collection GAS ?
Literature AN 1956, 19–20, 1957, 255

28 Velp – Het Laar
Situation exact findspot unknown, reconstruction fairly secure
Finds *(g)* solidi and medallions Constantinus I – Galla Placida, *(l)* gold bracelets and necklace
Collection Koninklijk Penningkabinet Den Haag, Cabinet des Médailles Paris
Literature Zadoks-Josephus Jitta 1950

29 Velp – Dulkeshof
Situation possibly secondary location
Finds *(d)* XX, *(m)* LIA/RA
Collection private
Literature AN 1957, 52

30 Velp – Hervormde kerk
Situation location reconstructed
Finds *(l)* gold nacklaces and bracelets
Collection Museum für Völkerkunde, Berlin
Literature Janssen 1852, 161–180, Braat 1954

31 Velp – Hogeweg
Finds *(m)* LIA
Collection GAS

32 Velp – Grote en kleine Durk
Situation location reconstructed, secure
Finds *(l)* bronze wire fibula, enamelled plate fibula, crossbow fibula, umbo, other bronze objects, silver spoon, glass balsamarium
Collection GMA
Literature Pleyte 1877–1903 (Gelderland), 37–39
Other data The composition of the material from this site is rather unusual

33 Rheden – Rhederhof
Finds *(d)* x?, *(e)* x?, *(l)* bronze vase
Collection RMO
Literature JRMO 1928, 57

34 Aalst – Nedereindsestraat I
Finds *(d)* x, *(e)* x
Collection HKKO

35 Aalst – Nedereindsestraat II
Finds *(a)* Drag 18/31, stamp MEÐÐI (see ch. 6.2.1), *(b)* St 2, *(d)* St 110A
Collection GAS

36 Kesteren – Bandijk
Situation possibly in secondary position
Finds *(e)* XX
Collection GAS

37 Kesteren – Lede en Oude Waard
Situation The castellum Carvo (Carvone?) is not situated south of the Bandijk, where an associated settlement (site no. 38) has been found (the area indicated by Bogaers/Rüger 1974, 71). In spite of intensive efforts, the exact location of the fort has not been established. The most logical assumption, therefore, seems to be that it was located north of the Bandijk and eroded by a post-Roman branch of the Rhine. All deposits north of the dike are indeed post-Roman. At the scale of the map, the indicated position of the fort should be fairly accurate
Literature Bogaers/Rüger 1974, 70–71

38 Kesteren – Nedereindsestraat
Situation a.s.s.
Excavation ROB 1968, 1977
Collection RMO, HKKO, GAS
Literature Bogaers/Rüger 1974, 70–71, Hulst 1974, AN 1975, 160–161, JROB 1977, 32

39 Kesteren – Prinsenhof
Excavation ROB/HKKO 1974
Collection HKKO, ROB
Literature AN 1975, 160–161; Wigcherink 1979

40 Kesteren – Begraafplaats
Finds *(a)* Drag 31, stamps TVLLVS FE, CERIALIS·F, VIDVCUSF, Drag 33, (b) St 1, St 2, St 3, *(c)* T1, *(d)* Br 5c, Gose 379/80, *(e)* St 201, St 218
Collection HKKO
Literature AN 1967, 112, 1971, 36–37

41 Kesteren - dorp
Situation a.s.s., but the only Merovingian find is possibly secondary
Finds *(o)* Bö D 9–12. There is also one vessel Bö B/C reported 'from Kesteren', but for this find site no. 49 is a more likely findspot
Collection RMO
Literature Modderman 1949, no. 88

42 Kesteren – Peppelenwoerd
Situation a.s.s.
Finds *(a)* Drag 31, *(b)* xxx, *(d)* St 110B, xxx, *(e)* Nb 89, Nb 104, St 218, xxx, *(m)* LIA,RA, *(q)* loomweight
Collection GAS, private
Literature Kalee 1964

43 Kesteren – De Hoge Woerd
Situation a.s.s.
Excavation ROB 1960, 1961
Finds *(a)* Drag 30, Drag 37, Drag 33?, xx, stamp OF.CN, *(b)* xxx, *(c)* Girth-beaker, HBW 52, HBW 28, Br 16C, xxx, *(d)* Hofh 50/51, Ha 59 ?, Br 36, Hofh 78, xxx, *(e)* St 201, St 210, St 213A, St 214, St 218, xxx, *(g)* as Vespasianus, silver coin Marcus Antonius, *(h)* stamp on tegula, probably LXG,[14] *(j)* xxx, *(k)* tuff, xxx, *(l)* fibulae, glass, wall painting fragments, *(m)* LIA, RA, *(n)* La Tène fibulae, bracelets Haev 3a, 3b, 6a, 7a, *(q)* spindle whorls, loomweights, iron slag
Collection RMO, HKKO, private
Literature Heuff 1905; ER III, 117, al. 4–6; AN 1960, 83, 1961, 69–71; Kalee 1964

44 Ochten – Groenestraat
Finds *(b)* xx, *(d)* St 109A, xxx
Collection GAS

45 Ochten – Heuningstraat
Situation a.s.s.
Finds *(d)* xx, *(e)* Nb 104, xx
Literature Modderman 1949, no. 119

14 Pers. comm. J.E. Bogaers.

46 Ochten – dorp
Situation a.s.s.
Finds *(a)* x, *(b)* x, *(d)* St 110B, x, *(e)* Nb 111, Nb 112, St 219, T 3, x, *(g)* bronze coin Nerva?, *(o)* Bö D9–12
Collection RMO
Literature Modderman 1949, no. 117; ER III, 118, al. 3

47 Kesteren – Bergzicht
Situation a.s.s.
Finds *(b)* St 2, *(d)* xx, *(i)*]RΛ[on jug-wall *(m)* RA
Collection RMO
Literature Modderman 1949, no. 94

48 Opheusden – De Zeven Morgen
Situation a.s.s.
Finds *(a)* Drag 29, Drag 37 (see ch. 6.2.1), Nb 16, Drag 27, Drag 31, Drag 32, Drag 33, Drag 39, Drag 40?, Lud Tf, Lud Tv, Drag 44, Drag 45, xxx, *(b)* St 2, St 3, Br 5A, St 10, Br 8A, Gose 226/227, xxx, *(c)* Girth-beaker, HBW 55, T 1, xxx, *(d)* St 107, Br 5c, St 129A, St 145, Br 36, Br 37, Hofh 78, xxx, *(e)* St 201, St 202, St 210, St 219, Nb 89, Nb 98, Nb 104, Nb 110, Nb 111, Nb 112A, Br 22, xxx, *(h)* ADIV̂TOR on Br 36, EXGERINF on imbrex, *(i)*]C[on terra sigillata fragment, *(j)* xxx, *(k)* wattle and daub fragments, *(l)* iron objects, fragments of glass, *(m)* RA, *(q)* bone, a.o. horse and cattle
Collection HKKO
Literature AN 1969, 68–69

49 Opheusden – De Kouwenoort
Situation There is some doubt about the relationship of the objects, but a Merovingian cemetery seems fairly certain
Finds *(g)* coins Hadrianus(?) – Postumus (in Merovingian urn!?), solidus Gratianus?, *(o)* Bö B1b, Bö B3b and possibly a third pot, formerly in the museum in Tiel (see site nr 41), *(q)* Bronze Age needle and razor, found in one of the urns?
Collection Municipality of Rhenen
Literature Jesse 1911; Modderman 1949, no. 92; Doorenbos 1950, 21–22; Ypey 1962/63

50 Opheusden – dorp
Situation a.s.s.
Finds *(e)* St 201, *(m)* xx, *(o)* x
Collection RMO, HKKO
Literature Modderman 1949, no. 87

51 Opheusden – Molukkenstraat
Situation Sherds possibly in secondary position
Finds *(d)* xx
Collection GAS

52 Opheusden – Stijwaard
Situation a.s.s.
Finds *(m)* LIA/RA
Literature Op 't Hof 1970, 11

53 Dodewaard – Het Hoge Huis
Situation a.s.s.
Finds *(d)* St 131, Br 36/37, xx, *(e)* St 210, St 218, xx, *(m)* LIA, RA
Collection RMO, GAS, HKKO
Literature Modderman 1949, no. 85

54 Dodewaard – Linge
Situation a.s.s.?
Finds *(m)* LIA
Collection HKKO

55 Dodewaard – De Logt
Situation a.s.s.
Finds *(m)* RA
Literature Op 't Hof 1970, 10

56 Dodewaard – Appelenborg
Situation a.s.s.
Finds *(a)* xx, *(b)* xx, *(c)* T 2,xx, *(d)* Hofh 78, xxx, *(e)* St 201, Nb 89, Nb 104, xxx, *(g)* bronze coin Nerva and/or coin Constantinus I?, *(m)* RA
Collection GAS
Literature ER III, 118, al. 4; Modderman 1949, no. 127

57 Dodewaard – Hervormde kerk
Situation a.s.s.
Finds *(d)* x?, *(e)* x?, *(g)* denarii Faustina and Caracalla, *(l)* tombstone with incription M(arcus) TRAIANIV[S] GVMATTIVS GAISIONIS F(ilius) VE-T(eranus) ALAE AFROR(um) T(estamento) P(oni) I(ussit) in secondary position
Collection RMO
Literature ER II, 170–171, no. 281; JRMO 1889, 42 and 1902, 28; Modderman 1949, no. 86

58 Hien – Nieuwe dijk
Finds *(d)* xx, *(j)* xx, *(m)* RA
Collection GAS

59 Hien – De Wuurdjes
Situation a.s.s.
Finds *(a)* Drag 37 (see ch. 6.2.1), Drag 31, Drag 32, Drag 33, Drag 38, Drag 45, Lud Sf/SMc, xxx, stamp CILLVTIVSF (see ch. 6.2.1), *(b)* St 2, St 3, St 10, Br 4, Br 5A, Br 8A, Br 8B, xxx, *(c)* HBW 52, HBW 55, HBW 64, HBW 75, T 1, T 2, Br 16C, waster from kiln (terra nigra), xxx, *(d)* St 110A, St 111, St 129B, St 132B, St 138, St 147, Br 36, Br 37, xxx, *(e)* St 201, St 202, St 210, St 216, St 218, St 219, T 4, Nb 89 (a.o. late specimens), Nb 104, Nb 112 A and B, xxx, *(g)* 2 bronze coins, barbaric imitations (IIId), *(h)* LXG[on tegula, *(j)* xxx, *(k)* tubuli, tuff, limestone, opus signinum, *(l)* glass, bronze and iron fragments, slags, *(m)* RA, *(o)* crescent shaped rim, *(q)* loomweight, wattle and daub
Collection HKKO
Literature Datema 1976

60 Wely – Waalbandijk
Finds *(a)* xx, *(d)* xx, *(e)* xx, *(m)* RA
Collection GAS

61 Wely – Het Höfke
Situation a.s.s.
Finds *(a)* Drag 30, *(b)* xx, *(d)* Hofh 78, xxx, *(e)* St 201, St 204B, T 4, xxx, *(m)* LIA, RA
Collection GAS, HKKO
Literature Modderman 1949, no. 81

62 Opheusden – De Roetert
Situation a.s.s.
Finds *(a)* Drag 29 (see ch. 6.2.1), Drag 37 (see ch. 6.2.1), xxx, *(b)* xxx, *(c)* HBW 55, Br 16C, T 2, xx, *(d)* St 110A, St 132B, Hofh 78, Br 36, Br 37, xxx, *(e)* St 201, St 202, St 210, Al 27, T 3, T 4, Nb 89, xxx, *(j)* x, *(l)* iron ring, *(m)* LIA, RA, *(o)* Bö D 9–12, *(q)* (pottery?-) slags
Collection RMO, HKKO
Literature Modderman 1949, no. 77: Op 't Hof 1970, 11

63 Hemmen – De Grote Wust
Situation a.s.s.
Finds *(b)* xx, *(c)* xx, *(d)* St 107, xxx, *(e)* St 210, St 114A, Br 16C, Nb 89, Nb 104, Al 29, T 3, xxx, *(k)* wattle and daub, *(m)* RA, *(o)* Bö D 9–12
Collection RMO, GAS, private
Literature Modderman 1949, no. 83

64 Hemmen – De Kleine Wust
Situation a.s.s.
Finds *(d)* Br 36, xx, *(e)* St 210, St 218, T 4, Al 27, *(m)* xx
Collection RMO
Literature Modderman 1949, no. 83

65 Randwijk – Hokkerde
Situation a.s.s.
Finds *(m)* LIA[15]
Literature Op 't Hof 1970, 11

66 Hemmen – Gesperden
Situation a.s.s.
Finds *(e)* x, *(m)* LIA, RA
Collection RMO, GAS
Literature Modderman 1949, no. 82

67 Hemmen – Lingehof
Situation a.s.s.
Finds *(m)* LIA/RA
Literature Heldring 1838–9, 135–139; Havinga 1969, maps 1 and 2

68 Hemmen – dorp
Situation a.s.s.
Finds *(a)* xx, *(d)* xx, *(e)* xx, *(g)* denarius Traianus, *(m)* RA, *(o)* Bö D 9–12
Collection RMO
Literature Modderman 1949, no. 79

69 Hemmen – Hemmermeer
Situation a.s.s.
Finds *(d)* Br 36/37, x, *(e)* x, *(i)* DEAE VAGDAVERCUSTI SIM[P]LICIVS SVPER DEC(urio) ALAE VOCONTIOR(um) EXERCI[T]VVS BRITANNICI on bronze pedestal, *(l)* bronze pedestal and lamp, net-sinker?, *(m)* LIA, RA, *(n)* bracelet Haev type ?, *(q)* loomweight, spindle whorl
Literature Janssen 1844, 270-281; ER II, 169, no. 280; Modderman 1949, sub no. 78; Zegers/Zandbergen 1958, *bijlage* 1

70 Zetten – dorp
Situation a.s.s.
Finds *(b)* xx, *(c)* HBW 27, *(d)* xx, *(e)* St 201, St 210, Nb 89, Nb 104, Al 28?, xxx, *(g)* denarius Severus Alexander, *(m)* RA, *(o)* Bö D 9–12
Collection RMO
Literature Modderman 1949, no. 59

71 Randwijk – dorp
Situation a.s.s.
Finds *(o)* Bö D 9–12
Collection RMO
Literature Modderman 1949, no. 72

15 After the digging of a drainage ditch in 1982, new material was collected which also included Roman pottery (pers. comm. R. S. Hulst). The data could not be incorporated in fig. 23 and Appendices 2 and 3.

72 Randwijk – Nijborgh
Situation a.s.s.
Finds *(g)* bronze coin Magnus Maximus
Collection GAS
Literature AN 1958, 55

73 Randwijk – De Hoge Asterd
Situation a.s.s.
Finds *(a)* Drag 18/31, *(c)* T 2, *(d)* St 107, St 129, Hofh 78, Br 36, xxx, *(e)* St 201, Nb 89, T 4, *(m)* RA
Collection RMO, private
Literature Modderman 1949, no. 73

74 Zetten – De Korte Stukken
Situation a.s.s.
Finds *(d)* xx, *(e)* xx, *(m)* RA

75 Zetten – De Hoge Hof
Situation a.s.s.
Excavation RMO 1933–36
Finds *(a)* Drag 29 (see ch. 6.2.1), Drag 37 (see ch. 6.2.1), Drag 24/25, *(b)* St 10, xxx, *(c)* HBW 55, T 1, xx, *(d)* Hofh 58, Hofh 65, St 110A, St 110B, St 130, Br 36, Br 37, xxx, *(e)* St 201, St 210, St 214B, T 4, Nb 89, Nb 104, St 219, Al 27, Al 28, Al 29, T 3, *(g)* 3, illegible, *(j)* x, *(l)* fibulae, *(m)* LIA, RA, *(n)* bracelets Haev 3b, *(o)* Bö D 9–12, *(p)* glass beaker, *(q)* iron spur, tuff net-sinker
Collection RMO, GAS, RMK
Literature Braat 1937

76 Zetten – De Grote Woerd
Situation a.s.s.
Finds *(d)* x, *(e)* x, *(m)* LIA/RA
Literature Modderman 1949, no. 63

77 Zetten – De Vergulde Bodem
Finds *(a)* xx, *(b)* xx, *(d)* xx, *(e)* xx, *(m)* RA, *(o)* Bö D 9–12
Collection RMO
Literature Modderman 1949, no. 58

78 Andelst – De Meeuwerden
Finds *(d)* Hofh 78, *(m)* RA, *(q)* wattle and daub
Collection private

79 Herveld – De Grote Woerd bij Rome
Situation a.s.s.
Finds *(a)* Drag 31, Ch 320 (see ch. 6.2.1), xx, *(b)* xx, *(c)* Br 16C, xx, *(d)* St 112, St 146, xxx, *(e)* St 201, St 210, St 218, St 219, Nb 89, Al 34?, T 3, xxx, *(g)* 'coins of Maximianus and Constantinus', *(m)* LIA, RA, *(o)* Bö D 9–12, xxx, *(q)* slags

Collection RMO, GAS, HKKO
Literature Heldring 1838–9, 27–29; Janssen 1844, 299–302; Modderman 1949, no. 55

80 Andelst – De Hoge Hof
Situation a.s.s.
Finds *(a)* xx, *(c)* T 1, *(d)* Hofh 78, St 146, xx, *(e)* St 201, St 210, St 218, Nb 89, Al 30?, T 3, xxx, *(j)* x, *(k)* tuff, *(l)* bronze bowl, *(m)* RA, *(o)* Bö D 9–12
Collection RMO, GAS, private
Literature Heldring 1838–9, 27; Modderman 1949, no. 56

81 Andelst – dorp
Situation a.s.s.
Finds *(a)* xx, *(b)* xx, *(c)* T 1, Br 16C, *(d)* St 127, Br 36, xxx, *(e)* St 201, St 210, T 4, Nb 89 (a.o. very late specimens), Nb 104, Al 27, T 3, xxx, *(g)* as Augustus, denarius (IA), as Nerva, antoninianus Postumus, follis Constantinus I, *(j)* xxx, *(k)* tuff, *(m)* MIA, LIA, RA, *(o)* Bö D 9–12 *(q)* iron slags, wattle and daub, loomweights, bronze object
Collection RMO, GAS, HKKO, RMK
Literature Modderman 1949, no. 57; AN 1968, 54

82 Andelst – Huis Andelst
Situation location reconstructed
Finds *(b)* Nb 33C, *(c)* HBW 26C?
Collection RMO

83 Herveld – De Woerd
Situation a.s.s.
Finds *(c)* xx, *(d)* xx, *(e)* St 201, xxx, *(j)* x, *(k)* tuff, *(m)* MIA, LIA, RA, *(o)* Bö D 9–12
Collection RMO, GAS
Literature Heldring 1838–9, 25–26; Modderman 1949, no. 53

84 Herveld – De Heijmer
Finds *(a)* Drag 32, xx, *(b)* xx, *(c)* xx, *(m)* xx
Collection GAS

85 Herveld – De Legt (Moordakker) I
Situation a.s.s.
Finds *(a)* x, *(b)* St 12, xx, *(d)* Hofh 78, Br 36, xxx, *(e)* St 201, St 210, Nb 104, xxx, *(j)* x, *(k)* tubuli and cement, tuff, *(m)* LIA, RA, *(o)* xx?
Collection RMO, GAS, HKKO
Literature Janssen 1844, 297–299; Modderman 1949, no. 54

86 Herveld – De Legt (Moordakker) II
Situation a.s.s.
Finds *(p)* Merovingian sword, axe (s?), spur?, rings?
Collection RMO
Literature Janssen, 1844, 297; Holwerda 1908, 251–252, nos. 157–166

87 Herveld – Tielsestraat
Situation a.s.s.
Finds *(c)* xx, *(d)* xxx, *(e)* St 210, xxx, *(m)* LIA, RA
Collection RMO, private
Literature Modderman 1949, no. 49

88 Slijk-Ewijk – Hooge Brug
Situation a.s.s.
Finds *(d)* xx, *(m)* RA, *(o)* Bö D 9–12
Collection RMO
Literature Modderman 1949, no. 19

89 Slijk-Ewijk – De Woerd
Situation a.s.s.
Finds *(b)* St 2, *(d)* xx, *(e)* xx, *(m)* LIA, RA, *(o)* xx
Collection RMO
Literature Modderman 1949, no. 46

90 Oosterhout – Hoog Essen
Situation a.s.s.
Finds *(a)* Drag 37, xx, *(b)* xx, *(c)* Br 16C, T 1, xx, *(d)* Hofh 78, St 138, St 146, xxx, *(e)* St 201, St 210, Nb 89, Nb 104, Nb 112B, T 4, Al 28, xxx, *(j)* xxx, *(m)* MIA, LIA, RA, *(o)* Bö D 9–12
Collection RMK, RMO, GAS, private
Literature Modderman 1949, no. 45

91 Elst – Eimeren
Situation a.s.s.
Finds *(a)* xx, *(d)* xx, *(e)* St 210, Nb 89, Br 36, Al 28, xxx, *(m)* LIA, RA, *(o)* Bö D 9–12
Collection RMO, GAS
Literature Modderman 1949, no. 22

92 Valburg – dorp
Finds *(a)* xx, *(d)* xx, *(m)* RA
Collection RMO
Literature Modderman 1949, no. 48

93 Heteren – Het Lage Land
Situation a.s.s.
Excavation ROB 1968, 1969–70
Collection ROB, RMO
Literature Heldring 1838–9, 62–63; Modderman 1949, no. 67; AN 1969, 4–5, 1971, 2–3

94 Heteren – Polderstraat
Finds *(c)* HBW 52, *(e)* Nb 104
Collection GAS

95 Heteren – Uilenburg[16]
Situation a.s.s.
Finds *(c)* Girth-beaker, T 2, *(d)* Hofh 78, Br 36, Br 37, xx, *(e)* St 210, St 213A, T 4, Nb 89, Nb 104, xx, *(m)* LIA/RA
Collection RMO
Literature Modderman 1949, no. 66

96 Homoet – Salingsbouwing
Situation a.s.s.
Finds *(a)* Lud Sb, xx, *(b)* St 2, xx, *(c)* Ha 80, Ha 91A, Br 16C, HBW 55, T 1, T 2, *(d)* Hofh 78, St 138, Br 37, xxx, *(e)* St 201, St 210, St 219, Nb 89, Nb 104, xxx, *(i)*]VSA[on jug, *(m)* EIA, MIA, LIA, RA
Collection RMO, GAS, private
Literature Modderman 1949, no. 21; JAWNN 1974, 17

97 Homoet – De Hoge Woerd
Situation a.s.s.
Finds *(b)* xx, *(d)* St 147, Br 36, xxx, *(e)* St 201, St 210, St 218, St 219, Br 21, xxx, *(k)* tuff?, *(m)* MIA?, LIA, RA, *(o)* Bö D 9–12, *(p)* gold coin tremissis Metz monetarius Garoaldus
Collection RMO, private
Literature Heldring 1838–9, 19–20, Modderman 1949, no. 47

98 Lijnden – Het Höfken
Situation a.s.s.
Finds *(a)* Drag 33, xx, *(b)* xx, *(c)* T 2, *(d)* St 107, St 138, xxx, *(e)* St 201, St 210, Nb 104, xxx, *(m)* LIA, RA, *(o)* Bö D 9–12, *(q)* wattle and daub, slag
Collection RMO, GAS, private
Literature Modderman 1949, no. 20

99 Elst – Grote en Kleine Zuiling
Finds *(d)* xxx, *(e)* xxx, *(j)* xxx, *(k)* tuff, cement *(l)* iron axe, bronze strip, glass, *(m)* RA
Collection private

100 Elst – Reeth
Situation a.s.s.
Finds *(a)* Drag 31, xx, *(c)* Girth-beaker, *(d)* St 107, xxx, *(e)* St 201, Nb 89, Nb 104, *(m)* LIA, RA, *(o)* Bö D 9–12
Collection RMO, GAS
Literature Modderman 1949, no. 36

16 Because there were plans for a new ditch across this site, a small rescue excavation was carried out in January 1983 by R. S. Hulst. Its results could not be incorporated here.

101 Elst – De Wuerde
Situation a.s.s.
Finds *(e)* Nb 89, xx, *(m)* LIA/RA, *(o)* Bö D 9–12
Collection RMO
Literature Modderman 1949, no. 34

102 Elst – Alg. Begraafplaats
Finds *(g)* denarius Commodus
Collection private

103 Elst – Galgenplek
Situation a.s.s.
Finds *(a)* xx, *(d)* St 107, xxx, *(e)* St 219, xxx, *(m)* MIA?, LIA, RA, *(o)* Bö D 9–12
Collection GAS, RMO
Literature Modderman 1949, no. 35

104 Elst – Brienenshof
Situation a.s.s.
Finds *(d)* St 109, St 111, xxx, *(e)* St 210, Nb 89, xxx, *(m)* LIA, RA, *(o)* Bö D 9–12
Collection GAS

105 Elst – dorp
Situation a.s.s.
Excavation ROB 1947, 1981
Collection RMO, RMK
Literature Bogaers 1955; AN 1956, 20; AN 1964, 130; AN 1970, 131–132; Bogaers 1970a; AN 1980, 128; JROB 1981, 30

106 Elst – De Hoge Hof
Situation a.s.s.
Finds *(a)* xx, *(b)* x, *(d)* Br 36, xxx, *(e)* St 201, St 218, Nb 89, Nb 104, xxx, *(m)* LIA, RA, *(q)* spindle whorls
Collection GAS, private
Literature Modderman 1949, no. 37; JAWNN 1972, 16–19

107 Elst – Aamse Pad
Situation a.s.s.
Finds *(m)* LIA
Collection GAS

108 Elst – Snodenhoek
Situation a.s.s.
Finds *(e)* Al 29, xx, *(m)* RA, *(o)* Bö D 9–12
Collection RMO
Literature Modderman 1949, no. 24

109 Raayen – De Woerd
Situation a.s.s.
Excavation RMO 1929
Finds *(a)* Drag 37 (see ch. 6.2.1), xx, *(b)* St 10, Br 4,

Br 5, Br 7B, xxx, *(c)* HBW 52, HBW 55, T 1, T 2, xxx, *(d)* St 109/110A, St 110B, St 129B, St 132, Hofh 78, St 138, Br 36, Br 37, xxx, *(e)* St 201, St 210, St 215, St 219, T 4, Nb 89 (a.o. very late specimens), Nb 104, Al 29, T 3, xxx, *(j)* xxx, *(k)* limestone, tuff, *(m)* LIA, RA, *(n)* bracelet Haev 3b, *(o)* Bö D 1–6, Bö D 9–12, Stampfuss 1939 Abb. 24–2, *(q)* crucibles

Collection RMO, RMK

Literature Braat 1937, 23; Modderman 1949, no. 31

110 Raayen – Het Höfke

Situation location reconstructed

Finds *(g)* bronze coin Faustina, *(l)* bronze handle, jug and patera, bottle(s?) Isings 51; pots, jugs and iron objects are mentioned

Literature Heldring 1838–9, 143–146; Janssen 1844, 262–264; Bogaers 1955, 20–21

111 Elst – Groenoord

Situation a.s.s.

Finds *(a)* xx, *(b)* xx, *(d)* Br 36, xxx, *(e)* St 201, St 210, Nb 89, Nb 104, xxx, *(m)* EIA?, MIA, LIA, RA, *(o)* Bö D 9–12, *(q)* spindle whorls

Collection RMO, GAS

Literature Modderman 1949, no. 32; AN 1957, 183

112 Elst – Distelheuvel

Situation a.s.s.

Finds *(a)* xx, *(d)* xx, *(e)* xx, *(m)* LIA/RA

Collection RMO

Literature Modderman 1949, no. 30

113 Elst – De Perk

Finds *(g)* bronze coin Antoninus Pius?

Literature BROB 1951–1, 2

114 Elst – De Laar

Situation a.s.s.

Finds *(a)* Drag 45, xx, *(b)* xx, *(c)* T 2, *(d)* xx, *(e)* St 201, St 210, T 4, Nb 89, Nb 104, Al 29, T 3, Al 27 (late specimen), *(m)* LIA, RA, *(o)* Bö D 9–12, *(q)* spindle whorl

Collection RMO

Literature Modderman 1949, no. 28

115 Elst – Laarstraat

Situation a.s.s.

Finds *(c)* xx, *(d)* xx, *(e)* xx, *(m)* LIA/RA

Collection RMO

Literature Modderman 1949, no. 42

116 Driel – Het Vlot

Finds *(m)* LIA/RA

Literature Modderman 1949, no. 75

117 Driel – Baarskamp/Lizesland

Situation a.s.s.

Finds *(a)* Ha 12, Ha 13, Drag 29 (see ch. 6.2.1), Drag 37 (see ch. 6.2.1), Drag 27 (a.o. very early specimens), Drag 31, stamp VENICAR[(see ch. 6.2.1), Drag 32, Drag 33, Drag 44, Drag 45, Curle 11, xxx, stamp]OF L C VIR[(see ch. 6.2.1), *(b)* St 1, St 2, St 7, St 10, St 21, Br 3, Br 4, Br 5, xxx, *(c)* Girth-beakers, Ha 91A, Br 16 C, HBW 28, HBW 52, HBW 55, HBW 81, T 1, T 2, xxx, *(d)* Ha 47, Hofh 50, St 107, St 109/110A, St 110B, St 111, St 129, St 132?, St 138, Ha 69, Hofh 78, St 146, Br 36, Br 37, xxx, *(e)* Hofh 81A, St 201, St 210, St 216, St 218, St 219, T 4, Nb 89, Nb 104, Nb 112A, Nb 112B, xxx, *(f)* 'fine Nijmegen' pottery, pipe-clay bulb (?), *(g)* bronze coin Augustus with countermark, *(j)* xxx, *(k)* tuff, tubuli, *(l)* fibulae types Ettlinger 1973, nos. 45, 4 and 42,3 and Böhme 1972, nos. 21b and 46c, glass, copper plate, iron objects, oister shells, *(m)* MIA, LIA, RA, *(n)* bracelets a.o. Haev 7a, *(o)* Bö B?, Bö D 1–6, Bö D 9–12, *(q)* loomweight

Collection private, RMO

Literature Modderman 1949, no. 68; BROB 1951–3, 3–4; Willems 1980a, 347–8

118 Driel – Marskamp

Situation a.s.s. The site may well be part of site no. 117

Finds *(a)* Drag 37, Drag 27, Drag 31, Drag 33, xx, *(b)* St 2, Br 4, xxx, *(c)* HBW 52, T 1, T 2, xx, *(d)* Ha 48, St 110B, St 132B, xxx, *(e)* St 118, Nb 89, Nb 104, xxx, *(j)* xxx, *(m)* MIA?, LIA, RA, *(q)* wattle and daub

Collection private, RMO

Literature Modderman 1949, no. 69

119 Driel – Ooijevaarsnest

Situation a.s.s.

Finds *(a)* xx, *(b)* St 1, xx, *(c)* T 1, T 2, *(d)* St 138, xxx, *(e)* St 201, St 210, T 4, St 219, Nb 89, Nb 104, xxx, *(j)* xxx, *(m)* RA

Collection private

Literature Modderman 1949, no. 74

120 Driel – Stuwdam

Finds *(a)* Drag 31, stamp PETRVLLVSF, *(d)* xx, *(e)* Hofh 86, *(i)* x on Drag 31 and SALLIOS C(ai) TVRM (a) S(olvit) M(erito) on flagon

Collection RMK, private

Literature AN 1966, 100; 1967, 26

121 Doorwerth – Doorwerthsche Waarden

Situation The interpretation of no. 120, sub *(d)* as an offering to an unknown deity is probably correct on

account of the inscription. The jug is severely damaged. The interpretation of the undamaged jugs 120, sub *(e)* and 121, sub *(b)*, is problematical. They could also represent offerings but, because they have survived undamaged, an interpretation as grave-goods from burials alongside the limes-road following the Rhine is just as likely

Finds *(b)* St 7, *(d)* St 129, xx, *(e)* xx, *(j)* xx, *(m)* xx
Collection private

122 Driel – Rijndijk
Finds *(a)* Drag 27?, stamp PE[]O (see ch. 6.2.1), x, *(d)* St 110B, xx, *(e)* x, *(g)* 1st-century bronze coin, *(l)* bronze fibula, iron spearhead
Collection GAS
Literature AN 1958, 166

123 Driel – Oldenhof
Situation a.s.s.
Finds *(a)* Drag 37, Ch 320, Drag 33, Drag 45, xxx, *(b)* St 1, St 2, St 23, Br 4, red-brown slip ware Hussong/Cüppers 1972, *Taf.* 20, 16 (see ch. 6.2.2 for a description), *(c)* Girth-beaker, HBW 52, T 1, T 2, Ch 342, *(d)* St 107, St 110B, St 111, St 129, St 138, St 147, Br 36, Br 37, Al 31, xxx, *(e)* St 201, St 210, St 218, St 219, Nb 89, Nb 98, Nb 112, T 4, Al 25, Al 27, Al 28, Al 29, Al 30, Al 34, T 3, xxx, *(f)* 'fine Nijmegen' pottery, *(j)* xxx, *(l)* iron and bronze fragments, *(m)* LIA, RA, *(n)* bracelets Haev ?, glass bead, *(o)* Bö B6, Bö B?, Bö D 9–12, Bö D 16, crescent-shaped rims, xxx, see also ch. 6.2.7, *(q)* slags
Collection private, RMO
Literature Modderman 1949, no. 65; JAWNN 1973, 15–17; Willems 1980a

124 Driel – Hemelrijk
Finds *(m)* LIA/RA
Collection private

125 Driel – Drielsche Veer
Situation Location reconstructed. See Brouwer 1982 for details
Finds See Holwerda 1931 and Brouwer 1982
Collection RMO
Literature Holwerda 1931; Kern 1962; Brouwer 1982

126 Arnhem – Meinerswijk
Situation a.s.s.
Excavation ROB 1979
Collection GAS
Literature AN 1980, 30–32; Willems 1980a, 1980b; see chapter 9

127 Elden – Klapstraat
Situation secondary location possible
Finds *(j)* xx
Collection private

128 Arnhem – Malburgen
Situation location reconstructed but unreliable
Finds *(l)* stone with inscription HERCULI MAGUSANO ET HAEVAE ULPI(us) LVPIO ET VLPIA AMMAVA PRO NATIS V(otum) S(olverunt) L(ibentes) M(erito)
Literature ER II, 167–168, no. 275

129 Arnhem – Immerlooplas
Finds *(l)* gladius, *(i)* XV (or LX?) on the tang
Collection private
Literature AN 1980, 128–29

130 Huissen – Loostraat I
Situation a.s.s.
Finds *(a)* xx, *(b)* xxx, *(d)* Br 36, xxx, *(e)* St 201, Nb 89 (a.o. late specimen), xxx, *(j)* xx, *(k)* wattle and daub, *(l)* glass? *(m)* RA
Collection GAS

131 Huissen – Loostraat II
Finds *(a)* Drag 31, stamps LVCANVSF, CATVLLVSF, BOVDVSFE, Drag 33, stamp MAININAF, *(b)* St 2, Br 17A, *(c)* HBW 52, *(d)* St 110B, St 113, St 146, xx, *(e)* Nb 104, Nb 111
Collection private
Literature AN 1978, 245

132 Huissen – Loostraat III
Finds *(m)* RA
Collection private

133 Huissen – Loostraat IV
Finds *(a)* Drag 37 (see ch. 6.2.1), xx, *(d)* St 110A
Collection private
Literature JAWNN 1980, 19

134 Huissen – Stadswal
Finds *(b)* x, *(d)* x, *(e)* x, *(o)* xxx, *(p)* scramasaxes, spearheads
Collection private
Literature AN 1964, 245–6; JAWNN 1974, 23; JAWNN 1982, 27–9

135 Huissen – Hazebergh
Situation Material found in secondary position, but possibly indicative of a fort which may have been situated in Huissen; it may, however, also have been transported to Huissen, for instance, from site no.

194 on the other side of the present Pannerdensch Kanaal
Excavation ROB 1951
Finds (only a portion of the original finds could be located and examined). *(a)* Gose 63, Drag 29, (see ch. 6.2.1), Drag 37 (see ch. 6.2.1), Ch 320 (see ch. 6.2.1), x, *(b)* x, *(c)* T 1, x, *(d)* Hofh 50, x, *(e)* St 201, St 210, Nb 89 (a.o. late specimens), Nb 104, Al 27, Al 28, Al 30, Al 34, Pirling 48, T 3, *(g)* dupondius Antoninus Pius, *(h)* LX[G], LEG XXX VVP, VEX. EX. G. INF, *(j)* xxx, *(m)* x, *(o)* x?
Collection GAS, private
Literature BROB 1951–2, 4; Bogaers/Rüger 1974, 73; Willems 1980a, 341–2

136 Angeren – Zakastraat
Finds *(o)* Bö B?, *(p)* glass beads
Collection GAS
Literature AN 1957, 183

137 Angeren – dorp
Situation a.s.s.
Finds *(o)* Bö D 9–12
Collection RMO
Literature Modderman 1949, no. 1

138 Angeren – Kamervoort
Situation a.s.s.
Finds *(e)* xx, *(m)* MIA, LIA, RA, *(n)* flint, *(q)* wattle and daub
Collection GAS

139 Angeren – De Keulstukken
Situation a.s.s.
Finds *(e)* St 201, xx, *(m)* MIA, LIA, RA, *(q)* wattle and daub, slag
Collection GAS

140 Huissen – De Kerkelanden
Situation a.s.s.
Finds *(b)* xx, *(c)* T 2, *(d)* St 109, St 129B, St 138, Br 36, xxx, *(e)* St 201, St 210, St 219, T 4, Nb 104, xxx, *(l)* iron object, *(m)* EIA, MIA, LIA, RA, *(q)* spindle whorl, wattle and daub
Collection private
Literature JAWNN 1976, 29–30

141 Huissen – De Plaat
Finds *(b)* xx, *(c)* xx, *(d)* Hofh 78, Br 36, xxx, *(e)* St 218, xxx, *(j)* xx, *(m)* MIA, LIA, RA, *(o)* Bö B, Bö D 9–12, *(q)* loomweights, spindle whorls, slags, wattle and daub
Collection private

142 Huissen – Bergerden
Situation a.s.s.
Finds *(d)* Ha 47, Hofh 78, xxx, *(e)* St 201, St 210, xxx, *(l)* glass bowl Isings 3, fragments of three bronze fibulae, *(m)* EIA, MIA, LIA, RA, *(n)* bracelets Haev, a.o. 7a, *(q)* loomweights, slags
Collection GAS, private

143 Bemmel – De Heuvel
Situation a.s.s.
Excavation RMO 1942
Finds *(a)* Drag 18/31, Drag 45, Nb 24D, xx, *(b)* St 16, xx, *(c)* Hofh 104, HBW 28, T 1, T 2, *(d)* St 109, St 110B, St 138, Hofh 78, Br 36, Br 37, xxx, *(e)* St 201, St 210, St 215, T 4, Nb 89, Nb 104, T 3, xxx, *(g)* sestertius Marcus Aurelius, *(l)* bronze fibula and handle, *(m)* LIA, RA, *(n)* bracelet Haev 3a, *(o)* Bö B, Bö D 9–12, *(q)* the kiln fragment mentioned by Braat is probably a sieve or cheese press
Collection RMO, private
Literature Modderman 1949, no. 5; Braat 1949

144 Bredelaar – Imkershof
Situation a.s.s.
Finds *(a)* xx, *(b)* xxx, *(c)* Br 16 C, T 1, *(d)* St 107, Hofh 78, Br 36, Br 37, xxx, *(e)* St 201, St 210, St 218, Br 21, Nb 89, Nb 104, xxx, *(m)* LIA, RA, *(o)* Bö D 9–12, Bö D 16
Collection RMO, GAS, private
Literature Modderman 1949, no. 23

145 Elst – Het Hofstuk
Situation a.s.s.
Finds *(c)* xx, *(d)* St 147, xx, *(e)* St 218, St 219, xxx, *(m)* LIA/RA, *(o)* Bö D 9–12
Collection RMO, GAS
Literature Modderman 1949, no. 26

146 Elst – Aamsestraat
Finds *(g)* aureus Traianus
Collection RMK
Literature AN 1971, 35

147 Elst – Aam I
Situation a.s.s.
Finds *(a)* Drag 37 (see ch. 6.2.1), Lud Sb, *(d)* St 110B, St 138, Br 36, xxx, *(e)* St 201, St 210, Nb 89, xxx, *(m)* LIA, RA, *(o)* Bö D 9–12
Collection RMO, GAS, private
Literature Modderman 1949, no. 25

148 Elst – Merm
Situation a.s.s.
Finds *(b)* St 10, xxx, *(c)* HBW 52, *(d)* xxx, *(e)* Nb 89,

xxx, *(l)* bronze and iron objects, *(m)* LIA/RA, *(q)* spindle whorls
Collection GAS
Literature Modderman 1949, no. 40

149 Ressen – Oudenhof
Situation a.s.s.
Finds *(m)* LIA, *(o)* Bö D 9–12
Collection private

150 Ressen – Kerkenhof
Situation a.s.s.
Excavation BAI 1927
Finds (The finds from the collection G. J. van Olst, presently in the GAS collection, have not been included. With the exception of the umbo (?) and probably some of the coins the origin of the finds is obscure) *(a)* Drag 37, Ch 320 (see ch. 6.2.1), Drag 31, Drag 45, Drag 51, Lud Sl, xx, *(b)* xx, *(c)* Girth-beaker, HBW 27?, HBW 54?, HBW 55, T 1, T 2, Ch 342?, xx, *(d)* Hofh 50, Hofh 78, Br 37, xxx, *(e)* St 201, St 210, Br 9, Br 12C, Nb 89, Al 25?, Al 27, Al 28, Al 29, xxx, *(g)* x?, *(j)* xxx, *(k)* tubulus?, *(l)* bronze umbo (?) with Medusa-head, *(m)* LIA, RA, *(n)* bracelets Haev 3a, 3b, 7a, 7c, *(o)* Bö D 9–12, *(q)* spindle whorl, wattle and daub
Collection RMO, GAS, private
Literature Van Giffen/Evelein 1928; Modderman 1949, no. 9; *Gelre* 1958, X

151 Ressen – De Woerdt
Situation a.s.s.
Excavation RMO 1941, 1943, 1948
Finds *(a)* Drag 37 (see ch. 6.2.1), Drag 38, xxx, stamp CIRRVSF, *(b)* St 1, St 17, Br 4, xxx, *(c)* HBW 52, HBW 55, T 1, T 2, x, *(d)* St 111, St 138, St 146, Br 36, xxx, *(e)* St 201, St 202, St 210, Nb 89, Nb 104, St 214, St 219, T 4?, xxx, *(g)* as Augustus, bronze coin (barbaric imitation ?) Constantius II, *(j)* xxx, *(l)* aucissa fibula, bronze wire fibulae, silver plated bronze button, bronze needle, other bronze and iron objects, *(m)* MIA, LIA, RA, *(n)* bracelets Haev 3b, *(q)* loomweights, net-sinkers, spindle whorls, kiln (Most, but not all of the finds of the RMO excavations have been included.)
Collection RMO, private
Literature Braat 1949

152 Oosterhout – Griftdijk I
Situation a.s.s.
Finds *(a)* xx, *(b)* x?, *(d)* xxx, *(e)* T 3, xxx, *(m)* LIA, RA, *(o)* Bö B 7–8, Bö D 9–12, xxx
Collection GAS, private

153 Oosterhout – Griftdijk II
Finds *(a)* Drag 18/31, Drag 45, *(b)* xx, *(d)* St 111, *(e)* St 218
Collection GAS

154 Oosterhout – Hoge Hof
Situation a.s.s.
Finds *(a)* Drag 27, stamp DONTIOIIIC (see ch. 6.2.1), Drag 45, xx, *(b)* xx, *(d)* St 108, xx, *(e)* St 210, St 219, Nb 89, Nb 104, xx, *(f)* lamp with Christ-monogram, *(g)* as Traianus *(i)* VV on Drag 27, *(l)* iron spearhead, bronze and lead weight, *(m)* MIA?, LIA, RA, *(o)* Bö D 9–12
Collection RMO, RMK
Literature Modderman 1949, no. 44

155 Oosterhout – Waaiensteinkolk
Finds *(b)* xx
Collection private
Literature Modderman 1949, no. 62

156 Lent – Steltsestraat I
Situation a.s.s.
Finds *(a)* Drag 45, Ch 320, xx, *(b)* xx, *(c)* T1, T2, xx, *(d)* Hofh 78, xxx, *(e)* St 201, Al 27, Al 28, xxx, *(l)* whetstone, *(m)* LIA/RA, *(o)* Bö B?, Bö D 9–12, crescent-shaped rims, *(p)* hand-made ware, *(q)* wattle and daub
Collection RMO, GAS
Literature Modderman 1949, no. 39
Other data The finds include new material found by R. S. Hulst early in 1983, when the Appendices had already been printed. The site should have appeared on Appendix 4, where it is lacking because the earlier finds did not include late-Roman sherds, and on Appendix 2 the symbol for 'settlement' should have been used.

157 Lent – Steltsestraat II
Finds *(g)* follis Justinianus I
Collection GAS

158 Lent – Het Laauwik I
Situation a.s.s. The finds include unpublished late-Iron Age/early-Roman material (pers. comm. R. S. Hulst, 1983). The site should, therefore, have been indicated on Appendices 1 and 2
Excavation ROB/IOGA 1972, ROB 1975
Collection GAS, ROB
Literature JROB 1972, 23–24; JAWNN 1972, 14–16; JROB 1975, 20–21; Hulst 1973

159 Lent – Het Laauwik II
Excavation ROB 1972, 1975

Collection ROB
Literature JROB 1972, 23–24; Hulst 1973; JROB 1975, 20–21

160 Lent - Waalbrug
Situation The finds are probably in secondary position and presumably indicate an eroded settlement on the northern bank of the Waal
Finds *(a)* xx, *(c)* xxx, *(d)* St 109, St 110B, *(e)* xxx, *(g)* two bronze coins Antoninus Pius, *(j)* xxx
Collection private

161 Bemmel – Cuperswei
Situation Pleistocene sand
Finds *(e)* T 3, xx, *(o)* Bö D 9–12
Collection private

162 Bemmel – Dorpstraat
Finds *(o)* Bö B 1a
Collection private
Literature AN 1971, 125

163 Bemmel – De Plak I
Finds *(d)* xx, *(m)* RA
Collection Oudheidkamer Bemmel, private
Literature Bredie (no date)

164 Bemmel – De Plak II
Finds *(b)* xx, *(e)* xx, *(m)* LIA, RA
Collection private
Literature Bredie (no date)

165 Bemmel - De Plak III
Excavation ROB 1971
Finds *(m)* MIA, LIA, *(n)* potters kiln
Collection ROB
Literature JROB 1971, 20; JAWNN 1971, 30–33; Bloemers/Hulst, in prep.
Other data C14 dates of 2250 ± 55 and 2150 ± 50 BP, see Lanting/Mook 1977, 149

166 Bemmel – 't Hoog
Finds *(e)* St 210, *(m)* LIA, RA, *(q)* bronze fragment
Collection private
Literature Bredie (no date)

167 Loenen – dorp
Situation a.s.s.
Finds *(m)* RA?, *(o)* Bö D 9–12
Collection RMO
Literature Modderman 1949, no. 52

168 Bemmel – Baal
Situation a.s.s.
Finds *(a)* xx, *(b)* Nb 33A, *(c)* HBW 55?, *(d)* St 110A, St 129B, St 132B, Hofh 78, xxx, *(e)* St 210, St 219, T 4, Nb 89, Nb 104, Gose 539, Al 27, Al 28, Al 30, T 3?, *(m)* LIA, RA, *(o)* Bö D 9–12, *(q)* spindle whorl
Collection RMO, private
Literature Modderman 1949, no. 8

169 Haalderen – Zandheuvel
Situation a.s.s.
Finds *(e)* xx, *(g)* republican as, *(m)* LIA/RA, *(o)* xx
Collection RMO
Literature Modderman 1949, no. 7

170 Flieren – De Wildeman
Situation a.s.s.
Finds *(m)* RA?, *(o)* Bö D 9–12
Collection RMO
Literature Modderman 1949, no. 14

171 Flieren – dorp
Situation a.s.s.
Finds *(d)* xx, *(e)* St 201, xx, *(o)* Bö D 9–12, *(p)* gold coin tremissis Anastasius
Collection RMO
Literature Modderman 1949, no. 12

172 Boerenhoek – Het Meer
Situation a.s.s.
Finds *(d)* Hofh 78, xxx, *(e)* St 201, St 218, Nb 89, xxx, *(m)* LIA, RA, *(q)* wattle and daub, fragment of lead
Collection RMO, private
Literature Modderman 1949, no. 3

173 Flieren – De Lohof
Situation a.s.s.
Finds *(d)* Hofh 78, St 147, Br 36, Br 37, xxx, *(e)* St 201, St 210, Nb 89, Nb 104, xxx, *(m)* LIA, RA
Collection RMO, GAS
Literature Modderman 1949, no. 13

174 Gendt – Angerensestraat
Finds *(b)* St 2, *(d)* St 110B, *(e)* Nb 104, St 218, *(f)* lamp Evelein type A2, *(h)* EVCAR on lamp
Collection private
Literature AN 1977, 255

175 Doornenburg – Roswaard
Finds *(g)* as Domitian
Collection GAS
Literature AN 1957, 213

176 Doornenburg – De Kemp
Situation a.s.s.
Finds *(e)* xx, *(m)* LIA/RIA

Collection RMO
Literature Modderman 1949, no. 6

177 Doornenburg – De Woerd
Situation a.s.s.
Finds *(b)* xx, *(d)* xx, *(e)* St 218/Al 34?, Al 28, xxx, *(l)* fragment of marble, *(m)* LIA, RIA, *(o)* Bö D 9–12, *(q)* spindle whorls
Collection GAS, RMO, private
Literature Modderman 1949, no. 2

178 Gendt - De Hoge Hof
Situation a.s.s.
Finds *(d)* xx, *(e)* St 210, Nb 104, Al 34?, xxx, *(g)* coins, a.o. Valentinianus ?, *(m)* LIA, RA, *(o)* xx
Collection RMO, private
Literature Pleyte 1877–1903 (Batavia), 40; Modderman 1949, no. 18

179 Gendt – Huis te Gendt
Situation a.s.s.
Finds *(e)* xx, *(m)* LIA/RA
Collection RMO, private
Literature Modderman 1949, no. 19

180 Pannerden – Rijndijk
Situation a.s.s.
Finds *(d)* x, *(e)* x, *(m)* LIA, RA
collection RMO/GAS?

181 Pannerden – Lobberdensche Waard
Finds *(d)* x, *(e)* x, *(j)* xxx, *(l)* gladius
Collection GAS

182 Herwen – De Bijland
Situation All material was found during dredging activities. The castellum Carvium has probably been eroded by a post-Roman branch of the Rhine, but the finds, including large numbers of building materials (tuff, limestone, etc.), indicate its original location. They should, however, not be interpreted as an indication of the exact location of the 'Drusus-dam', which was probably a little further upstream
Literature Bogaers/Rüger 1974, 90–92; Willems 1980a, 339–40; chapter 3

183 Lobith – Tolkamer
Situation Reconstructed location of the 'Drusus-dam' on the (reconstructed) spot where the Waal branched off the Rhine. An early-Roman military camp in the immediate surroundings (virtually no early material was found in De Bijland) is therefore also possible
Literature Bogaers/Rüger 1974, 90–92; Willems 1980a, 339–40; chapter 3

184 Feldhausen – Eltense heide
Finds *(m)* EIA, MIA?, LIA
Collection GAS

185 Didam – Aalsbergen
Finds *(m)* EIA, MIA?, LIA, *(n)* spindle whorl, wattle and daub
Collection RMO, GAS

186 Loil – Loilsche Veld
Finds *(o)* Bö D 1
Collection GAS
Literature Modderman 1953a, 47

187 Loil – Loilscheweg
Finds *(m)* LIA
Collection RMO

188 Babberich – Kamphuizen
Situation a.s.s.
Finds (e) Nb 89, *(m)* LIA/RA
Collection RMO

189 Zevenaar – Oud Zevenaarseweg
Situation a.s.s.
Finds *(a)* Drag 37, *(e)* xxx, *(j)* xx, *(l)* fragment of bronze fibula, fragment of bronze pan, late-Roman bronze hairpins, *(m)* MIA, LIA, RA, *(n)* bracelet Haev 7a, *(q)* spindle whorls, wattle and daub, loomweight
Collection private

190 Zevenaar – Steenfabriek
Situation a.s.s.
Finds *(d)* xx, *(e)* xx, *(m)* LIA/RA
Collection RMO
Literature Modderman 1953a, 47

191 Groessen – De Hogenhof
Situation a.s.s.
Finds *(m)* LIA, *(q)* wattle and daub
Collection GAS
Literature Janssen 1844, 230–231

192 Groessen – Woerd
Situation a.s.s.
Finds *(a)* Drag 37, *(d)* xxx, *(e)* St 201, xxx, *(j)* xxx, *(m)* LIA, RA, wasters, Anglo-Saxon urn (?), *(o)* Bö D 9–12, crescent-shaped rims, *(q)* tuff, basalt-lava, wattle and daub, slags

Collection RMO, GAS
Literature Janssen 1844, 231–232

193 Groessen – De Hofakker
Situation a.s.s.
Finds *(m)* LIA/RA
Collection RMO

194 Loo – Loowaard
Situation All material was found as a result of dredging activities. The probable fort at this site must have been eroded by the Rhine
Finds (a) Drag 29, Drag 30, Drag 37 (see ch. 6.2.1)[17], Ch 320 (?), Drag 18, Drag 18/31, Drag 31, stamps VENICAR[and IVVI/, Drag 27, Drag 33, stamps SEXTIM and rosette, Drag 45, xxx, *(b)* St 2, St 10, xxx, *(c)* Ha 91A, Br 16C, HBW 55, T1, T2, xxx, *(d)* Ha 48, St 107, St 109A, St 109B/110A, St 110B, St 111, St 115, St 129A, St 132A, St 132B, St 138, St 146, Nb 79A, Ha 59, Br 36, Br 37, Hofh 78, St 147, xxx, *(e)* St 201, St 205, St 210, St 213B, St 218, St 219, T4, Nb 89 (including late specimens), Nb 103, Nb 104, Nb 112A, Gose 539, Al 27 (?), Al 34, T3, xxx, *(f)* 'fine Nijmegen' pottery (?), *(h)* QMR, QIMFN and MR on handles St 138,]XXIIPR[and fragment of square stamp on tegula, LEGXX[ ]AC on imbrex, *(i)*]MAR[on Drag 27,]IIZV[]V[]IS on Drag 31, *(j)* xxx, *(k)* tuff, *(l)* net-sinkers, glass, querns, bronze saucepan-handle, sieve, belt-buckle, fibula Van Buchem 1941, Pl.xxii-10, bronze wire fibula, iron horse-trappings, rings, scissors and axe, *(m)* EIA?, RA, *(o)* Bö B 1, Bö D 9–12, crescent-shaped rim, *(p)* iron spearhead Bö A4, bead, *(q)* iron knife.
Collection GAS, private
Literature Willems 1980a, 341–2

195 Westervoort – Schans
Situation a.s.s.
Finds *(e)* xx, *(m)* RA
Collection RMO
Literature Modderman 1953a, 47

196 Duiven – Achterstehoek
Situation a.s.s.
Finds *(a)* Drag 37, *(d)* xx, *(e)* xx, *(j)* xx?, *(m)* MIA, LIA, RA, wasters, *(n)* parts of kiln, pottery-slag, *(q)* basalt-lava, whetstone
Collection RMO, GAS

17 Most of the decorated terra sigillata and the stamps could not be included in chapter 6.2.1. They were only available for study for a short time.

197 Duiven – dorp
Finds *(b)* xx, *(c)* xx, *(d)* xx, *(e)* Nb 89, xxx, *(g)* bronze coin Traian, *(m)* EIA, MIA, LIA, RA, *(q)* loomweight, slags, wattle and daub
Collection RMO, GAS, private
Literature Janssen 1844, 229–230; JRMO 1872, 5; AN 1958, 166

198 Duiven – De Eng
Finds *(m)* RA
Collection RMO

199 Lathum – Lathumse Waard
Finds *(l)* net-sinkers (reused tegulae)
Collection GAS

200 Boven Leeuwen – Den Hogen Berg
Situation a.s.s.
Finds *(a)* xx, *(d)* xx, *(e)* Nb 89, xx, *(m)* LIA, RA, *(o)* xx ?
Collection RMO
Literature Modderman 1951, no. 27; Peddemors 1978, no. 124

201 Boven Leeuwen – Riemsdijkse Bouwing
Situation a.s.s.
Finds *(d)* xx, *(e)* xx, *(m)* LIA/RA
Collection RMO
Literature Modderman 1951, no. 26; Peddemors 1978, no. 123

202 Puiflijk – Het Hout
Finds *(c)* xx, *(d)* xx, *(m)* LIA/RA
Collection RMO
Literature Modderman 1951, no. 49: Peddemors 1978, no. 117

203 Puiflijk – De Langenberg
Finds *(d)* St 132A?, Hofh 78, xx, *(e)* xx, *(m)* LIA/RA
Collection RMO
Literature Modderman 1951, no. 46; Peddemors 1978, no. 122

204 Puiflijk – Den Bult
Finds *(b)* St 2, xx, *(e)* St 210
Collection RMO
Literature Modderman 1951, no. 48; Peddemors 1978, no. 121

205 Puiflijk – Mariënhoef
Finds *(m)* LIA/RA
Collection RMO
Literature Modderman 1951, no. 47; Peddemors 1978, no. 120

206 Puiflijk – De Heuf I
Situation Pleistocene sand (river dune)
Finds *(d)* xxx, *(e)* St 210, St 213A, xxx, *(m)* LIA, RA
Collection private
Literature Peddemors 1978, no. 119

207 Puiflijk – De Heuf II
Finds *(b)* xx, *(d)* xx, *(e)* xx, *(m)* LIA, RA
Collection RMO
Literature Modderman 1951, no. 44; Peddemors 1978, no. 116

208 Puiflijk – Kerkstraat
Finds *(m)* RA?
Collection private
Literature Knippenberg 1964, 17; Peddemors 1978, no. 114

209 Druten – Roodhekkenpas
Situation a.s.s.
Finds *(b)* xx, *(e)* xxx, *(m)* MIA, LIA, RA, *(n)* bronze fibula 'of Middle-La Tène scheme', iron 'Ringkropfnadel', bracelets Haev 6a, 6c, 7a, 7b, bronze bracelet
Collection RMK, AWNN, private
Literature Peddemors 1978, nos. 107, 108, 109

210 Druten – Hof Westerhout
Finds *(m)* LIA
Collection private
Literature Peddemors 1978, no. 106

211 Druten – Boldershof
Excavation ROB 1973
Finds *(l)* Roman barge
Collection RIJP (Ketelhaven)
Literature Hulst/Lehmann 1974; Peddemors 1978, no. 105

212 Druten – Brouwerstraat
Situation a.s.s.
Finds *(a)* Drag 37 (see ch. 6.2.1), Drag 18, Drag 27, Drag 33, xx, *(b)* St 7, xxx, *(c)* T 1, xx, *(d)* St 129A, Br 36, xxx, *(e)* St 210, Nb 89 (early specimens), Nb 104, St 219, xxx, *(g)* dupondius ?, *(i)* Λ on jug sub *d*, *(j)* x, *(k)* tuff, slate, *(l)* gem engraved with figures of Victoria and Fortuna, iron fragments, *(m)* LIA, RA (including waster from kiln?), *(o)* Bö D 9–12
Collection RMK, AWNN, RMO
Literature AN 1970, 67–68; Peddemors 1978, nos. 101, 102

213 Druten – dorp
Finds *(g)* denarius, *(m)* LIA, RA
Collection RMO, private
Literature Modderman 1951, no. 39; Peddemors 1978, no. 103

214 Druten – Klepperhei
Situation a.s.s.
Excavation ROB 1975–79
Collection RMO, AWNN, ROB[18]
Literature Modderman 1951, no. 41; Peddemors 1978, nos. 93–100; Hulst 1978; Hulst 1980

215 Puiflijk – Scharenburgsestraat
Situation a.s.s.
Finds *(d)* xx, *(e)* St 218
Collection RMO
Literature Modderman 1951, no. 45; Peddemors 1978, no. 113

216 Puiflijk – De Hosterd
Situation a.s.s.
Finds *(e)* xx, *(m)* LIA/RA
Collection RMO
Literature Modderman 1951, no. 51; Peddemors 1978, no. 112

217 Druten – Gelenberg
Situation Pleistocene sand (river dune)
Finds *(a)* x, *(b)* Br 5, *(c)* T 1, *(o)* Bö B 1a, Bö B?
Collection RMO, museum Den Bosch, private
Literature Modderman 1951, no. 38; AN 1963, 2; Peddemors 1978, nos. 90–92

218 Afferden – Het Hoog
Finds *(d)* xx, *(e)* xx, *(m)* LIA, RA, *(q)* spindle whorl
Collection RMO, GAS, private
Literature Modderman 1951, no. 36; Peddemors 1978, nos. 86–88

219 Deest – Grotestraat
Situation a.s.s. (not recorded by soil-survey). See site no. 220
Finds *(a)* Drag 37, Drag 18/31, Drag 31, *(b)* St 2, *(c)* T 1, HBW 52?, xx, *(d)* Hofh 78, xxx, *(e)* St 210, Nb 89, xxx, *(j)* xxx, *(k)* tuff, opus signinum, fragment of wall painting, *(m)* RA, *(o)* rim Stampfuss 1939, fig. 23, 1
Collection AWNN
Literature JAWNN 1972, 11; Peddemors 1978, nos. 79, 81

18 Especially older finds (in the RMO) contain late-Iron Age material which was not, or not convincingly (see Hulst 1978), present at the site of the excavation. For an overview of the different findspots see Peddemors 1978.

220 Deest – De Hosterd
Situation a.s.s. (not recorded by soil-survey). This site and site no. 219 may constitute one settlement
Finds *(c)* xx?, *(d)* St 132?, xxx, *(e)* St 201, xxx, *(j)* xxx, *(k)* tuff, *(m)* LIA, RA
Collection AWNN
Literature JAWNN 1973, 24; Peddemors 1978, no. 83

221 Deest – Vriesseweg
Situation a.s.s. (not recorded by soil-survey). The site may be part of the settlement constituted by site 219 (and 220?)
Finds *(d)* x, *(e)* x, *(m)* LIA/RA
Collection private

222 Winssen – Het Oude Veerhuis
Situation a.s.s.
Excavation RMO 1939, IOGA 1972
Finds *(a)* Drag 37, Drag 18, Drag 18/31, Drag 32, Drag 45, xx, *(b)* St 2, St 5?, Br 3A, Br 18, xxx, *(c)* T 1, *(d)* Hofh 78, St 147, St 146, Br 36, Br 37, xxx, *(e)* St 201, St 210, St 218, St 219, Nb 89, Nb 104, Nb 120A, xxx, *(f)* HNW 35b, *(h)* EXG [ERINF] (retro) on tegula, *(j)* xxx, *(k)* tuff, limestone, opus signinum, fragments of wall-painting, tubuli, hypocaust-tiles, wattle and daub, *(l)* iron key and fragments, a.o. knife, *(m)* RA
Collection RMK, RMO
Literature JRMO 1939, 5; Modderman 1951, no. 25a; Peddemors 1978, no. 73

223 Winssen – De Hoek
Situation a.s.s.
Finds *(a)* Drag 29
Collection AWNN

224 Winssen – Geerstraat
Situation a.s.s.
Finds *(a)* Drag 37 (see ch. 6.2.1), *(b)* St 1, *(m)* LIA/RA
Collection RMO
Literature Modderman 1951, no. 22; Peddemors 1978, no. 70

225 Winssen – De Hosterd
Situation a.s.s.
Finds *(m)* LIA/RA
Literature Modderman 1951, no. 25; Peddemors 1978, no. 61

226 Winssen – De Grote Woerd
Situation a.s.s.
Finds *(a)* xx, *(d)* xxx, *(e)* St 201, xx, *(g)* bronze coin 4th century, *(j)* xx, *(m)* EIA, MIA, LIA, RA, *(q)* spindle whorls, (pottery?-)slag, wattle and daub
Collection GAS, RMO, AWNN, private
Literature Modderman 1951, no. 18; Peddemors 1978, nos. 58–60

227 Ewijk – De Wolfsdarm
Situation a.s.s.
Finds *(a)* xx, *(b)* St 2, xx, *(c)* HBW 31B, T 1, *(d)* St 109, St 129B, Hofh 78, xxx, *(e)* St 201, St 210, Nb 89, Nb 104, St 216, St 218, xxx, *(j)* xxx, *(m)* EIA, MIA, LIA, RA, *(n)* bracelets Haev 3a, 7a, *(q)* slag, wattle and daub
Collection RMO, GAS, AWNN, private
Literature Modderman 1951, no. 17; Peddemors 1978, no. 55

228 Ewijk – Ewijksche Velden I
Situation a.s.s. (not recorded by soil-survey)
Finds *(d)* Hofh 78, *(e)* xxx, *(m)* EIA, LIA, RA, *(n)* bracelets Haev 3a, 7a
Collection GAS, private
Literature Peddemors 1978, no. 54

229 Ewijk – Ewijksche Velden II
Finds *(a)* Drag 37, xx, *(c)* xx, *(d)* Br 37
Collection GAS
Literature Peddemors 1978, no. 57

230 Ewijk – De Woerden
Situation a.s.s.
Finds *(e)* xxx, *(m)* LIA, RA, *(n)* bracelets Haev 6c, 7a
Collection AWNN, private
Literature Modderman 1951, no 16; Peddemors 1978, nos. 52–53

231 Ewijk – Ewijksche Velden III
Situation a.s.s. The finds discussed here do not constitute a nucleated settlement but are isolated finds between and around site nos. 230 and 232, possibly indicating some form of dispersed settlement.
Finds *(a)* Drag 37, *(b)* xxx, *(c)* Br 16C, HBW 52, T 1, xx, *(d)* Br 36, Br 37, *(e)* St 201, St 210, Nb 104, *(m)* MIA, LIA, RA, *(n)* bracelet Haev 3a
Collection AWNN, private
Literature AN 1971, 1–2; Peddemors 1978, nos. 48(?), 49(?), 51

232 Ewijk – Ewijksche Velden IV
Situation a.s.s. (not recorded by soil-survey)
Excavation ROB 1973–74, 1977
Collection ROB
Literature AN 1971, 2; JROB 1973, 15–16; 1974, 9–10; 1977, 31–32; Peddemors 1978, nos. 47, 50

233 Ewijk – De Woerdjes
Situation a.s.s.

Finds *(a)* Drag 37 (see ch. 6.2.1), Drag 27, Drag 31, Drag 32?, Drag 33, xxx, *(b)* Br 3A, xxx, *(c)* HBW 52, T1, *(d)* xxx, *(e)* St 201, Nb 104, xxx, *(m)* EIA?, MIA, LIA, RA, *(n)* bracelet Haev 3a
Collection AWNN, RMO, GAS, private
Literature Modderman 1951, no. 15; Peddemors 1978, no. 46

234 Ewijk – De Hoge Woerd
Situation a.s.s.
Finds *(a)* Drag 37 (see ch. 6.2.1), Drag 15/17, Drag 18/31, Drag 31, Drag 33, Drag 45, xxx, *(b)* St 2, St 4, xxx, *(c)* HBW 27, HBW 52, HBW 55, T 2, xxx, *(d)* Ha 45, Hofh 51, St 110, St 129, Hofh 78, Nb 69?, Br 36, xxx, *(e)* St 201, St 202, St 210, St 218, St 219, Nb 89 (a.o. late specimens), Nb 104, Nb 112A, Gose 539, Al 27, T 3, *(f)* 'fine Nijmegen' pottery, Hofh 26C, *(g)* 1/2 centenionalis Theodosius and Valentinianus II, *(i)* X on Drag 27, *(j)* xxx, *(k)* tuff, wattle and daub, *(l)* glass, bronze wire fibula, 'omega' fibula, iron and bronze fragments, *(m)* MIA, LIA, RA *(n)* bracelet Haev 7a, *(o)* Bö C 2, Bö D 9–12, *(q)* spindle whorl, loomweight
Collection AWNN, GAS, RMO
Literature Modderman 1951, no. 13; Peddemors 1978, no. 43

235 Ewijk – De Ooigraaf
Situation a.s.s.
Finds *(a)* Drag 31, xx, *(b)* xx, *(c)* Br 16C, xx, *(d)* St 109, xxx, *(e)* St 201, St 210, St 218, Nb 89, Nb 104, T 4, xxx, *(j)* xx, *(l)* iron knife, *(m)* LIA, RA, *(n)* bracelets Haev 7a, *(q)* spindle whorl, loomweights, slag, wattle and daub
Collection AWNN, RMO, GAS
Literature Modderman 1951, no. 12; Peddemors 1978, no. 42

236 Ewijk – De Blauwpomp
Situation a.s.s. (not recorded by soil-survey)
Finds *(d)* Br 37, *(e)* xxx, *(m)* LIA/RA
Collection private
Literature Peddemors 1978, no. 41

237 Ewijk – Woerd bij Den Alst
Situation a.s.s.
Finds *(c)* T 2, *(d)* xx, *(e)* St 219, xxx, *(m)* LIA, RA, *(o)* crescent-shaped rim
Collection RMO, AWNN
Literature Modderman 1951, no. 11; Peddemors 1978, no. 39

238 Ewijk – Den Alst
Situation a.s.s.
Finds *(e)* St 210, Nb 104, xx, *(m)* LIA/RA
Collection RMO, AWNN
Literature Modderman 1951, no. 10; Peddemors 1978, no. 38

239 Ewijk – De Grote Aalst
Situation a.s.s.
Finds *(a)* Drag 37, Drag 27, Drag 33, Drag 45, xx, *(b)* St 2, St 10, Br 4, Br 5A, Br 6A?, xxx, (c) terra nigra plates HBW 81 and 87, HBW 27A, Br 16C, T 1, T 2, xx, *(d)* Hofh 50/St 107, St 110A, St 131, St 146, c. Br 35, St 138, Hofh 78, Br 36, Br 37, xxx, *(e)* St 201, St 210, St 218, St 219, T 4, Nb 89 (a.o. late specimens), Nb 104, Br 20, Al 28, xxx, *(g)* coins[19], *(i)* IIII on T 1 (before firing), VERVS (?; read by J.E. Bogaers) on Drag 45 or 43, *(j)* xxx, *(k)* tuff, limestone, marble, opus signinum, small stones of a black and white mosaic, fragments of wall painting, *(l)* iron fragments, fibulae[19], glass fragments, *(m)* MIA, LIA, RA, *(q)* spindle whorls
Collection RMO, GAS, AWNN, private
Literature Modderman 1951, no. 9; Bergevoet 1970; Peddemors 1978, nos. 30, 31; JROB 1981, 107–8

240 Ewijk – dorp
Situation a.s.s.
Finds *(o)* Bö D 9–12
Collection RMO
Literature Modderman 1951, no. 8; Peddemors 1978, no. 32

241 Ewijk – Doddendaal
Situation location reconstructed
Finds *(c)* HBW 17, HBW 18, *(o)* Bö B 3b, Pirling 174
Collection RMO
Literature Holwerda 1925, 178; Peddemors 1978, no. 501

242 Ewijk – Schoenakerweg
Situation Finds from profile with two parallel ditches, probably indicating a (secondary) Roman road
Finds *(e)* xx, *(j)* xx
Collection private
Literature JAWNN 1976, 25–27; Peddemors 1978, no. 28

19 From 1980–1981, well over 300 coins and a number of fibulae were found at this site by means of a metal-detector. The majority of the coins dates to the 4th century but earlier coins, including even a Celtic rainbow-cup, are also present.

243 Ewijk – De Aalst
Finds *(e)* xx
Collection GAS
Literature Peddemors 1978, no. 27

244 Beuningen – De Heuve
Situation a.s.s.
Finds *(a)* Drag 37, Ch 320 (see ch. 6.2.1), Gose 98–104, Drag 18/31, Drag 31, Drag 43/45, xxx, (b) St 2, xxx, *(c)* Ha 91A, T1, T2, xx, *(d)* St 129B, Br 36, Br 37, St 147, xxx, *(e)* St 201, T4, St 219, Nb 89 (a.o. late specimens), Nb 104, Al 25, Al 27, Trier-Umbaukeramik 46A, xxx, *(f)* 'fine Nijmegen' pottery, *(g)* Celtic copper rainbow-cups, Avaucia-coin, as (probably) Commodus, centenionalis Gratianus, 1/2 centenionalis Constantius II, aes-IV Valentinianus II, solidus Arcadius, *(h)* [VEX] BRI[T] on tegula, *(j)* xxx, *(l)* bronze wire fibula, late-Roman bronze and silver hairpin, glass, *(m)* EIA, MIA, LIA, RA, *(n)* bracelets Haev 3a, 3b, 7a, 7b, 7d, glass beads, bronze bracelets, *(o)* Bö C?, Bö D 9–12, *(p)* glass bead, *(q)* spindle whorls
Collection AWNN, GAS, RMO, private
Literature Modderman 1951, no. 7; JAWNN 1975, 24–25; Peddemors 1978, no. 25

245 Beuningen – De Hosterd[20]
Situation a.s.s.
Finds *(a)* Drag 37, *(b)* xx, *(d)* xx, *(e)* Nb 89, xxx, *(g)* sestertius Marcus Aurelius, *(j)* xxx, *(k)* tuff, *(m)* MIA, LIA, RA
Collection AWNN, GAS, private
Literature Modderman 1951, no. 6; Peddemors 1978, nos. 18–20; JAWNN 1981, 38–41

246 Beuningen – De Tinnegieter[20]
Situation a.s.s.
Excavation ROB 1980, 1983
Collection ROB, RMO, AWNN
Literature Modderman 1951, no. 3; Peddemors 1978, nos. 21, 22; JAWNN 1979, 16–17; JROB 1980, 33; JAWNN 1981, 38–41; JROB 1982, 32–3

247 Beuningen – school
Finds *(p)* see Ypey 1973b
Collection RMK
Literature AN 1970, 150–151; Ypey 1973b; Peddemors 1978, no. 15

248 Beuningen – dorp
Situation a.s.s.
Finds *(a)* Drag 37 (see ch. 6.2.1), xx, *(b)* St 6, xx, *(c)* Br 36, Br 37, xxx, *(e)* St 201, St 202, St 218, Nb 89 (early specimens), Nb 104, xxx, *(j)* xx, *(k)* tuff *(m)* RA, *(o)* Bö D 9–12, *(q)* wattle and daub
Collection RMO, GAS, AWNN, private
Literature Modderman 1951, no. 2; Peddemors 1978, nos. 12 (partly),[21] 14, 15 (partly), 16

249 Beuningen – Christinastraat
Situation a.s.s.
Finds *(d)* Hofh 78, St 129A, *(e)* St 201, Nb 104, *(j)* x, *(k)* wattle and daub
Collection Municipality of Beuningen
Literature Peddemors 1978, no. 13

250 Lienden – Reekstraat
Situation a.s.s.[21]
Finds *(a)* Drag 37 (see ch. 6.2.1), Drag 27, Drag 18/31, Drag 31, Drag 33, Drag 36, Drag 44, Drag 45, xxx, *(b)* St 1, St 2, Br 4B, Br 5A, xxx, *(c)* Ha 91A, HBW 13?, HBW 52, Br 6, T 1, T 2, waster from kiln, xxx, *(d)* St 107, St 110B, St 111, St 129, St 138, St 146, St 147, Hofh 78, Br 36, Br 37, xxx, *(e)* St 201, St 202, St 210, St 214B, St 218, St 219, Br 17?, Br 20, T 4, Nb 89, Nb 104, Al 29, Al 34?, xxx, *(f)* 'fine Nijmegen' pottery ?, *(i)*VV on Br 36 (before firing), *(j)* xxx, *(k)* tuff, limestone, *(l)* iron objects, a.o. key, iron fragments, bronze fragments, *(m)* MIA, LIA, RA, *(n)* bracelets Haev 7a
Collection AWNN, GAS, RMO, private
Literature Modderman 1951, 27 and no. 1; JAWNN 1971, 14–17; 1972, 9–10; 1973, 23; Peddemors 1975, nos. 72–74; Janssen 1975; Peddemors 1978, nos. 5–11 and 12 (partly)[22]

20 The Roman finds which occur on the northern part of the 'Tinnegieter' should be considered an indication of the most southern extension of the Roman settlement on the 'Hosterd'. The border between the two sites is more or less artificial. Normally, they would have been considered one settlement, but it seems that the Roman occupation was centred on the 'Hosterd' and that the early-Medieval occupation occurs mainly on the 'Tinnegieter'.

21 This is a very long a.s.s. Roman pottery was found all over its surface, but the spot indicated on the distribution maps is the location of the main (stone) building, probably dating from II–IIIA or IIIc. The MIA-finds are concentrated on a spot more to the west (see Janssen 1975, *afb.* 1; this is approximately Peddemors 1978, site no. 7).

22 Site no. 12 in Peddemors 1978 does not exist. The finds originate from sites nos. 250 (LIA-bracelets) and 248 (pottery).

251 Molencind – De Oude Hof
Situation a.s.s.
Finds *(a)* Drag 33, *(d)* Br 36/37, *(e)* St 210, St 219, Nb 89, Nb 112A, xx, *(m)* LIA/RA
Collection RMO
Literature Modderman 1951, no. 109; Peddemors 1978, no. 389

252 Blauwesluis – Munnikkenwoerd
Situation a.s.s.
Finds *(d)* St 147, xx, *(e)* St 210, St 216, Nb 104, *(m)* LIA/RA
Collection RMO
Literature Modderman 1951, no. 114; Peddemors 1978, no. 388

253 Altforst – dorp
Situation a.s.s.
Finds *(e)* xx, *(m)* LIA/RA
Collection RMO
Literature Modderman 1951, no. 119; Peddemors 1978, no. 392

254 Altforst – Heppert
Situation a.s.s.
Finds *(m)* LIA/RA
Collection RMO
Literature Modderman 1951, no. 120; Peddemors 1978, no. 391

255 Altforst – De Hoge Varen
Situation a.s.s.
Finds *(b)* xx, *(c)* xx, *(e)* xxx, *(m)* MIA, LIA/RA, *(o)* Bö D 9–12
Collection RMO
Literature Modderman 1978, no. 121; Peddemors 1978, no. 390

256 Horssen – De Hoge Beemdert
Situation a.s.s.
Finds *(e)* xx, *(m)* LIA, RA, *(q)* slag
Collection AWNN, RMO
Literature Modderman 1951, no. 58; Peddemors 1978, nos. 381, 382

257 Laak – De Kwaden Bongerd
Situation a.s.s. The location is not on the reconstructed river system and may be due to favorable local circumstances. It is also possible that the maps used for the reconstruction are not adequate at this point.
Finds *(m)* LIA/RA
Collection RMO
Literature Modderman 1951, no. 57; Peddemors 1978, no. 380

258 Horssen – west
Finds *(b)* xx, *(e)* xx, *(m)* LIA/RA
Collection RMO, GAS
Literature Modderman 1951, no. 53; Peddemors 1978, no. 353

259 Horssen – oost
Finds *(c)* T 2, *(d)* xx, *(e)* St 201, *(m)* LIA/RA, *(o)* Bö D 9–12
Collection RMO
Literature Modderman 1951, no. 52; Peddemors 1978, no. 352

260 Horssen – Het Hooge Veld
Finds *(d)* xx, *(e)* xx, *(m)* LIA/RA
Collection RMO
Literature Modderman 1951, no. 54; Peddemors 1978, no. 351

261 Horssen – Het Sunten
Finds *(a)* xx, *(b)* xx, *(c)* xx, *(d)* Hofh 78, St 138, Br 37, xx, *(e)* St 210, T 4, Nb 89 (a.o. late specimen), Nb 104, *(l)* iron fragments, *(m)* MIA, LIA, RA, *(n)* bracelets Haev 3b and 7a, *(q)* slags
Collection RMO, AWNN, GAS, private
Literature Modderman 1951, no. 68; Peddemors 1978, no. 343–345

262 Molenhoek – west
Finds *(c)* T 2, *(e)* St 201, Nb 89, xx, *(m)* LIA?, RA
Collection RMO
Literature Modderman 1951, no. 55; Peddemors 1978, no. 350

263 Molenhoek – Kloosterweg
Finds *(b)* Br 3A, Br 7B
Collection private
Literature Peddemors 1978, no. 349

264 Molenhoek – oost
Finds *(b)* xx, *(d)* Br 37, *(e)* St 201, St 219, xx, *(m)* LIA/RA
Collection RMO
Literature Modderman 1951, no. 56; Peddemors 1978, no. 348

265 Afferden – Schriksestraat
Finds *(e)* xx, *(m)* LIA/RA
Collection private

266 Horssen – De Kloosterberg
Situation Pleistocene sand (river dune)
Finds *(e)* St 201, xx, *(g)* coins, a.o. Faustina, *(m)* LIA/RA
Collection RMO
Literature Modderman 1951, no. 59; Peddemors 1978, no. 346

267 Bergharen – De Galgenberg
Finds *(o)* Bö D 9–12
Collection Municipality of Bergharen
Literature Peddemors 1978, no. 341

268 Bergharen – De Horst
Finds *(o)* Bö D 9–12
Collection GAS

269 Bergharen – De Bergen
Finds *(d)* xx, *(m)* LIA/RA
Literature AN 1964, 271; Peddemors 1978, nos. 330–331

270 Bergharen – Wijkse Veld
Situation Large settlement; the indicated spot is approximately in the center
Finds *(a)* Drag 37?, Ch 320?, Drag 33, Drag 44?, Drag 45, xx, *(b)* St 2, Br 4, xxx, *(c)* T 1, xx, *(d)* Hofh 78, Br 36, Br 37, xxx, *(e)* St 201, St 210, St 216, St 219, Nb 89, Nb 104, Nb 111, Al 27, Al 28, Al 29, Trier Umbaukeramik 39, T 3, *(i)* x on jug sub *d*, *(l)* iron, bronze and lead objects and fragments, *(m)* EIA, MIA, LIA, RA, *(n)* bracelets Haev 3b and 7a, bronze fibula of Early-La Tène scheme, bronze swans-neck pin, *(o)* Bö D 9–12, *(q)* loomweight, slags, crucible, wattle and daub
Collection AWNN, RMO, GAS, private
Literature Modderman 1951, no. 64; Peddemors 1978, nos. 322–329

271 Bergharen – Molenstraat
Finds *(o)* Bö B?, Bö D 9–12
Collection GAS
Literature Peddemors 1978, nos. 332–333

272 Bergharen – De Gerstkamp
Situation a.s.s.
Finds *(a)* Drag 31, xx, *(b)* xxx, *(c)* Br 16C, T 2, *(d)* Hofh 78, St 132B, St 138?, St 147, xxx, *(e)* St 201?, St 210, St 217, St 218, Nb 89, Nb 104, Al 27, *(m)* LIA, RA, *(n)* bracelet Haev 7a, *(o)* Bö D 9–12
Collection AWNN, RMO, private
Literature Modderman 1951, no. 72; Peddemors 1978, nos. 376–377

273 Hernen – De Hofstede
Situation a.s.s.
Finds *(d)* xx, *(e)* St 201, Nb 89, xxx, *(m)* LIA, RA
Collection RMO, AWNN
Literature Modderman 1951, nos. 66–67; Peddemors 1978, nos. 373–375

274 Hernen – Het Lang Stuk
Finds *(g)* sestertius Septimus Severus
Collection private
Literature Bogaers 1973–74; Peddemors 1978, no. 372

275 Hernen – De Wijnakker
Situation a.s.s. (not recorded by soil survey)
Finds *(a)* Drag 37, Drag 27?, Drag 31, Drag 45, xxx, *(b)* St 2, Br 5A, xxx, *(c)* HBW 52, HBW 81, T 1, T 2, *(d)* St 107, St 110B, St 111, St 129A, Hofh 78, Br 36, Br 37, xxx, *(e)* St 201, St 210, St 218, St 219, Br 21, T 4, Nb 89, Nb 104, xx, *(j)* xxx, *(k)* tuff *(l)* bronze wire fibula, bronze pin, iron fragments, *(m)* LIA, RA, *(n)* bracelets Haev. a.o. 3a, 3b, 7a, glass beads
Collection AWNN, private
Literature Peddemors 1978, nos. 369–371

276 Hernen – De Schrebbelaar
Finds *(g)* sestertius Hadrian
Collection MFB

277 Batenburg – dorp
Situation a.s.s.
Finds *(g)* several coins Vespasian-Severus Alexander?, *(m)* LIA/RA
Collection RMO
Literature Modderman 1951, no. 108; Peddemors 1978, nos. 378–379

278 Lienden – Kapelhof
Situation a.s.s.
Finds *(c)* Br 16C, *(d)* xx, *(e)* St 201, xx, *(m)* LIA/RA
Collection RMO
Literature Modderman 1951, no. 106; Peddemors 1978, no. 361

279 Lienden – In de Hammen
Situation Pleistocene sand (river dune)
Finds *(e)* xx, *(m)* LIA/RA, *(o)* Bö D 9–12
Collection RMO
Literature Modderman 1951, no. 105; Peddemors 1978, no. 359

280 Lienden – Polder Neerveld
Situation a.s.s.? (not recorded by soil-survey)
Finds *(a)* Drag 27, Drag 31, *(b)* St 2, xxx, *(c)* Br 16C,

T 2, *(d)* Br 36, Hofh 78, xxx, *(e)* St 201, St 210, T 4, Nb 89, Nb 104, xxx, *(j)* xx, *(m)* LIA, RA, *(n)* bracelet(s) Haev?, *(q)* loomweight
Collection AWNN, private
Literature Peddemors 1978, no. 368

281 Hernen – Leursche Veld
Finds *(e)* xx
Collection RMO
Literature Modderman 1951, no. 63; Peddemors 1978, no. 367

282 Lienden – Overloonse Hof
Finds *(m)* LIA/RA
Collection RMO
Literature Modderman 1951, no. 62; Peddemors 1978, no. 366

283 Lienden – De Fleerde
Situation a.s.s.
Finds *(a)* Drag 37, Ch 320?, Drag 31, xx, *(b)* red-brown slip ware, see ch. 6.2.2, xxx, *(c)* T 1, T 2, Al 24, *(d)* Br 36, xxx, *(e)* St 201, St 210, T 4, Nb 89 (a.o. late specimens), Al 29, xxx, *(j)* xx, *(l)* pipe-clay statuette ?, *(m)* MIA, LIA, RA, *(o)* Bö D 9–12, *(q)* spindle whorl
Collection AWNN, RMO, private
Literature Modderman 1951, no. 60; Peddemors 1978, no. 363–364

284 Leur – dorp
Situation Pleistocene sand (river dune)
Finds *(c)* xx, *(e)* xx, *(l)* Isings 3, *(m)* LIA, RA
Collection RMO, AWNN
Literature Modderman 1951, no. 71; Peddemors 1978, no. 303, 305

285 Leur – watertoren
Situation No a.s.s. recorded by soil survey. Propably on river dune
Finds *(d)* xx, *(e)* xx, *(m)* LIA, RA, *(n)* bracelet Haev ?
Collection AWNN, private
Literature Peddemors 1978, no. 302

286 Hernen – Den Dries
Finds *(m)* LIA
Collection AWNN
Literature Peddemors 1978, no. 319

287 Hernen – dorp
Finds *(o)* Bö B 1a, Bö B 1b, biconical urn cf. Faider-Feytmans 1970, Pl. 89, 57, Bö D 10/11, barrel-shaped urn cf. Van Es 1964, Pl. 89, 1, hand-made urn, *(p)* axes, spearhead(s)
Collection private
Literature Peddemors 1978, no. 318

288 Hernen – De Loffert I
Finds *(m)* MIA, LIA, *(n)* bracelet Haev 7a
Collection AWNN
Literature Peddemors 1978, no. 314–316

289 Hernen – De Loffert II
Finds *(a)* Drag 37 (see ch. 6.2.1), Drag 18/31, Drag 31, Drag 33, Drag 38?, Drag 45, Ch ?, *(b)* St 6?, St 10, Br 5, Br 25A, xxx, *(c)* HBW 81, Br 19, T 1, xx, *(d)* Hofh 78, St 138, Br 37, xxx, *(e)* St 201, St 210, St 216, St 218, St 219, T 4, Nb 89, Nb 104, Nb 105, Al ?, xxx, *(j)* xxx, *(l)* lead fragment, iron key and fragments, *(m)* MIA, LIA, RA, *(n)* bracelet Haev 7a, *(q)* slag
Collection AWNN, private
Literature Peddemors 1978, nos. 311–313

290 Hernen – De Loffert III
Finds *(a)* Drag 31, *(b)* xx, *(c)* HBW 46, T 1, *(d)* Br 37, xxx, *(e)* St 201, xx, *(j)* xxx, *(l)* iron ring and fragments, *(m)* LIA, RA, *(n)* bracelet Haev 3a
Collection AWNN
Literature JAWNN 1974, 18–20; Peddemors 1978, nos. 109, 110

291 Hernen – De Loffert IV
Finds *(a)* Ch ?, xx, *(b)* St 2, xxx, *(c)* HBW 52, T1, T 2, *(d)* Hofh 78, Br 36, Br 37, xxx, *(e)* St 201, St 210, T 4, Gose 483?, Br 21, Nb 104, Al 27, T 3, xxx, *(j)* xxx, *(k)* tuff, *(l)* iron fragments, *(m)* LIA, RA, *(o)* Bö B?, Bö D 9–12, *(q)* slag
Collection AWNN
Literature JAWNN 1975, 26–27; Peddemors 1978, no. 307

292 Leur – De Galgenberg
Finds *(a)* Déch 67 (??), stamp OF. GABA. AP+ (OF. G SA͡L. APT͡I), Drag 18/31, stamp MOXIVSF, Drag 31, stamps SABINVS and SA(binus), xx, *(b)* St 1, St 2, *(c)* HBW 13, HBW 14, HBW 28, HBW 29A, HBW 81i, Ch 342, *(d)* xx, *(e)* St 213A, xx, *(g)* follis Constantinus II, follis Constantinus, *(h)* IIXII on HBW 81i, *(l)* bronze wire fibula, tutulus fibula, small bronze and iron fragments, *(m)* RA
Collection private
Literature JAWNN 1972, 8; Haalebos a.o. 1976, 82; Peddemors 1978, no. 301

293 Wijchen – Wezelsche berg[23]
Finds *(l)* iron spearhead
Collection private

294 Wezel – Het Goor I
Finds *(c)* xx, *(e)* xx, *(m)* LIA/RA *(n)* bracelets Haev ?
Collection RMO, private
Literature Modderman 1951, no. 93; Peddemors 1978, no. 277

295 Wezel – Het Goor II
Finds *(a)* Drag 29 (see Ch. 6.2.1), Drag 37, xx, *(b)* St 2, Br 5, xxx, *(d)* St 129A, St 138, Br 36, xxx, *(e)* St 201, St 210, St 219, Nb 89, Nb 104, xxx, *(m)* LIA/RA
Collection AWNN
Literature Peddemors 1978, no. 276

296 Wijchen – Achterlo I[23]
Finds *(b)* xx, *(c)* HBW 81, xx, *(d)* Hofh 78, St 138, *(i)* FORT (*is* or *unatus*) on St 138 (before firing), *(m)* LIA, RA *(n)* bracelets Haev 3a, 3b, 7a, 7b, 7d, glass beads, bronze buckle as in *Germania* 30, 1952, 335, 'Nauheimer' fibula, *(q)* slag
Collection AWNN, private
Literature JAWNN 1972, 7; Peddemors 1978, no. 289

297 Wijchen – Achterlo II[23]
Finds *(n)* bracelet Haev 7a
Collection private
Literature Peddemors 1978, no. 290

298 Wijchen – De Pas, Passerot I[23]
Finds *(a)* Drag 37 (see ch. 6.2.1), Drag 32, Drag 33, Drag 44, Drag 45, xxx, *(b)* St 2, St 10, Br 3A, Br 5, Nb 33C?, xxx, *(c)* HBW 55C, T 1, xxx, see also sub *f*, *(d)* St 107, St 109, St 110B, St 129A, St 129B, St 138, Hofh 78, Br 36, Br 37, xxx, *(e)* St 201, St 210, St 218, St 219, T 4, Nb 89 (a.o. early specimens), Nb 104, Nb 111A, Nb 112A, Gose 505, Al 27, xxx, *(f)* rimsherd of terra rubra like pottery, form Hofh 5, *(h)* VEX·B[RIT] and [LEG XX] X S(everianae) A(lexandrianae) on tegulae, *(i)* X on Br 36, XII on coarse ware, *(j)* xxx, *(k)* tuff, *(l)* iron key and fragments, (window-)glass, bronze buckle, late-Roman hairpin, *(m)* RA
Collection MFB, AWNN, private
Literature JAWNN 1971, 11–12; Janssen/Tuyn 1972; Bogaers 1972; Peddemors 1978, nos. 293, 294

299 Wijchen – De Pas, Passerot II[23]
Finds *(a)* Drag 37, Drag 18/31, Drag 31, Drag 27, Drag 33, Drag 45, xxx, *(b)* St 2, Br 4, Nb 38, xxx, *(c)* Ha 91A, T 1, xx, *(d)* Hofh 78, St 138, Br 36, xxx, *(e)* St 201, St 210, St 219, Nb 89, Nb 104, xxx, *(f)* 'fine Nijmegen' pottery, *(g)* 5 denarii of Nerva, Hadrianus, Faustina, and Antoninus Pius, 3 asses of Nero, Antoninus Pius?, and Augustus?, dupondius Traianus, *(j)* xx, *(l)* bronze wire fibula Van Buchem 1941, 24A and enamelled fibula Van Buchem 27C, *(m)* LIA, RA, *(n)* bracelets Haev 3b, glass beads MIA and LIA, *(q)* (pottery-?) slags (and parts of kiln?), loomweights
Collection AWNN, private
Literature JAWNN 1973, 15; Peddemors 1975, nos. 140–142; Tuyn 1978, 205–207; Peddemors 1978, no. 292

300 Wijchen – De Pas, Passerot III[23]
Finds *(c)* xx, *(d)* Hofh 78, xxx, *(e)* St 201, St 210, Br 21, Nb 89 (a.o. late example), Nb 104, Nb 112, Al 27, Al 28, xxx, *(j)* xx, *(l)* spoon-bow fibula, bronze wire fibulae, iron late-Roman razor, iron ring and fragments, *(m)* LIA, RA, *(n)* bracelets Haev 3a, 3b, 7a, glass bead, *(o)* Bö D 9–12, *(q)* spindle-whorls, loomweights, slags
Collection AWNN, private
Literature JAWNN 1973, 14; Peddemors 1975, nos. 127–139; Peddemors 1978, no. 288

301 Wijchen – De Pas, Eekhoornpad[23]
Finds *(d)* xx, *(e)* xx, *(l)* fibula Van Buchem 1941, 22C ?, *(m)* LIA, RA, waster from kiln, *(n)* bracelets Haev 3a, 3b, *(q)* loomweights
Collection AWNN, private
Literature Peddemors 1978, nos. 283–286

302 Wijchen – De Pas, Patrijsstraat[23]
Excavation IPL 1971
Finds (m) MIA, LIA, *(n)* bracelets Haev 7a, 7c?
Collection AWNN, IPL, GAS, private
Literature Peddemors 1978, nos. 295–296
Other data C14 dates of 3110 ± 40 BP (GrN 5481), 3480 ± 40 BP (GrN 5482) and 2350 ± 30 BP (GrN 5880). See Lanting/Mook 1977, 148

23 The settlement sites 296, 298, 299, 300, 301, and 302, together with the isolated finds 293, 297, and 303, which are very close together and except for the last three sites already combinations of findspots (see also Peddemors 1978), might constitute one large settlement. The nucleus of this settlement could have shifted somewhat through time, because there are clear differences in the dating of the various sites. Alternative explanations would be the growing together of two or more settlements or the filling-in of space between isolated farmsteads. The entire conglomerate of sites probably had a continuous occupation from the Early-Bronze Age (Janssen/Tuyn 1972) to the Early-Middle Ages (with a possible hiatus in the Late-Bronze Age).

303 Wijchen – Merelstraat[23]
Finds *(g)* dupondius Domitianus
Collection private
Literature AN 1969, 39; Peddemors 1978, no. 297

304 Wijchen – Woezik
Finds *(n)* bracelet Haev 3b
Collelction private
Literature Peddemors 1978, no. 271

305 Wijchen – Holenbergseweg
Excavation IOGA 1973
Collection IOGA, AWNN, MFB, private
Literature Haalebos/Willems/Giebels 1976; Peddemors 1978, nos. 266, 268, 270

306 Wijchen – Kruisberg I
Finds *(a)* Drag 15/17, Drag 27, Drag 31, stamps]VSF and CILLVTIVS·F (see ch. 6.2.1), Drag 33, *(b)* St 1, St 2, St 10, Br 5A, *(c)* HBW 8B, 19B?, 27, 28, 81F, Br 16C, *(d)* St 106?, St 107, St 110A, St 111, St 115, *(e)* St 201, St 201C, Hofh 85A?, Br 17, *(f)* HNW Pl. III, 182, *(h)* IIXIV on HBW 81F, *(m)* RA
Collection RMO, MFB
Literature Modderman 1951, no. 77; Peddemors 1978, no. 259

307 Wijchen – Kruisberg I
Finds *(a)* Drag 31, *(b)* xx, *(d)* xx, *(e)* St 214, xx, *(m)* LIA, RA, *(n)* bracelets Haev 7a, 7b, glass bead
Collection AWNN
Literature Modderman 1951, no. 77; Peddemors 1978, no. 265

308 Wijchen – Kraaijenberg
Finds *(p)* fibula (bird-form)
Collection private
Other data Both Peddemors (1978, no. 298) and Haalebos/Willems/Giebels (1976, *afb.* 1, 15) erroneously ascribe Roman material to this site

309 Wijchen – Homberg
Finds *(b)* xx, *(d)* St 129?, xx, *(e)* Nb 104, *(m)* MIA?, LIA/RA
Collection AWNN
Literature Peddemors 1978, no. 255

310 Wijchen – Het Vormer
Finds *(a)* Drag 45, xx, *(b)* xx, *(c)* Girth-beakers, T 2, *(d)* xxx, *(e)* Nb 89, xxx, *(l)* spoon-bow fibula, bronze wire fibula, *(m)* LIA, RA
Collection RMO, MFB, AWNN, private
Literature Peddemors 1978, no. 230

311 Wijchen – Klispoel
Finds *(m)* LIA
Collection private
Literature Peddemors 1978, no. 228

312 Wijchen – Hoog
Finds *(a)* xx, *(b)* xx, *(c)* T 1, *(d)* Hofh 78, xxx, *(e)* St 201, Nb 89, Nb 111, Al 27, *(m)* LIA, RA, *(q)* string of amber and glass beads, (pottery-?) slag, clay tubes (smelting activities ?)
Collection AWNN, private
Literature JAWNN 1972, 5; Peddemors 1978, no. 254

313 Wijchen – Aalsburg
Finds *(g)* denarius Commodus
Collection private

314 Wijchen – Leursche Bosch
Finds *(m)* EIA, LIA
Collection MFB
Literature Peddemors 1978, no. 281

315 Wijchen – Tienakker I
Situation The material from this large settlement comes from several closely associated findspots. The material from the 'Wijchens Meer' (site no. 316) was dumped and/or lost there by the inhabitants of this settlement.
Finds *(a)* Drag 37 (see ch. 6.2.1), Ch 320 (see ch. 6.2.1), Drag 27, Drag 18/31, Drag 31, Drag 32, Drag 33, Drag 38, Drag 45, *(b)* St 1, St 2, St 10, Nb 30, Nb 32, Nb 38, Br 16; red-brown slip ware Hussong/Cüppers 1972, *Taf.* 16, 23 and 28a, *Taf.* 20, 16, 19 and 24 (see ch. 6.2.2, p. 159 for a description), *(c)* HBW 52, T1, T2, HBW 55, Br 16C, Ch 342, Al 24?, *(d)* St 107, St 109, St 110B, St 111, St 129A, St 129B, St 132, St 138, St 143?, St 146, St 147, Hofh 78, Br 36, Br 37, Al 31, Pirling 90–92, xxx, *(e)* St 201, St 202, St 210, St 218, St 219, T 4, Gose 496, Nb 89 (a.o. late specimens), Nb 104, Nb 116, Nb 120B, Al 27, Al 28, Al 29, Al 30, Al 34, T 3, xxx, *(f)* lamp, *(g)* follis Constantinus I, *(h)* [ME]RCA͡TOR on handle of two-handled jug, VEX BRI͡T, VEX EX GER INF and [EX]GE[R INF] on tiles, *(i)* // or N on Drag 27, X on coarse ware, *(j)* xxx, *(k)* tuff, limestone, tubuli, hypocaust tiles, fragments of wall painting, *(l)* bronze wire fibula, bronze mirror, fragments of bronze objects, whetstone, small bronze enamelled lid, lead, iron objects and fragments, *(m)* LIA??, RA, early-Medieval, *(o)* Bö B?, Bö C?, Bö D 9–12, Bö 16C/17B, Stampfuss 1939, Abb 23, 1 and 23, 2 *(q)* spindle-whorls, bronze faucet
Collection AWNN, private

Literature Peddemors 1978, nos. 250–252; JAWNN 1982, 20–1

316 Wijchen – Wijchense Meer
Situation see sub 315
Finds *(a)* Drag 29, Drag 37 (see ch. 6.2.1), Drag 15/17, Drag 27, stamp VITA (see ch. 6.2.1), Drag 18/31, stamp ANISATVSF, (see ch. 6.2.1), Drag 33, Drag 40, Drag 45, *(b)* xx, *(c)* T 1, *(d)* St 109, St 110B, St 138, Hofh 78, Br 36, Br 37, xxx, *(e)* St 201, St 210, St 218, St 219, Nb 89, Nb 104, Al 27, Al 28, Al 34, xxx, *(g)* 5 quadrantes Domitianus-Antoninus Pius, *(h)* ADIVTORF on Br 36, VEX EX[on tegula, PHL? or PNL? on amphora, *(j)* xxx, *(k)* tuff, limestone, tubuli, hypocaust tiles, sandstone, *(l)* iron shears, hook with socket, spearhead and handle, bronze bell, dishes Den Boesterd 1956 c.type 83 and 84, stylus(?) fragment, terminal mounting of sheath, rings a.o. finger-ring and small fragments, golden cross-bow-fibula, net-sinkers (secondarily worked pieces of tuff and tegulae), pieces of worked wood, *(m)* EIA, MIA, LIA, RA, *(p)* bronze pin of belt-buckle, iron axe Bö Taf 33, 5, *(q)* small lead rolls and large lead rolls with pieces of wood (probably net-sinkers), parts of kiln?
Collection AWNN, private
Literature AN 1979, 89

317 Wijchen – Tienakker II
Finds *(n)* bracelet Haev 7a
Collection private
Literature Peddemors 1978, no. 249

318 Wijchen – dorp
Finds *(d)* x, *(e)* x, *(j)* xxx, *(m)* LIA/RA
Collection RMO, MFB?, private?
Literature ER III, 104, al. 5 and 105, al. 3; Modderman 1951, no. 83; Peddemors 1978, nos. 246–247

319 Wijchen – Molenberg/raadhuis
Finds *(a)* Drag 15/17, stamp IVLLI, Drag 18, stamp COM[and stamp PERECRINV(s) (see ch. 6.2.1), Drag 31 stamp MAGNVS.F, Drag 33, stamp OF-MERCA, *(b)* St 2, St 10, St 12, Br 4A1, *(c)* HBW 28, HBW 29A, HBW 31B, HBW 52A, *(d)* xx, *(e)* St 201, St 218, xxx, *(h)* VEX EX GER on tile (no association with cemetery), *(i)* AMAE ? on Drag 18, *(l)* bronze wire fibula, bronze spatula, iron finger ring with gem engraved with figure of Mars
Collection RMO, MFB, AWNN, IOGA
Literature Modderman 1951, no. 82; Peddemors 1978, nos. 244–245

320 Wijchen – Molenberg
Finds *(m)* LIA
Collection AWNN
Literature JAWNN 1975, 20; Peddemors 1978, no. 243

321 Wijchen – Esdoornstraat
Finds *(a)* Al 12, *(b)* Al 17?, *(e)* Al 30, *(l)* iron knife?
Collection MFB
Literature Peddemors 1978, no. 241

322 Woord – De Zandakkers
Finds *(m)* LIA, *(n)* bracelets Haev ?, glass bead
Collection AWNN, private
Literature Peddemors 1978, nos. 225, 227

323 Woord – Woordse straat
Finds *(e)* St 218, xx, *(m)* LIA/RA
Collection RMO
Literature Modderman 1951, no. 75; Peddemors 1978, no. 224

324 Wijchen – Valendries A-C
Situation Judging from the material there are only a few Roman graves in a large prehistoric urnfield (the late Mr. F.J.G.H. Bloemen's terrains A, B, and C)
Finds *(a)* xx, *(c)* HBW 55C, *(e)* St 214A, xx, *(l)* bronze fibula, *(m)* EIA, LIA, RA
Collection MFB, private
Literature Modderman 1951, no. 85; Peddemors 1978, nos. 237–240

325 Wijchen – kerk
Finds *(g)* denarius Marcus Aurelius, aes-IV Valentinianus II
Collection MFB

326 Wijchen – Palkerdijk
Finds *(a)* Drag 37, Drag 27, *(b)* xx, *(c)* xx, *(d)* St 111, xx, *(e)* Nb 89, xxx, *(h)* VEX EX[on tegula, *(j)* xx, *(m)* EIA, MIA, LIA, RA, *(q)* loomweight
Collection AWNN, GAS
Literature Peddemors 1978, nos. 211–212

327 Wijchen – Mussenberg
Finds *(e)* St 201
Collection private
Literature Haalebos/Willems/Giebels 1976, *afb.* 1, no. 6

328 Wijchen – Woezikseweg
Finds *(a)* xx, *(e)* x, *(m)* LIA, RA
Collection AWNN, private
Literature Peddemors 1978, no. 210

329 Beuningen – Bijsterhuizensestraat
Finds *(c)* xx, *(d)* St 107, St 129A, St 147, Br 36, xxx, *(e)* St 201, St 210, St 218, Nb 89, xxx, *(m)* LIA, RA
Collection RMO, GAS, AWNN
Literature Modderman 1951, no. 4; Peddemors 1978, no. 143

330 Neerbosch – Lindenholt
Finds *(m)* LIA, RA?, *(q)* whetstone
Collection AWNN
Literature Peddemors 1978, no. 142

331 Wijchen – Teersdijk I
Situation Site nos. 331 and 332 together are possibly one elongated cemetery
Finds *(a)* Drag 18, stamp O[F VI]TAL (see ch. 6.2.1), Drag 27, stamp IVCV͡NDI (see ch. 6.2.1), Drag 31, *(b)* St 2, *(c)* HBW 28, *(d)* St 108, St 109, St 110A, St 131, xx, *(e)* St 210, *(l)* iron fragment, *(m)* RA
Collection RMK, RMO, MFB
Literature Modderman 1951, no. 80; Peddemors 1978, no. 213

332 Wijchen – Teersdijk II
Situation see sub 331
Finds *(a)* Drag 18/31, stamp -CAL[AV]A.F, Drag 27, *(d)* St 109, xx, *(e)* xx, *(i)* M on Drag 18/31, *(l)* bronze wire fibula?, iron shears, iron pins
Collection RMK, private?
Literature Peddemors 1978, no. 214

333 Wijchen – De Berendonk
Excavation IPL 1976
Finds *(m)* LIA, *(n)* bracelets Haev 3a, 3b, 6b, loom-weights, spindle-whorls, whetstones
Collection AWNN, IPL, GAS, RMO, private
Literature Modderman 1951, no. 94; JAWNN 1974, 14; Peddemors 1975, nos. 103–118; JAWNN 1976, 11–14; AN 1976, 256; Peddemors 1978, nos. 201–208; JAWNN 1982, 15–9

334 Wijchen – Oosterweg
Finds *(a)* Drag 45, xx, *(d)* xx, *(e)* xx, *(j)* xx, *(m)* LIA/RA
Collection GAS
Literature Peddemors 1978, no. 233

335 Wijchen – Het Slotje
Finds *(m)* LIA, *(n)* glass bead
Collection AWNN, private
Literature Modderman 1951, no. 87; Peddemors 1978, nos. 231–232

336 Alverna – Heumenseweg I
Situation The site is known as 'terrain F' (collection F.J.G.H. Bloemen)
Finds See Remouchamps 1928; for stamps on terra sigillata see ch. 6.2.1
Collection MFB, RMO
Literature Remouchamps 1928; JRMO 1930, 6; Modderman 1951, no. 85; Haalebos/Willems/Giebels 1976, 81–82 and *afb.* 1, 9; Peddemors 1978, no. 197

337 Alverna – Graafseweg I
Finds *(a)* Drag 18/31, xx, *(b)* St 1, xxx, *(c)* T 1, *(d)* Hofh 78, Br 37, xxx, *(e)* St 201, St 210, St 218?, Nb 89 (early specimens), *(j)* xxx, *(l)* fragment of bronze mounting *(m)* RA
Collection AWNN, private
Literature Haalebos/Willems/Giebels 1976, *afb.* 1, 18; Peddemors 1978, no. 196

338 Alverna – Graafseweg II
Finds *(n)* bracelet Haev 3b
Collection RMO
Literature Peddemors 1978, no. 195

339 Alverna – Vossenberg
Excavation RMO 1925
Finds *(a)* Drag 37, *(b)* St 2, xx, *(c)* T 1, *(d)* xxx, *(e)* St 210, Nb 89, Nb 104, dolium-form, xxx, *(j)* xxx, *(m)* LIA, RA, *(q)* whetstone
Collection RMO, AWNN
Literature JRMO 1925, 9–10; Modderman 1951, no. 90; Peddemors 1978, no. 177

340 Boschkant – De Waaiendonk
Situation Pleistocene sand (river dune)
Finds *(d/e)* x
Literature Modderman 1951, no. 89; Peddemors 1978, no. 176

341 Lunen – dorp
Finds *(a)* Drag 30, Drag 37, Drag 18/31, Drag 27 stamp AMABILIS (see ch. 6.2.1), Drag 36, *(b)* St 2, xxx, *(c)* Ha 91A, HBW 55, Br 5A/B, Br 16C, *(d)* St 107, St 130, St 146, St 152, Hofh 78, Br 36, xxx, *(e)* St 201, St 202, St 210, St 216, St 218, St 219, T 4, Nb 89, Nb 104, xxx, *(f)* fragment of mica-gilt ware, *(h)* tile-stamp of the *legio X gemina*, *(j)* xxx, *(k)* tuff, limestone, *(l)* iron ring and fragments, *(m)* LIA, RA, *(n)* probable waster from kiln
Collection RMO, AWNN, GAS, private
Literature Modderman 1951, no. 86; Peddemors 1978, nos. 220–222

342 Nederasselt – dorp
Finds *(a)* Drag 31, *(b)* St 10, xx, *(d)* St 132A, xx, *(e)* St 210, T 4, xx, *(m)* LIA/RA
Collection RMO, GAS
Literature Modderman 1951, no. 95; Peddemors 1978, no. 355

343 Nederasselt – Het Molenwiel
Finds *(l)* iron lamp, *(m)* LIA, RA?
Collection private
Literature Peddemors 1978, no. 354

344 Nederasselt – Molenhoek
Finds *(a)* Drag 18/31, Drag 31, stamp G[ER]MÀNI? (see ch. 6.2.1), *(c)* HBW 58C/59, *(d)* St 111
Collection RMK
Literature JRMK 1966, 311; Peddemors 1978, no. 508

345 Overasselt – Duifhuis
Finds *(m)* LIA
Collection RMO
Literature Modderman 1951, no. 97; Peddemors 1978, no. 169

346 Alverna – Heumenseweg II
Finds *(a)* Drag 24/25, stamp SECVNÐ[I] (see ch. 6.2.1), x, *(b-e)* x, *(m)* EIA, MIA
Collection RMO, MFB
Literature Peddemors 1978, no. 193

347 Alverna – Geitweg I
Finds *(l)* crossbow fibulae
Collection Nijmegen University
Literature Van Buchem 1941, 121

348 Alverna – Heumenseweg III
Finds *(c)* HBW 28, Br 16C, *(d)* St 106, St 108, St 110B, xxx, *(e)* St 210, St 213A, *(m)* RA
Collection RMO, MFB, GAS?, private
Literature Peddemors 1978, nos. 188–190

349 Alverna – Wijchense Ven I
Finds *(l)* fibula Van Buchem 1941, 23, Nauheimer fibula, bronze wire fibula, bronze handle?
Collection private
Literature same site as Peddemors 1978, no. 185

350 Alverna – Wijchense Ven II
Finds *(m)* MIA, LIA, *(n)* bracelets a.o. Haev 3b, spindle whorl, slag, wattle and daub
Collection RMK, RMO, GAS
Literature Bloemen 1933; Bursch 1935; Peddemors 1978, no. 183, 184

351 Alverna – Heumenseweg IV
Excavation RMO 1933–34, ROB 1951
Collection MFB
Literature Bursch 1935; Modderman 1953b; Peddemors 1978, no. 187

352 Alverna – Heumenseweg V
Finds *(m)* LIA, *(n)* bracelet Haev ?
Collection GAS, private
Literature Peddemors 1978, no. 181

353 Overasselt – De Bullenkamp I
Finds *(a)* Drag 18/31, stamps BVCCI[VS·F], DIVICI·M·, MECCO FEC, MARC[EL]LIVS and LVCIVSF, Drag 31 stamp VRBANVSF, Drag 33 stamp VÀRE-CVNDV· (for stamps see ch. 6.2.1), *(b)* St 2, St 10, St 12, *(c)* c. HBW 20, HBW 52, *(d)* St 111, St 112, xx, *(e)* St 218, Nb 89 (a.o. early specimens), Nb 104, *(g)* sestertius Vespasianus, sestertius Marcus Aurelius, *(l)* bronze inkwell, bronze spoons, bronze 'dagger' fibulae, *(m)* RA
Collection RMK, private
Literature JRMK 1941, 125 and 1942, 126; Peddemors 1978, no. 174

354 Overasselt – Heumenseweg VI
Finds *(b)* St 2 *(e)* Hofh 85C
Collection private

355 Overasselt – Scheiwal
Excavation RMO 1921 (22?) and 1930
Collection RMO, AWNN
Literature Braat 1934; Modderman 1951, no. 92; Peddemors 1978, no. 168

356 Overasselt – De Bullenkamp II
Situation The material from this site contains many 'unusual' finds. There is, however, no reason to assume it was a cemetery like site no. 353. It may be that we are dealing with an 'unusual' settlement, but more likely a great number of the normal ceramics has not been saved (or objects from elsewhere have been added).
Excavation RMO 1943
Finds *(a)* Drag 37, stamp [C]ERIALISF (see ch. 6.2.1), Déch 67, Drag 33, Drag 38, *(b)* St 10, Br 3, xxx, *(c)* Ha 91A, *(d)* St 129A, St 138, xxx, *(e)* St 205, St 218, St 219, *(f)* lamp, head of pipe-clay statuette, *(g)* bronze coins Domitianus, Hadrianus, Antoninus Pius, *(h)* FORTIS on lamp, *(l)* bronze fibulae, a.o. bronze wire fibula and enamelled fibula, bronze mounting, bronze finger-ring with glass bead, blue melon bead, gem engraved with

cupid riding dolphin, bronze parts of harness?, iron shovel, iron object, bronze lock, *(m)* LIA, RA (a.o. unusual forms), *(n)* bracelets Haev 3a, 3b, 6c, glass beads *(o)* Bö C? *(q)* wattle and daub
Collection RMO, private
Literature JRMK 1942, 127 and 1943, 90; Modderman 1951, no. 91; Peddemors 1975, nos. 99–101; Peddemors 1978, no. 173

357 Overasselt – De Schatkuil
Finds *(a)* Drag 18, Drag 27, stamp OF CRES, Drag 31, stamp CELSINVS F (see ch. 6.2.1), *(b)* St 2, *(d)* St 110, *(e)* St 218, Nb 104, Nb 105, *(l)* spearhead
Collection private

358 Overasselt – Valenberg
Finds *(a-e)* x, *(j)* xxx, *(k)* limestone, quartzitic sandstone, *(m)* RA, *(n)* bracelets Haev 3a, 3b
Collection private
Literature ER III, 104, al. 3 (?); Peddemors 1975, nos. 97–98; Peddemors 1978, no. 167

359 Overasselt – St. Walrick
Finds *(n)* bracelet Haev 7a
Collection private
Literature Peddemors 1978, no. 165

360 Overasselt – De Broekberg
Finds *(a)* Drag 18/31, *(m)* LIA, RA, *(n)* bracelets Haev 3b, iron knives
Collection RMK, AWNN
Literature Peddemors 1978, no. 158

361 Overasselt – Blankenberg
Finds *(m)* LIA
Collection GAS
Literature Peddemors 1978, no. 157

362 Overasselt – Worsemse Blok
Finds *(b)* St 2, xx, *(c)* girth beaker, HBW 52, T 2 *(d)* xxx, *(e)* St 201, xxx, *(j)* xxx, *(m)* EIA, RA
Collection GAS, AWNN
Literature Peddemors 1978, no. 155

363 Overasselt – Worsem
Finds *(c)* HBW 28, T 2, *(d)* Hofh 78, xx, *(e)* St 201, xxx, *(m)* RA
Collection AWNN

364 Heumen – Vosseneind I
Finds *(e)* xx, *(m)* LIA/RA
Collection RMO, GAS
Literature Modderman 1951, no. 102; Peddemors 1978, no. 154

365 Heumen – Vosseneind II
Finds *(b)* Br 5A, *(e)* xx, *(m)* LIA ?, RA ?
Collection AWNN, (GAS?)
Literature Peddemors 1978, no. 153

366 Heumen – Dorpsstraat
Finds *(o)* Bö B 1b
Collection RMK
Literature JRMK 1958, 242

367 Heumen – De Hosterd
Finds *(a)* xx, *(d)* xx, *(e)* Nb 104, xxx, *(m)* LIA, RA
Collection RMO, AWNN
Literature Modderman 1951, no. 101; Peddemors 1978, no. 150

368 Gennep – kerk
Finds *(a)* Drag 18/31, stamp OF FL·GER, Drag 27, Drag 31, stamp MIC(cio), Drag 33, Drag 35, Curle 21, *(b)* St 2, St 10?, Br 20, xx, *(c)* HBW 52A, *(d)* St 105, St 106A, St 106B, St 110B, St 130, Br 2C, Br 10, Br 20, *(e)* St 201C, St 210, St 218, Nb 97, *(i)* N.NOVVM on Drag 18/31, A M on Drag 18/31, X on Drag 31, IN (?) on Drag 31, *(m)* RA
Collection private
Literature Bogaers/Morren 1954

369 Gennep – Maaskemp
Finds *(a)* Ch 320 (see ch. 6.2.1)
Collection Bonnefantenmuseum Maastricht
Literature Bloemers 1977, 22

370 Gennep – Niers
Finds *(g)* follis Constantius II/Constans and Magnentius
Collection Goltziusmuseum Venlo
Literature AN 1962, 196–197

371 Milsbeek – Bloemenstraat
Finds *(d)* Hofh 78, *(e)* St 210, xx, *(l)* blue glass gaming-counter, *(m)* LIA/RA
Collection private

372 Middelaar – Heikant
Finds *(a)* Drag 27, Drag 31 stamp LOSSA (retro) and stamp]VSF, *(b-e)* x
Collection Museum Venray

373 Middelaar – Plasmolen I
Situation Apart from the finds mentioned below, some finds in the RMK (see JRMK 1940, 177 and 1941, 125) may also originate from this cemetery.
Finds *(a)* x, stamp L COSI VI, *(c)* HBW 28, *(e)* St 201, *(l)* enamelled 'trumpet' fibula, *(m)* LIA??, RA

Collection Bonnefantenmuseum Maastricht, (RMK)
Literature JRMO 1939, 91; Bursch 1942, 60

374 Middelaar – Plasmolen II
Finds *(g)* sestertius Domitianus
Collection private

375 Milsbeek – Pastoorsdijk
Finds *(d)* St 129B, Hofh 78, xx, *(e)* St 210, xxx, *(m)* LIA, RA
Collection private

376 Milsbeek – De Hel
Finds *(c)* xx, *(d)* Br 36, *(e)* St 201, xx, *(m)* EIA, MIA, LIA, RA, *(n)* glass bead, *(o)* Bö D 9–12
Collection private
Literature JAWNN 1976, 22–24; Verscharen 1978

377 Mook – Kloosterberg I
Excavation RMO 1931–33
Finds *(h)* stamps [L]EG X[and LXG on tegulae
Collection RMO
Literature Braat 1934

378 Mook – Kloosterberg II
Finds Possible Roman tumulus
Collection In situ

379 Katerbosch – Eindweg
Finds *(a)* Drag 37, Drag 33, xx, *(b)* xxx, *(c)* T 2, *(d)* St 110A, Hofh 78, Br 37, xxx, *(e)* St 201, St 210, St 216, St 218, St 219, Nb 89, Nb 104, xxx, *(j)* xx, *(l)* glass bottle, *(m)* LIA/RA, *(o)* Bö B?, Bö D 9–12
Collection private
Literature JAWNN 1979, 7–10

380 Katerbosch – Veerstraat
Finds *(d)* Hofh 78, xx, *(e)* St 201, xx
Collection private

381 Katerbosch – Elzenstraat
Situation The site is indicated as a probable settlement on Appendix 3. Additional finds after the map had been printed have shown it to be definitely a settlement also during the Middle-Roman Period.
Finds *(a)* Drag 27, *(b)* xx, *(d)* Hofh 78, xxx, *(e)* St 201, St 210, Nb 89, xxx, *(j)* xxx, *(m)* RA, *(o)* Bö B?, Bö D 9–12
Collection private

382 Katerbosch – Middelaarse Broek
Finds *(a)* Drag 18/31 stamp NICEPHOR, Drag 33, Drag 40, *(l)* bronze object
Collection Museum Venray

383 Mook – Molenbroek
Situation Several burials which were discovered in the immediate surroundings of this cemetery (e.g. those of the 'Plankraai' and 'Het Heibergske') are also included in the description of the finds. Together they undoubdedly constitute one fairly large cemetery, probably alongside a road. Even sites nos. 382 and 384 are probably part of this cemetery. See also under site 539.
Finds *(a)* Drag 18/31, Drag 27, Drag 31 stamp BONOX-I·M·, Drag 33 stamp [V]ERECVNDV, xxx, stamps CINTVCNATVS and DIIC[, *(b)* St 2, St 11, Br 4A, Br 6B, Br 8, Br 11, *(c)* HBW 26A, HBW 28, HBW 81F, *(d)* St 110B, St 111, St 129B, xxx, *(e)* St 114B, St 218, Nb 89, Nb 104, xxx, *(k)* tubulus, *(l)* glass balsamarium, bronze jug c. Den Boesterd no. 260, enamelled bronze perfume flask Den Boesterd no. 307, bronze crater, bronze lamp with oak-leaf, bronze jug-handle Den Boesterd no. 279, sarcophagus of sandstone, stone lid of beauty-box
Collection gemeente Mook, Noordbrabants Museum, RMK
Literature Hermans 1865, 14; Habets 1883; 1895, 281; ER III, 57, al. 4 and 58, al. 3 and 4; Bogaers/Morren 1954; Willems 1983b, 266–7

384 Mook – Startsche Veld
Finds *(a)* Drag 31, Drag 35, *(b)* Br 2A, Br 2C, Br 3B, *(c)* c.HBW 52, *(d)* St 110A, St 110B, Br 5C, Br 37, xx, *(e)* St 210, St 216, St 218, Nb 104, *(f)* lamp Evelein form A3, *(l)* bronze flask
Collection Municipality of Mook

385 Mook – Spoorbrug
Finds *(a)* Drag 37 (see ch. 6.2.1), Drag 18 stamps OF PRIMI and OF VITAI (see ch. 6.2.1), Drag 18/31, Drag 27, Drag 33 stamp BOVDVS, Drag 35 rosette stamp (see ch. 6.2.1), Drag 36, *(b)* St 1, St 2, Br 5A, xx, *(c)* Ha 91A, HBW 25, HBW 52, HBW 81E, HBW 90, Br 19, Br 16C, *(d)* Hofh 51, St 106, St 109, St 110B, St 111, St 130?, St 147, St 151B, St 155, xxx, *(e)* St 201, St 204A, St 210, St 213A, *(f)* lamps Evelein A 1/2 and ?, Br 2 (mica-gilt surface), 'fine-Nijmegen' pottery, head of pipe-clay statuette, *(g)* 5 bronze coins, *(h)* STROBILI on lamp Evelein A 1/2, *(l)* bronze bells, glass jar (Isings 67C?), blue melon beads
Collection RMO, RMK

386 Molenhoek – Stationsstraat
Finds *(j)* xxx
Collection RMK
Literature AN 1960, 218

387 Mook – Station
Finds *(a)* Drag 33, stamps TOCCA F and rosette (see ch. 6.2.1), *(b)* St 2, *(d)* Br 37
Collection RMK
Literature JRMK 1941, 125

388 Mook – Mokerheide
Situation Location reconstructed
Finds *(g)* sestertius Vespasian, follis Constantinus I
Collection RMK

389 Groesbeek – Het Vilje
Finds *(a)* Drag 37, Drag 18/31 stamp VIDVCVSFE (see ch. 6.2.1), Drag 31 stamps PRID·FEC (identical) (see ch. 6.2.1), Drag 32, xx, *(b)* St 1, St 2, St 11, Hofh 100, Br 3, Nb 32C, xx, *(c)* HBW 27A?, HBW 77?, xx, *(d)* St 108, St 109, St 110A, St 110 B, St 113, St 129B, St 145?, St 146, Br 20, Br 36, xxx, *(e)* St 201, St 210, Nb 89, Nb 104, xxx, *(f)* lamp(s?), 'fine Nijmegen' pottery HNW 15, HNW 27B, HNW 36B, *(m)* RA
Collection RMK, RMO, Provinciaal Utrechts Genootschap van Kunsten en Wetenschappen
Literature Hermans 1865, 15–16; JRMO 1933, 6; AN 1963, 2–3

390 Malden – Windvleugel
Finds *(o)* Bö B 1b
Collection private (RMK?)

391 Malden – Heumensoord
Excavation RMO 1931–32, IOGA 1972
Collection RMO, RMK, IOGA, private
Literature Bogaers/Rüger 1974, 81–83

392 Malden – Hooge Langeberg
Situation Location reconstructed
Finds *(g)* aes-III Valentinianus I
Collection private
Literature AN 1970, 83

393 Nijmegen – Hatert I
Finds *(a)* Drag 37, Drag 31 stamp [ME]LAVSVS F, *(b)* Br 8A, *(c)* HBW 81H, *(d)* St 111, xx, *(e)* St 201, *(g)* as Titus (under Vespasian)
Collection RMK
Literature AN 1963, 32–33

394 Nijmegen – Hatert II
Finds *(d)* xx, *(j)* xx, *(o)* Bö D 9–12
Collection RMK (?)
Literature AN 1963, 33

395 Nijmegen – Hulzen I
Excavation IOGA 1979 (finds not included in description; see fig. 50)
Finds *(d)* St 147, xxx, *(e)* St 210, Nb 89, Nb 104, xx, *(j)* xxx, *(l)* bronze wire fibula, whetstone, wooden wells
Collection AWNN, IOGA
Literature JAWNN 1975, 8–12; Bogaers/Haalebos 1980a

396 Nijmegen – Hulzen II
Excavation IOGA 1979
Collection IOGA
Literature Bogaers/Haalebos 1980a

397 Nijmegen – Oude Graafseweg
Finds *(a)* Drag 18/31 stamp AFRI M, *(b)* xx, *(d)* St 110A, *(e)* xx, *(l)* tuff sarcophagus, glass
Collection RMK
Literature JROB 1950, 32; AN 1966, 101

398 Nijmegen – Hees
Excavation Allard Pierson Stichting 1934, IOGA 1977 and 1981–3
Collection RMK, RMO, Allard Pierson Museum
Literature Brunsting 1937; AN 1978, 247–9

399 Nijmegen – Waterkwartier
Excavation RMO 1834, Gem. Nijmegen 1920–21, IOGA 1975
Collection RMK, RMO, IOGA
Literature Bogaers 1979b
Other Data This site can be identified as the Roman municipium *(Ulpia) Noviomagus (Batavorum)*

400 Nijmegen – Kronenburgerpark
Situation Elongated cemetery, probably alongside the Roman road from site no. 399 to the east. The cemetery extends from around the Kronenburgerpark to at least the Grote Markt region.
Collection RMK
Literature Holwerda 1941 (including catalogue nos. 19 and 86); Daniëls 1955, 43, 257–265, 315, 318–319, 325; Den Boesterd 1959; Isings 1964; Bogaers 1979b, 61

401 Nijmegen – Korte Brouwersstraat
Finds *(o)* Bö D 9–12
Collection private
Literature Leupen/Thijssen 1980, 689; JAWNN 1981, 36–7; JAWNN 1982, 9

402 Nijmegen – Priemstraat
Finds *(m)* LIA
Collection private

403 Nijmegen – Valkhof I
Situation This large site (see *Noviomagus* 1979, *Abb.* 5, no. 3, *Abb.* 32, no. 3 and *Abb.* 71, no. 1) can be divided into three spatially overlapping but chronologically separated settlements: the early-Roman settlement on the Valkhof, Waalkade, Kelfkensbos and Hunerpark (probably *Batavodurum* = *Oppidum Batavorum*), a middle-Roman (commercial?) centre on the Waalkade and a late-Roman settlement on and around the Valkhof, which includes a military fortification on the Valkhof (from c.260–c.350 ?). A Merovingian settlement on this site is possible (cf. Thijssen 1980, 13) but as yet definite proof is lacking
Excavation Gem. Nijmegen 1910–11, RMO 1946, RMO/ROB 1954–55, ROB 1973–75, 1979–80
Collection RMK, ROB
Literature Bogaers/Rüger 1974, 76–79; Bogaers 1979a; Bogaers 1979b, 59; Wynia 1979, 64–5; AN 1980, 36; Thijssen 1980; JROB 1982, 34–35; Bloemers 1983, 192

404 Nijmegen – Valkhof II
Excavation Gem. Nijmegen 1910–11
Collection RMK
Literature Weve 1910; Thijssen 1980; Leupen/Thijssen 1980; Brunsting 1983
Other data The location of this site on Appendix 5 is not correct! It is identical with that of site 403.

405 Nijmegen – Mariënburg
Excavation RMO/ROB 1947, 1949–53, 1955–57, 1961, ROB 1976, 1978–80
Collection RMK, ROB
Literature Wynia 1979; AN 1980, 36; Brunsting 1983, 41

406 Nijmegen – Traianusplein
Excavation ROB 1973–75
Collection ROB
Literature Bloemers 1979b

407 Nijmegen – Hunerberg west
Situation The settlement on this site is the western ward *(vicus)* of the *canabae legionis* belonging to the fortress on the Hunerberg. Sites 407 and 408 may constitute one settlement.
Excavation ROB 1973–80
Collection ROB
Literature Bloemers 1979d, 50, 54

408 Nijmegen – Schildersbuurt
Situation The settlement traces discovered here may be of a military nature. They may also belong to the western *vicus* of the *canabae legionis*
Excavation ROB 1978–79
Collection ROB
Literature Bloemers 1979d, 55–6; AN 1980, 33–35

409 Nijmegen – Museum Kamstraat
Situation The cemetery is composed of five different parts (terrains O, E, S, CC, and OH) which, however, should not be taken to represent different cemeteries (cf. Stuart 1977, 3–6) because their division is due to the modern topography.
Excavation F.M.L. Leydekkers 1906–7, ROB 1975–76, 1978, 1980
Collection RMK, RMO, ROB
Literature Bloemers 1979c

410 Nijmegen – Hugo de Grootstraat
Excavation ROB 1975, 1980–83
Collection RMK, ROB
Literature Wynia 1979; JROB 1980, 34–5; JROB 1981, 30–1; JROB 1982, 33–4; Bloemers 1983, 193–8

411 Nijmegen – Barbarossastraat
Excavation ROB 1974
Collection ROB
Literature AN 1975, 162; Bloemers/Louwe Kooijmans/Sarfatij 1981, 73

412 Nijmegen – Hunerberg
Excavation RMO 1917–21, ROB 1950, RMO/ROB 1951, 1956–67, IOGA 1971–78, ROB 1973–1979
Collection RMO, RMK, IOGA, ROB, Centraal Museum and Fundatie van Renswoude Utrecht, Hessisches Landesmuseum Darmstadt, private
Literature Bogaers/Rüger 1974, 76–80; Bloemers 1979a; Bogaers/Haalebos 1979b; AN 1980, 33–34; Bogaers/Haalebos 1980b

413 Nijmegen – RK Kerkhof
Collection RMK
Literature Stuart 1963; Bloemers 1979d, 56

414 Nijmegen – Broerdijk
Collection RMK
Literature Stuart 1963; Bloemers 1979d, 56

415 Nijmegen – De Klokkenberg
Excavation ROB 1976
Collection ROB
Literature Bloemers 1979c, 36

416 Nijmegen – Hunerberg oost
Situation The settlement on this site is the eastern ward (*vicus*) of the *canabae legionis* belonging to the fortress on the Hunerberg
Excavation ROB 1972–79
Collection ROB
Literature Bloemers 1979d, 51–53; AN 1980, 32–33, 71

417 Nijmegen – Kops Plateau
Excavation RMO 1915–21, 1937, RMO/ROB 1957, IOGA 1972, ROB 1973, 1975–76
Collection RMO, RMK, ROB, private
Literature Bogaers/Haalebos 1979a

418 Nijmegen – Kleine Kopse Hof
Situation The finds are not from systematic excavations, but their origin from this cemetery is fairly secure. The site may have been one large cemetery together with site no. 419.
Collection RMK
Literature ER II, 118, no. 137; Stuart 1963; Bloemers 1979 c, 36

419 Nijmegen – Hengstberg
Situation The finds from this site, almost certainly a large military cemetery, are nearly all lost or of uncertain provenance. Although the general location of the cemetery is known ('Bos van Dommer', 'Ubbergen', 'Hengstberg') its full extent cannot be established. It may include site nos. 418 and 543.
Collection RMK
Literature Janssen 1846, 346–352, 397–400; ER II, 111–117 (at least nos. 134–135); ER III, 99, al. 7–10; Daniëls 1955, 285–290; Stuart 1963, 13–14, 110; Bogaers 1971

420 Ubbergen – Rijksstraatweg
Finds *(a)* x, *(b)* x, *(c)* x, *(d)* x, *(e)* x, *(h)* VEX BRIT on tile, *(o)* T3?, Bö B?, Bö D 9–12, including wasters from kiln, crescent-shaped rims, *(p)* slags, *(q)* iron objects and fragments
Collection private
Literature JAWNN 1980, 14–15

421 Berg en Dal – Heilig Land Stichting
Finds *(l)* bronze fibula, gold ear-drop *(q)* whetstone
Collection private

422 Berg en Dal – Water Meerwijk
Finds *(l)* tuff altar or pedestal with inscription IN H(onorem) D(omus) [D(ivinae)] A[ ] LEG(atus) LEG(ionis) I M(inerviae) S[E]VERI(anae) ALEXSAN[D]RIANAE V(otum) S(olvit) L(ibens) M(erito) FVSC[O] II ET DEXTRO C[O(n)S(ulibus)]
Collection RMK
Literature De Waele 1931, 34, 72, 91 and Pl. I; ER II, 109, no. 116

423 Berg en Dal – Oorlogsmonument
Finds *(g)* as Domitianus
Collection RMK
Literature AN 1966, 87

424 Beek – De Ravenberg
Finds *(d)* Br 36, *(h)* V[on tegula, *(j)* xxx, *(k)* tuff, hypocaust-tiles, tube
Collection private
Literature AN 1968, 75

425 Beek – Kalorama
Finds *(j)* xxx, *(k)* tuff
Collection private, in situ
Literature AN 1968, 76

426 Beek – Keteldal
Situation Finds in secondary position, but probably indicating the presence of a villa higher on the slope
Collection Museum Kranenburg
Literature BJ 145, 1940, 343

427 Beek – Koningstraat
Situation Location reconstructed, exact findspot unknown
Finds *(l)* milestone with inscription [I]MP(eratori) CAE(sari) NE[R]VA(e) TRAIA[NO] AUG(usto) GER(manico) PO[NT(ifici)] MAX(imo) TRIB(unicia) [POT(estate)] P(atri) P(atriae) CO(n)S(uli) [ ].
Collection RMK
Literature ER II, 113–114, no. 126

428 Beek – Marterstraat
Finds *(a)* Drag 18/31 stamp M̂A[RCELL F](retro) (see ch. 6.2.1), Drag 27 stamp V[ ]ES (?), Drag 31 stamps IARVS F (?), LOLIM̂A (retro) and TERTIVS F (see ch. 6.2.1), Drag 33 rosette stamp, *(b)* St 1, Br 3A, Br 17A, Nb 32A, *(c)* xx, *(d)* St 109, St 110B, St 112, St 131, St 132B, Br 36, Br 37, xxx, *(e)* St 201B?, St 216?, St 218, Nb 89, Nb 96, Nb 103, Gose 485, *(m)* RA
Collection RMK, private
Literature AN 1972, 107–108

429 Beek – Smorenhoek
Finds *(m)* LIA, RA?
Collection GAS
Literature Pons/Modderman 1951, 195

430 Persingen – dorp I
Finds *(d)* xx, *(e)* xx, *(m)* LIA/RA
Literature Pons/Modderman 1951, 195

431 Persingen – dorp II
Situation Location reconstructed
Finds *(d)* Br 37?, *(l)* glass jar Isings 67c, engraved gem (Minerva)

Collection RMO
Literature Pleyte 1877–1903 (Batavia), 32

432 Zyfflich – Pepergasse
Finds *(d)* Hofh 78, xxx, *(e)* Nb 89, xxx, *(h)* the stamps mentioned by Rüger 1968, 112, 22 (see fig. 50) may have been found on this site, *(m)* LIA/RA
Collection RLM
Literature Pons/Modderman 1951, 195; BJ 164, 1964, 545

433 Berg en Dal – De Holdeurn I
Situation The site is larger than the area that was (partially) excavated. It includes occupation traces north of the modern road.
Excavation RMO 1845, 1938–1942
Collection RMO, RMK, private
Literature ER III, 100–103; Holwerda 1944; Holwerda/Braat 1946; Braat 1956; AN 1962, 39–40, 50

434 Berg en Dal – De Holdeurn II
Finds *(a)* Drag 18/31, *(b)* c.Nb 53a, *(d)* St 111, *(e)* St 201, St 218, St 219, *(g)* bronze coin Faustina, *(h)* EX GER IN on tegula, *(j)* xxx
Collection RMK
Literature AN 1958, 103

435 Wyler – Vogelsang
Situation In addition to the finds described, the Merovingian material mentioned by Pleyte could also originate from this cemetery.
Finds *(o)* Bö B5b, *(p)* sword (sax) Bö group C, spearhead Bö B3
Collection Museum Kranenburg, RMO?
Literature Pleyte 1877–1903 (Batavia), 33 (?); BJ 171, 1971, 538–540

436 Mehr – Haus Zelm
Situation a.s.s.?
Finds *(d)* Br 36/7, x *(e)* Nb 89, x, *(m)* LIA/RA
Collection private
Literature BJ 164, 1964, 538–39

437 Niel – Pfarrkirche
Situation Finds in secondary location
Finds *(h)* LEG I M ANTO(retro) and LE[on tegula, *(j)* xxx
Collection RLM
Literature BJ 159, 1959, 429; Von Petrikovits 1960, 47–8, note 72

438 Kekerdom – Kaliwaal
Situation The (Early-La Tène!) fragments could have been imported as antiquities by soldiers of the 10th legion, but a more recent importation cannot be wholly excluded.
Finds *(l)* two fragments of bronze 'Iberian' fibulae
Collection RMK
Literature Ypey 1967 (findspot 'Cuyk' is not correct)

439 Millingen – Nieuw Zeeland
Situation No a.s.s. recorded by soil-survey. The building material may be in secondary position (used for a Medieval chapel ?) but this is by no means certain. The RMO excavation has never been published.
Excavation RMO 1936
Finds *(c)* T 1, *(d)* St 138, Br 36, Br 37, xx, *(e)* St 201, St 219, Nb 89, xxx, *(j)* xxx, *(k)* tuff, tubuli, *(m)* LIA/RA, *(q)* wattle and daub
Collection RMO
Literature JRMO 1936, 4; Spann 1967, 35–6

440 Millingen – Vruchtenoord
Situation Location reconstructed (cf. Spann 1967)
Finds *(a)* Drag 27, Drag 31 stamps PRIDIANVS, OF VITAL, BILLICVS, LVCANVS, Drag 35, xxx, *(b)* St 2, xxx, *(c)* girth-beakers, Ha 91, HBW 27c, HBW 52b, HBW 74, *(d)* Ha 45, St 111, xxx, *(e)* St 201, xxx, *(f)* 'yellow glazed' vase, *(g)* bronze coin Traian, silver coin Severus Alexander, *(h)* BENIO on HBW 27c, *(i)* NITV on bronze patera (Den Boesterd 1956, no. 68), *(l)* bronze ornament with Posthumus-medallions, amber box in form of human head, glass bottles, flasks, balsamaria and bowls, bronze objects a.o. Den Boesterd 1956, nos. 68, 79, 104, 106, 236, 298, 311, and 313, fibulae, handles, key and enamelled lid, iron and bronze horse-bit
Collection RMK, RMO?
Literature Mestwerdt 1907, 42–4; Spann 1967, 29–34, 59–65 (incl. various older and unpusblished references); Spann 1972

441 Milligen – Eversberg[24]
Situation a.s.s. (not recorded by soil-survey)
Excavation RMO/W.P.L. van Eyck 1885–87, RMO 1936
Finds See description by Spann 1967, 49–59. The composition of the material is rather curious and represents almost certainly a selection of the original finds.
Collection RMO, GAS, private

24 By accident, findspot 441 has not been split up into two different sites (cf. chapter 1, p. 18) It consists of a settlement (late-Iron Age to middle-Roman) and a Merovingian cemetery.

Literature JRMO 1885, 17, 1886, 60, 1887, 53, 1888, 16, 1936, 4; Spann 1967, 20–28, 49–59; Spann 1972; Peddemors 1975, nos. 66–67

442 Millingen – De Lange Paal
Situation Location reconstructed (cf. Spann 1967)
Finds (see description by Spann 1967, 45–49)
Collection RMO
Literature Spann 1967, 15–20, 45–49

443 Millingen – Heerbaan
Finds *(a)* Drag 31 stamps LOSSAFEC and MATE(rnus), Drag 32 stamp LVGIITV, Drag 33 stamp]C.FE (or]CVSF?), xxx, *(b)* St 2, Br 4a, *(d)* xxx, *(e)* St 218, St 219?, Br 21B, Nb 89, xxx, *(l)* bronze jug c. Den Boesterd 1956, no. 257, bucket and basin
Collection RMO, private
Literature AN 1960, 66

444 Milligen – De Paverskamp
Situation a.s.s. (not recorded by soil-survey)
Excavation ROB 1972
Collection ROB
Literature Hulst 1975

445 Milligen – kerk
Situation The original location of the altarstone is unknown, but presumably in the immediate surroundings (sites 442/443 ?)
Finds *(g)* gold coin Justianianus, *(l)* altarstone of limestone with inscription DEAE DOMINAE RVFIAE [M]ATERNAE ARAM ET [LV]CVM CONSACRAVIT MVCRONIA MARCIA VBI OMNIBVS ANNIS SACRVM INSTITVIT (ante diem) XVI K(alendas) AVG(ustas) [E]T NATALI (die) MATERNAE F(iliae) SVAE (ante diem) [V]I N(onas) OCTOB(res) ET PARENTALI(bus) (ante diem) [I]X K(alendas) MARTIAS RVFI(i)S SIMIL[I] PATRI ET [S]IMILI [F]IL(io) [ET] MATERNAE [F(iliae)]
Collection RMO
Literature ER II, 109–10, no. 117; Spann 1967, 15, 45

446 Millingen – De Pals
Finds *(o)* Bö C?, Bö D ?
Collection private

447 Bimmen – Dorfstrasse
Finds *(a)* Drag 32 stamp]TVS FEC, *(b)* Nb 32D, Nb 33C, *(d)* St 111, St 132A, St 132B, xx, *(e)* St 218, Nb 112B, Nb 104, *(l)* glass fragment, bone pyxis?
Collection Museum Kalkar, private
Literature BJ 172, 1972, 524–25

448 Keeken – dorf
Situation a.s.s.
Finds *(b)* x, *(e)* St 210, Nb 89, xx, *(g)* denarius Lucilla, *(m)* LIA/RA
Collection RLM?; RMK
Literature BJ 164, 1964, 537

449 Keeken – Hufschenhof
Situation a.s.s.
Finds *(b)* Nb 32, *(d)* St 147, Br 36, *(e)* St 218, Nb 89, Nb 104, xx, *(h)* LXG[on tegula, *(j)* xx(x?), *(m)* LIA/RA, *(n)* bracelet Haev ?
Collection RLM
Literature BJ 164, 1964, 536

450 Rindern – Friedhof/Kirche
Finds See description by B. Follman in Bogaers/Rüger 1974, 93–95
Collection RLM, church at Rindern, museum Kalkar, private
Literature Bogaers/Rüger 1974, 93–95

451 Rindern – Frankenhof
Situation a.s.s.
Finds *(d)* Br 36, *(e)* St 210, Nb 104, *(l)* marble ball
Collection private
Literature BJ 164, 1964, 540–1

452 Rindern – Spyck
Finds *(a)* Drag 37, *(d)* St 109, St 131, St 151, *(e)* St 219, *(q)* wood of ship and querns of basaltic lava, probably medieval
Collection private ?
Literature BJ 166, 1966, 609; BJ 170, 1970, 409

453 Kleve – Kanalstrasse
Situation unknown
Finds *(d)* xx, *(j)* xxx, *(k)* tuff
Collection Museum Kleve
Literature BJ 157, 1957, 441

454 Donsbrüggen – Sandgruben
Excavation RLM 1939, 1948
Finds (description not complete) *(a)* Drag 37, Drag 18/31 stamps SECVNDIN F and M[ ]VS F, Drag 31, Drag 45, *(b)* St 2, Nb 32C, Nb 40, Nb 53B, *(c)* girth-beaker, Nb 57, xx, *(d)* Br 37, xxx, *(e)* St 201, Nb 104, Gose 467, xxx, *(l)* iron fibula Almgren 15, bronze wire fibula, 'eyes' fibula, iron knife, glass fragments *(m)* RA, *(o)* Bö D?, *(p)* spearhead, buckle, beads
Collection RLM, museum Kleve
Literature BJ 136/37, 1932, 320; BJ 142, 1937, 318; BJ 145, 1940, 280, 301, 320; BJ 146, 1941, 329–330, 376; BJ 149, 1949, 336, 355; BJ 150, 1950, 152; BJ 159, 1959, 389

455 Donsbrüggen – Kirche
Finds *(d)* St 106B, Br 3A, Br 36
Collection RLM
Literature BJ 159, 1959, 389

456 Donsbrüggen – Heidenkirchhof
Finds *(d)* c. St 110A
Collection Museum Kleve
Literature BJ 148, 1948, 383

457 Donsbrüggen – Gnadental
Finds *(a-e)* x, *(m)* LIA/RA
Collection RLM
Literature BJ 146, 1941, 301

458 Kleve – Nassauer Mauer I
Finds *(d)* Br 36, *(e)* St 201, *(f)* lamp *('Firmalampe')*
Collection Museum Kleve
Literature BJ 159, 1959, 419

459 Kleve – Nassauer Mauer II
Finds *(d)* Hofh 50, St 129A, xx, *(e)* St 210, Br 19[1], x
Collection Museum Kleve
Literature BJ 159, 1959, 419–20

460 Schneppenbaum – Qualburg I
Excavation RLM 1937
Collection RLM, Museum Kleve, private
Literature Bogaers/Rüger 1974, 96–98

461 Schneppenbaum – Qualburg II
Excavation RLM 1937
Collection RLM, Museum Kleve, private
Literature Bogaers/Rüger 1974, 96–98

462 Schneppenbaum – Waldmannshaus
Finds *(a)* Drag 18/31 stamps CELSINVS F and CELSINVS, *(b)* St 2, *(d)* St 111, Br 37, xx, *(e)* Nb 104, *(g)* as Caligula, *(i)* //// on Drag 18/31
Collection private
Literature BJ 166, 1966, 579–80

463 Nierswalde – Cornelienhof
Finds *(d)* Br 36, x, *(e)* x
Collection School Nierswalde
Literature BJ 162, 1962, 547–8

464 Nierswalde – An der Klosterhufe
Finds *(d)* xx, *(e)* xx, *(m)* LIA/RA
Collection RLM
Literature BJ 162, 1962, 547

465 Nierswalde – Parz. Wefers
Finds *(m)* LIA, *(q)* whetstone (LIA or Roman)
Collection RLM
Literature BJ 162, 1962, 547, 549

466 Asperden – Versunkenes Kloster
Excavation RLM 1964
Collection RLM
Literature Bogaers/Rüger 1974, 99–100

467 Kessel – Niers
Finds *(g)* solidi Constantinus I, Constantius II and Magnentius, *(l)* gold ring with polished rock-crystal, iron fragment
Collection RLM
Literature Nachrichtenblatt des Niersverbandes 2, 1936, 22; BJ 142, 1937, 196, 202; BJ 143/4, 1938–9, 292

468 Kessel – Kartenspielerweg
Finds *(a)* Drag 37, *(b)* St 2, *(d)* x, *(e)* St 202, St 210, Nb 104, x, *(j)* xxx, *(m)* LIA/RA
Collection Museum Kleve
Literature BJ 142, 1937, 320; BJ 157, 1957, 424

469 Megen – De Hoge Hof[25]
Literature Modderman 1950, no. 31; AN 1967, 92 and 1969, 114

470 Macharen – Harense Broek
Literature Modderman 1950, no. 34

471 Berghem – Twinkel
Literature Modderman 1950, no. 5

472 Berghem – Hoge Tussenreten
Literature Modderman 1950, no. 6; Knippenberg 1959, 49

473 Berghem – De Lallenberg I
Literature Beex 1955; AN 1956, 23

474 Berghem – De Lallenberg II
Literature Beex 1955; AN 1956, 23; Bogaers 1970b

475 Haren – De Spaanse Steeg
Literature Modderman 1950, no. 35–36; AN 1960, 270; Verwers 1973; Peddemors 1975, nos. 204–232

476 Haren – dorp
Literature Modderman 1950, no. 37

25 For sites 469–503 and 514–515, which are all in the province of North Brabant and south of the Meuse, no further information can be supplied in this catalogue (see also p. 14). In due time, a complete inventory will be published by W.J.H. Verwers.

477 Haren – Berkse straat
Literature Modderman 1950, no 38

478 Haren – Berchems Broek
Literature Modderman 1950, no. 48

479 Deursen – Onze Lieve Vrouwenberg
Literature JRMO 1939, 7; Modderman 1950, no. 47

480 Deursen – Het Hoge Veld
Literature Modderman 1950, no. 46

481 Deursen – De Pachtkamp
Literature Modderman 1950, no. 42

482 Deursen – De Woerden
Literature AN 1968, 118

483 Deursen – Dennenburg I
Literature Modderman 1950, no. 43; AN 1968, 94

484 Deursen – Dennenburg II
Literature AN 1968, 29–30

485 Deursen – Het Steenwerk
Literature ER III, 74, al. 2; Modderman 1950, no. 44; AN 1968, 94

486 Dieden – dorp
Literature Modderman 1950, no. 41; perhaps also Hermans 1865, 116

487 Oss – IJsselstraat
Literature AN 1973, 15, 105 and 1974, 184, 256; Peddemors 1975, nos. 233–4; Verwers 1978; See also Verwers 1981

488 Herpen – Molenstraat
Literature Beex 1973, no. 229

489 Nistelrode – Vorssel
Literature AN 1969, 123

490 Nistelrode – Slabroekse Heide
Literature Remouchamps 1924

491 Schaijk – Gaalsche Heide
Literature Modderman/Isings 1960/61; AN 1971, 98–9

492 Escharen – Graafsche Raam I
Literature AN 1968, 94

493 Escharen – Escharense Veld I
Literature AN 1973, 103; Peddemors 1975, nos. 235–244; Koolen 1978; Verwers/Beex 1978, 20–1

494 Escharen – Escharense Veld II
Literature Verwers/Beex 1978, 20–1

495 Gassel – Rad van Avontuur
Literature Beex 1973, no. 237

496 Groot-Linden – Oostermeerweg
Literature Beex 1967, 63, 66

497 Klein-Linden – Kapel
Literature Hermans 1865, 11; Beex 1967, 67

498 Beers – Dommelsvoort
Literature JRMO 1928, 11

499 Cuijk – centre I
Literature Bogaers/Rüger 1974, 84–7

500 Cuijk – centre II
Literature Bogaers/Rüger 1974, 84–7; Koeling/Koolen 1978

501 Cuijk – Grotestraat
Literature JRMO 1913, 7; JRMO 1914, 7: JRMO 1915, 5; JRMO 1929, 8; Koeling/Koolen 1978, 20

502 Cuijk – Maaskade
Literature JRMO 1970, 264

503 Oeffelt – Heerstraat
Literature Hermans 1865, 6–7; Beex 1973, no. 251

504 Opheusden – Maneswaard
Situation Material found in secondary position as a result of dredging activities
Finds *(e)* xx, *(k)* tuff
Collection private

505 Neersteind – De Nachtigaal
Finds *(c)* HBW 28, *(d)* St 109A, St 131, *(f)* St 302
Collection private

506 Altforst – De Hoge Woerd
Finds *(d-e)* xxx, *(m)* MIA, LIA, RA
Collection GAS

507 Middelaar – Dorpsstraat
Finds *(d)* xxx, *(e)* St 201, Al 27, xxx, *(m)* LIA, RA, *(o)* Bö D 9–12
Collection private
Literature JAWNN 1979, 10–11

508 Middelaar – Witteweg
Finds *(a)* Ch 320, xx, *(b)* St 2, Br 5A, xxx, *(c)* T1, xx, *(d)* St 111, St 129B, St 147, Hofh 78, Br 37, xxx,

(e) St 210, St 218, Nb 89, Nb 104/Al 28, Al 27, xxx, *(g)* follis or aes-III Constantinus I, *(j)* xxx, *(k)* tuff, hypocaust-tiles, *(l)* glass, *(m)* RA *(q)* basalt lava, slags
Collection private
Literature JAWNN 1980, 16–7; JAWNN 1982, 12–4

509 Middelaar – Elkenstraat
Finds *(o)* Bö D 9–12
Collection private

510 Arnhem – Velperweg
Finds *(g)* sestertius Severus Alexander
Collection private

511 Elst – Aam II
Situation a.s.s.
Finds *(p)* sceleton and glass beads
Collection private
Literature JROB 1981, 110; JAWNN 1982, 24

512 Wijchen – De Poort
Situation a.s.s.
Finds *(m)* EIA, MIA *(n)* bracelet Haev 7a
Collection private
Literature JAWNN 1980, 28–30

513 Kesteren – Broekdijk
Situation a.s.s.
Finds *(a)* Drag 45, xx, *(b)* xxx, *(c)* T1, T2, xxx, *(d)* St 109, Br 36, Hofh 78, xxx, *(e)* St 201, St 210, St 213A, St 218, St 219, Nb 89, Nb 104, xxx, *(f)* 'fine Nijmegen' pottery, *(j)* xxx, *(k)* tuff, limestone, other stone with cement, *(m)* LIA, RA, *(q)* wattle and daub, whetstone, decorated bone fragment
Collection GAS

514 Escharen – Graafsche Raam II[26]
Literature Lafaurie 1958, 1960

515 Cuijk – Padbroek[26]
Literature JAWNN 1975, 21–3; 1976, 19–22; 1977, 13–14; Koeling 1977, 118–9; Verwers 1981

516 Andelst – Willem Alexanderstraat
Finds *(c)* xxx, *(e)* xx, *(m)* MIA?, LIA, *(o)* Bö D 9–12
Collection GAS
Literature JROB 1981, 111

517 Bemmel – Kattenleger
Finds *(b)* St 2, *(d)* St 110B, Br 37
Collection private
Literature JROB 1981, 110

518 Beuningen – Krommehoekstraat
Finds *(o)* Bö D 9–12
Collection GAS

519 Wijchen – Het Sterrebosch
Finds *(o)* Bö B1a, Bö B3b, Bö D 1, Bö D 9–12, La Baume 1967, type D 8, *(p)* francisca Bö B1, iron spearhead and axe
Collection MFB, private
Literature JROB 1981, 111; JAWNN 1981, 10–1

520 Alverna – Leemweg I
Finds *(m)* EIA, LIA
Collection RMO, MFB
Literature Peddemors 1978, no. 235

521 Alverna – Valendries E
Finds *(l)* bronze fibula, *(m)* EIA, LIA
Collection RMO
Literature Modderman 1951, no. 85; Peddemors 1978, no. 234

522 Alverna – Leemweg II
Finds *(m)* EIA, LIA
Collection RMO
Literature Peddemors 1978, no. 216

523 Alverna – Geitweg II
Finds *(m)* EIA, MIA, LIA
Collection AWNN, GAS
Literature Peddemors 1978, no. 178

524 Alverna – Meiberg
Finds *(a)* xx, *(c)* Br 16C, *(d)* Hofh 78, xxx, *(e)* Nb 89, St 219, xxx, *(j)* xxx, *(m)* MIA, LIA, RA, *(q)* loomweight
Collection MFB?
Literature AN 1957, 53; Peddemors 1978, no. 159

525 Middelaar – Van Heumenstraat
Finds *(b)* xx, *(d)* Hofh 78, xxx, *(e)* St 201, St 210, xxx, *(m)* RA, *(q)* wattle and daub
Collection private

526 Milsbeek – Kerstenberg
Finds *(a)* Drag 18/31, *(b)* St 2, St 11, Br 7A?, *(c)* Br 16C, *(d)* St 109B/110A, Br 8, Br 36
Collection private
Literature JROB 1981, 152; JAWNN 1981, 16–8

26 See p. 129, note 25.

527 Mook – Plasmolensehof
Finds *(d)* St 151B, *(j)* xx, *(l)* iron knife
Collection private
Literature JROB 1981, 152

528 Ubbergen – Berkenlaan
Finds *(a)* stamp of the *legio I Minervia Antoniniana (j)* x
Collection private

529 Homoet – De Blauwe Den
Finds *(d)* St 110B, *(e)* Br 14A
Collection GAS
Literature JROB 1981, 110

The data from the following sites, most of which were discovered or reported in 1981 or 1982, are not shown on the Appendices which were being printed when the information became available. For this reason, the approximate co-ordinates are also indicated under 'other data'

530 Heteren – Het Lage Land II
Finds *(a-f)* xxx, *(l)* bronze fibulae, bronze bowl, iron object, *(m)* RA
Collection GAS, HKKO, private
Other data location 181.2/439.0

531 Oosterhout – Elzenwoerd
Situation a.s.s. (not recorded by soil-survey)
Finds *(a-e)* xxx, *(j)* xxx, *(l)* bronze stilus, glass, *(m)* RA
Collection private
Literature JROB 1982, 120
Other data location 184.4/431.9

532 Druten – Genechtstraat
Finds *(g)* denarius Marcus Antonius
Collection private
Literature JROB 1982, 120
Other data location 169.5/433.2

533 Kekerdom – Millingerwaard
Situation eroded a.s.s.
Finds *(a)* xxx, *(b-e)* xxx, *(j)* xxx, *(k)* xxx
Collection private
Literature JROB 1982, 121
Other data location 197.1/431.7

534 Heumen – Rijksweg
Finds *(a)* xx, *(b-e)* xxx, *(j)* xxx
Collection GAS
Literature JROB 1981, 110; JROB 1982, 120
Other data location 186.1/420.5

535 Druten – Kerkstraat
Finds *(m)* LIA
Collection GAS
Other data location 168.5/433.6

536 Angerlo – Kellse Vicarie
Excavation ROB 1982
Collection ROB
Literature JROB 1982, 33
Other data location 206.7/445.6

537 Ouddijk – Hees
Finds *(b-e)* xxx, *(o)* Bö D 9–12
Collection Museum Zevenaar
Literature JROB 1982, 119
Other data location 206.0/437.3

538 Zevenaar – Steenheuvel
Finds *(a)* Ch 320, *(b-e)* xxx, *(n)* bracelets Haev 3a, 7a
Collection Museum Zevenaar
Literature JROB 1982, 120
Other data location 202.5/436.9

539 Mook – Startse dijk
Situation The site was discovered in 1954. Although its location was recorded precisely, the find circumstances were not. Most of the finds clearly indicate a settlement, but they also include material from burials, undoubdedly belonging to the cemetery at site 383.
Finds *(a)* Drag 18/31, Drag 31, Drag 33, Drag 45, *(b)* St 11, St 22, Br 3, Br 4A, Br 17, xxx, *(c)* xx, *(d)* St 107, St 109, St 110B, Br 36, xxx, *(e)* St 210, St 218, T 4, Br 21A, xxx, *(j)* xxx, *(l)* bronze enamelled figure fibula, bronze pendant, bone disc, *(m)* RA, *(o)* Bö D 9–12
Collection private
Literature JROB 1982, 169
Other data location 190.3/417.0

540 Mook – Plasmolensehof
Finds *(g)* as Antoninus Pius
Collection private
Other data location 191.7/416.8

541 Mook – Bisselt
Finds *(g)* dupondii Antoninus Pius
Collection private
Other data location 191.0/419.3

542 Wageningen – Kleine Doove
Situation a.s.s. (not recorded by soil survey)
Finds *(m)* LIA/RA

Collection GAS
Literature Van de Westeringh 1983, addendum between p. 13 and 14
Other data location 171.8/440.1

543 Nijmegen – Eversweg
Situation The site could be a part of site 419
Finds *(c)* HBW 28, xx, *(d)* St 129A, x, *(e)* x, *(m)* xx
Collection private
Other data location 190.6/427.0

6 THE FINDS

6.1 INTRODUCTION

In a study like the present one, the material evidence, that is, the finds, play a major role in several ways. Their importance exceeds by far that of being instrumental in dating sites or in dividing them into cemeteries, settlements, etc. This is, of course, their primary function, but they can and should be employed to develop hypotheses regarding as many aspects of pre- and protohistoric society as possible. The opportunity to do so is, however, limited by two groups of interrelated problems.

The first set of problems concerns the relation between the observed variability in the archaeological record and the economic, social, or other aspects of past behaviour. This is partly a matter of interpretation. Simple operationalizations at a practical level would result in questions like: does the introduction of bronze coinage imply a market economy, does the presence of terra sigillata imply wealth and/or social status, etc.[1] It is also a matter of conservation, because the observed variability in the archaeological record is clearly different from that in the originally deposited material, and even from that in the fraction that is potentially still available.[2] Essentially, hypotheses have to be formulated and conclusions have to be reached on the basis of only a small portion of the already restricted amount of material that has survived until today.

The second set of problems is of a more prosaic nature. The quality and quantity of the material from the eastern river area which is used in this study is, comparatively, very good. It is varied, generally fairly accurately datable, and usually the find-circumstances are reasonably well documented. In a statistical sense it is, however, in no way a systematic or a random sample of what is available. It is composed of material from fairly recent and adequately collected surface finds, but also from older and recent collections which are clearly not representative of all the material at a site and of accidental finds recovered during many kinds of earth-removing activities for building purposes, the digging of ditches, and the like.

Moreover, there are several reasons why material could not be used, such as the finds from unpublished excavations which are only sporadically incorporated, finds which could no longer be located, those which were not available for study, and those which were for some reason rejected. The latter include at least one category

1 Following the first reports inspired by the ideas of the New Archaeology, there is by now a vast amount of literature on this subject, from highly theoretical considerations to practical applications and very critical appreciations. This cannot be evaluated here, but, if anything has become especially clear over the past decade, it is that interpretations of relations between the archaeological record and the structure of the society that left it are neither simple nor straightforward.

2 See e.g. Schiffer 1976.

which could have been very informative. These are the generally rather special finds discovered during dredging activities, which regrettably almost invariably have somewhat obscure find-circumstances.[3]
The usefulness of the find-material presented here is thus unsuited to the application of sophisticated statistical techniques. None the less, it contains a great deal of valuable information for other purposes. In this chapter, a number of find-categories are treated in more detail in order to present some of that information, which would otherwise remain hidden in the catalogue. This special treatment does not extend to all find-categories. For varying reasons, some were omitted. The rare or unique finds will be discussed later, whenever they provide information on a specific subject. Among these is also the metalware, which is very diverse, with the exception of only three frequent types of finds: nails, fibulae, and coins. The chronological information yielded by the last two is, of course, employed in dating sites, but single coins especially are not suitable for that purpose.
A separate discussion of the coins found in the region will be published in due time.[4] This is necessary, because a numismatical evaluation has to incorporate all coin-finds in the area. The catalogue does not include excavation finds or, with the exception of a few very important finds, coins whose provenance is not fully established. Comparable arguments apply to the fibulae.[5] Moreover, after closing the catalogue it was discovered that the data concerning metal objects in particular were incomplete. This is partly due to the exclusion of dredging-finds mentioned above, but even more to the activities of a whole new category of collectors, who search known sites for metal objects with the help of detectors. None of these collectors belonged to the group of amateur archaeologists whose collections were examined.
The seemingly fairly limited numbers of coins, fibulae, and other metal objects may drastically alter in the next decade or so, after incorporation of the wave of detector-finds. Even more importantly, it may to some extent also alter possible interpretations, for instance, of the circulation of money in certain periods. The recent discovery of more than 300 mainly 4th-century coins at site 239 (probably not from a hoard) is the most striking example.[6] This could well be an exceptional case, but there is no proof for that until more sites have been systematically investigated.[7]

Typology and Chronology

Two of the matters to be discussed in this chapter are typology and chronology. This is only done in those cases where the start of an inventory for the region and the presentation of more detailed data than those given in the catalogue seemed useful (terra sigillata), and also where new information on typochronological issues could be provided. For most of the pottery, this is neither necessary nor possible, because adequate typochronological series are already available in the copious literature on this subject and the pottery-finds from the eastern river area being discussed here are generally unstratified and therefore unsuitable for this purpose. For some of the other finds, the additional information consists of an overview of the variation and significance of the evidence dispersed in the catalogue, supplemented as much as possible with data from excavations.
Apart from typology and chronology there are two other matters which are discussed whenever relevant. These are the problems of import versus local production and of the significance of observed frequency and distribution.

Production

The question of what was produced and what was imported in the eastern river area will be evaluated in more detail in the second part of this study, when the different aspects of the economy are discussed. In this chapter, the production of some materials, but especially of pottery, is considered. In the absence of kiln-sites, it is often difficult to decide whether pottery was manufactured lo-

3 At first, it was decided to accept at face value the stories connected with objects which were *presented* to a museum and reject those that were *sold*, on the assumption that in the first case there was no reason for doubt. This decision quickly proved to be unrealistic, and therefore most of these finds were not taken into consideration.
4 Verwers, in prep.
5 An inventory of several groups is being prepared by Dr J.K. Haalebos (pers. comm. 31.10.1978).
6 See chapter 5, note 19.
7 For an evaluation of the efficiency of metal-detectors, see e.g. Crowther 1981. In this respect it is worth noting that the systematic collection of metal objects by means of a detector in the occupation layer of the settlement at Wijk bij Duurstede-De Horden (*JROB* 1977, 36; 1978, 42; 1979, 53; 1981, 37) caused a very significant increase of metal excavation finds.

cally or imported. Kilns are relatively scarce in the region, but at least there are one or more examples for each of the periods under consideration.

The earliest one[8] is the kiln from Bemmel (165) which, in view of its date,[9] is already very sophisticated. It is an updraught kiln, which is also the most common type during the entire Roman Period. A possible second kiln was excavated in Ressen (151) in 1948.[10] In this case, if we are indeed dealing with a kiln, the native pottery was fired in an open pit. This method of firing pottery has also been observed elsewhere for an earlier period.[11] The coexistence of firing-techniques at different levels of technical sophistication is, however, a perfectly normal phenomenon, and one which can also be observed today. The kiln-pit from site 151 could date to the Late-Iron Age or Early-Roman Period, or even later. Certainly early-Roman potting activities have only been reported for sites 412 (Nijmegen-Hunerberg) and 499 (Cuijk).[12] For the Middle-Roman Period, potter's kilns have been found at sites 399 (Nijmegen-Waterkwartier) and 433 (Holdeurn).[13]

This is the total of actual kilns discovered so far, but there are also other indications. Probable wasters of native pottery have been found at sites 192, 212, 222, 301, 341, and 479(?), of middle-Roman wheel-turned pottery at sites 1, 59, 250, 407, and 416(?). Hinz and Hömberg[14] have shown that late-Roman glazed ware was probably produced at site 466. Other late-Roman pottery may have been manufactured at site 7.[15] A large number of clear wasters and other material indicate that one or more (late-) Merovingian kilns were certainly present at site 420. Production of Merovingian pottery at site 499 is also a probability on account of a considerable number of wasters.[16] Apart from wasters, which do not always constitute incontrovertible evidence for local production because single sherds which landed in a fire by accident may also look like a waster from a kiln, there are a number of other finds indicating production of pottery. Parts of kilns were identified at sites 48(?), 299, and 315.[17] A number of collections also contained slags which were different from the normal (iron-) slags, and may point to pottery production. These were present in the material from sites 59, 62(?), 98(?), 139(?), 196, 226, 270, and 312. Apart from pottery, related materials such as brick, loomweights, and spindle-whorls were locally produced as well. There is also evidence for the production of glass in the region, and numerous slags testify to the smelting of iron.

Frequency and distribution

Because of the problems already mentioned above, such as the incomparability of sites and the lack of information about some of them, it is only possible to discuss frequencies and distributions in fairly general terms. It should be kept in mind that, for example, not all of the enumerations of sites 'with finds of type x' are reliable in an absolute sense because some collections were not studied.

On the other hand, the total amount of material examined is large, varied, and from many different sites so that, at least in a relative sense, acceptable statements about frequencies and distributions can be made. It seems very unlikely that conclusions about the relative frequency of most find-categories will change drastically. Future research will undoubtedly show denser distribution patterns, and recurring combinations of certain classes of finds at certain sites can probably be further elaborated. These are, however, more or less eternal problems in research, and not special ones.

At a regional level, it seems acceptable that the total quantity and variability of pottery is proportionally related to the fraction that has been studied, although certain distortions have to be taken into account. For example, the presence or absence and relative quantities of terra sigillata and those of native ceramics require a different approach because their chances of being seen, of

8 The kiln material and wasters from Duiven (196) are probably of middle-Iron Age date and therefore fall outside the scope of the present discussion. There is nothing among the burnt-clay fragments that points to a real kiln, so we are probably dealing with a kiln-pit.

9 Two charcoal samples yielded dates of 2250 ± 55 BP (GrN–7226) and 2150 ± 50 BP (GrN–7227), which places the kiln at the transition of the Middle- to the Late-Iron Age.

10 Braat 1949, 31 and Pl.IX, 2–3.

11 Willems 1983b, 229–34 (Early-Iron Age) and note 8 (Middle-Iron Age?).

12 Brunsting 1964, 308 and Bogaers 1966, 87.

13 Daniëls 1927, 90–3; Holwerda 1944, and Holwerda/Braat 1946.

14 1968, 184.

15 See note 106.

16 Pers. comm. J.R.A.M. Thijssen, autumn 1980.

17 The fragment of an oven floor from site 143 mentioned by Braat 1949, 37 is probably a sieve or cheese press.

actually being picked up, and of inclusion in a collection, are definitely not equal. The possibility of different rates of recovery in different parts of the region has already been discussed in chapter 1, but apart from that, there is also the theoretical possibility of different rates of recovery for successive chronological periods. In practice, however, this does not seem to be an important potential source of bias. Almost all the settlement sites in the entire region are shallow and the earliest material may also be found at the surface, even at the more deeply stratified sites.[18]

Nonetheless, these considerations only point to the fact that at least some material from the deepest levels of stratified sites will generally be found in a surface collection. They do not imply that this material is representative of the total range of variation of artifacts present in those layers. Therefore, especially late-Iron Age and early-Roman finds may be underrepresented at a restricted number of settlement sites.

6.2 WHEEL-TURNED POTTERY

6.2.1 Terra Sigillata

Of the 290 settlement sites (including probable and possible settlements) where Roman ceramics were found, no fewer than 139 (48%) yielded terra sigillata (t.s.) and the same is true for 55 of the 84 Roman Period cemeteries and single graves, all of which contained Roman pottery. In quantitative terms, the overwhelming majority has, of course, been found on the Nijmegen sites. There is no way in which that material can be evaluated precisely, because only a part of it, and predominantly from cemeteries,[19] has been published. A few observations can be made on the overall distribution of terra sigillata, both in quantitative and in chronological terms.

Frequency

The majority of the settlements where terra sigillata was found had only a few sherds: in 96 out of 139, no more than 10 and usually fewer examples were counted; 11 settlements yielded 11–25 sherds and 32 settlements more than 25 pieces. These last two groups of sites[20] are listed on fig. 24.

One of the most interesting questions to be answered is, of course, whether the (quantitative) distribution of t.s. is uneven or not, and if it is, in what direction and to what degree. On the basis of the material studied, however, this proved to be a difficult problem to solve. There is no way in which this material can be considered a random sample: it is, on the contrary, heavily biased. There are many differences in the opportunities of finding material at a site, in the way in which available sherds were collected, how they were dealt with afterwards, whether or not they were available for study, and so on.

There are, however, several reasons for assuming not an absolute but at least a relative proportional rate of recovery of t.s. sherds. First, the probability of a t.s. sherd being included in a collection is higher than other pottery. Not only is it more easily discovered by its colour, any amateur archaeologist walking a field with overflowing pockets will still pick up that last piece of sigillata although he would not bother to bend down for other sherds. We may assume, therefore, that t.s. is generally overrepresented, but, at the same time, that the quantities picked up at each site are more or less comparable to each other, given that the opportunities for collecting sherds are the same.

In this last respect, the most important distinction is not between (partially) excavated and other sites. Archaeological excavation is, of course, usually a fine way of collecting sherds, but so are other methods, such as the meticulous search through heaps of earth resulting from all work involving soil-removal: an activity often engaged in by amateur archaeologists. There is, therefore, a major distinction between large collections resulting from earth-removal and small collections generally resulting from field-walking. These two cannot really be compared, because the incidence of t.s. in a collection is clearly primarily a function of the total number of sherds. This conclusion is supported by the fact that of the 290 settlements to be considered the 151 with no t.s. almost all yielded small or very small quantities of Roman mate-

18 Which is due to the *opspit*-phenomenon discussed above, p. 72.

19 For the cemeteries, see Breuer 1931, Vermeulen 1932, Brunsting 1937, Stuart 1977. For the castra, see Bogaers/Haalebos a.o. 1976, 1977, and 1980b.

20 Due to lack of information an occasional site may be absent from this list, for example nos. 135, 395, 450, and 487.

Fig. 24 Settlements[21] with more than 10 sherds of terra sigillata.

Column 1: x = excavated sites or sites where large amounts of pottery (at least 250 sherds) were collected.
Column 2: x = Sites with (a) stone building(s).
Column 3: x = sites associated with military affairs, e.g., because the material contains military tile-stamps or other 'military' artifacts (without the implication that they are themselves military)
xx = sites strongly associated with military affairs (implying that they are (probably) military)

11–25 sherds		1	2	3
7	Bennekom – Achterstraat	x		x
23	Arnhem – Rosandepolder		x	x
62	Opheusden – De Roetert	x		
105	Elst – dorp	x	x	x
222	Winssen – Het Oude Veerhuis	x	x	x
233	Ewijk – De Woerdjes	x		
239	Ewijk – De Grote Aalst	x	x	
275	Hernen – De Wijnakker	x	x	
299	Wijchen – De Pas, Passerot II	x	see 298	see 298
341	Lunen – dorp	x	x	x
377	Mook – Kloosterberg	x	x	x

>25 sherds		1	2	3
1	Ede – Veldhuizen	x		x
38	Kesteren – Nedereindsestraat	x		x
48	Opheusden – De Zeven Morgen	x		x
59	Hien – De Wuurdjes	x	x	x
117	Driel – Baarskamp/Lizesland	x	x	xx
118	Driel – Marskamp		see 117	see 117
126	Arnhem – Meinerswijk	x	x	xx
182	Herwen – De Bijland	x	x	xx
194	Loo – Loowaard	x	x	xx
214	Druten – Klepperhei	x	x	x
232	Ewijk – Ewijksche Velden	x		
234	Ewijk – De Hoge Woerd	x	x	
244	Beuningen – De Heuve	x		x
250	Lienden – Reekstraat	x	x	
289	Hernen – De Loffert II	x		
298	Wijchen – De Pas, Passerot I	x	x	x
315	Wijchen – Tienakker	x	x	x
391	Malden – Heumensoord	x	?	xx
399	Nijmegen – Waterkwartier	x	x	x
403	Nijmegen – Valkhof I	x	x	xx
406	Nijmegen – Traianusplein	x	irrelevant	xx
407	Nijmegen – Hunerberg west	x	x	xx
408	Nijmegen – Schildersbuurt	x	x	xx
412	Nijmegen – Hunerberg	x	x	xx
416	Nijmegen – Hunerberg oost	x	x	xx
417	Nijmegen – Kops Plateau	x	irrelevant	xx
433	Berg en Dal – De Holdeurn	x	x	xx
441	Millingen – Eversberg	x	x	x
460	Schneppenbaum – Qualburg	x	?	xx
466	Asperden – Versunkenes Kloster	x	x	xx
499	Cuijk – centre I	x	x	xx
500	Cuijk – centre II	x	x	x

rial. At the other end, inspection of fig. 24 reveals that settlements with more than 10 t.s. sherds virtually all had large or very large amounts of other fragments (hundreds or even thousands). This not only justifies the conclusion that the incidence and quantity of t.s. is a function of total numbers recovered but it also points to the fact that t.s. must have been widely available. The situation is that where enough sherds are present t.s. will always be found among them; this means that not just in 48% but probably in 100% of the settlements at least some t.s. was used. Whether this holds good for all Roman periods will be considered below.

Unfortunately, this wide availability of t.s. makes it even harder to reach conclusions about quantitative distributions for settlements in the region. There is, of course, a very uneven distribution when the large Nijmegen sites are compared to all the others. This is, again, mainly due to the total amount of material collected, but there is no doubt that the many thousands of t.s. sherds from Nijmegen can never be equalled by other sites.

When these other sites are considered, it is important to note that not all settlements with large or fairly large amounts of material are registered on fig. 24. For example, sites 43, 75, 93, 109, 123, 143, 150, 151, 192, 235, 270, 355, 461, and 479 are lacking: they all yielded only 1–10 t.s. sherds. Although this question can only be accurately evaluated when more material and more precise counts of several sites are available, it looks as if there could be differences in numbers of t.s. between sites in the region.

In order to explore this further, two other variables have

21 Note that sites 117–118 and 298–299 are closely associated.

been added to the list on fig. 24. These are the presence or absence of stone buildings at the site and the association with military affairs. In both cases some relation may exist. Of the 55 settlements where certainly, probably, or possibly stone buildings occur, no fewer than 29 are listed. When all 'possible' cases are omitted, these figures become 45 against 29. On the other hand, there are 235 (or 245) settlements without evidence for buildings in stone, of which only 15[22] are listed. The importance of these figures is reduced by the fact that 31 (29) of the 55 (45) sites have yielded large or very large amounts of pottery, while for the other 235 (245) sites that number is only 36 (38). The relation is not entirely spurious, because among the 29 settlements with a lot of sigillata and stone buildings, 28 may be in the category 'large amounts of pottery'; but approximately the same is true for the 14 settlements with a lot of sigillata but without stone buildings, 13 of which are in that same category. The relation is, however, not particularly strong.

When the variable 'military association' is introduced, the relations become even more complicated, because 'military' and 'stone-building' are strongly associated. Except for early-Roman military settlements, all certainly military camps are (partly) built in stone.[23] Unfortunately, it is impossible to define sharply which of the sites are military. The canabae around the Nijmegen fortress (sites 407, 408, and 416) and the Holdeurn (433) are, in some ways, military sites. The same is true to a lesser extent for a number of possible vici (23, 38, 118, 461, and 500), if these are indeed such settlements. Then there are the probable camps (117, 194) and a number of sites with military tile-stamps, as given in fig. 24. The problems connected with the 'military' interpretations are discussed elsewhere. Because, 'military' is, of course, the antecedent variable in relation to 'stone-building', that would explain much of correlation between the latter and the variable '> 10 sherds of t.s.'.

Because 'military' cannot be sharply defined we must be content here with some observations on the relation between 'military' and quantity of t.s.. It turns out that all certainly military camps (126, 182, 403, 406, 412, 460, 466, and 499) are in the >25 category. It could be objected that almost all of these have been excavated, but in several cases that argument is not valid. For example, within the first half hour of the discovery of site 126, no less than 14 t.s. sherds were found under unfavourable circumstances (in mole-hills). The data are less precise for other sites, but dozens of t.s. surface finds from sites 403 and 412 are stored in the Museum Kam in Nijmegen, while those from site 182 were brought up by dredging, and the first t.s. from site 466 by digging in the 1870s.

Find-histories of other sites cannot be reconstructed in detail, except for site 406, which was exceptional because it was not indicated as an early military camp by surface finds before its discovery in 1974. In this respect it is equally significant that the three potential and unexcavated camps (117/8, 194, and 450[24]) also yielded more than 25 t.s. sherds and that the canabae and all but one[25] of the possible vici are indicated on fig. 24.

It may thus be concluded that there is a definite connection between 'military' settlements and a high incidence of t.s.. On the other hand, a number of 'civil' settlements also yield considerable quantities. This may partly be explained by high total numbers of recovered material, but other causes, such as a relation with stone buildings, cannot be excluded. In this respect, the significance of terra sigillata in relation to other characteristics of settlement sites will be evaluated in the forthcoming second part of this study (chapter 8).

Chronology

One important factor in the distribution and frequency of t.s., namely its chronology, has so far been omitted. Because the material of major excavated sites cannot be considered, a detailed evaluation is again impossible. In general, the overwhelming majority of all t.s. can be dated to the Middle-Roman Period or, more precisely, to the second century and the first half of the third. Flavian sigillata is abundant in Nijmegen, but it also occurs in small numbers in several other sites. When pre-Flavian sigillata is considered, the distribution is restricted or very restricted.

As far as South-Gaulish ware is concerned, there are some difficulties in its determination. Because of the

22 It should be noted that these figures include such sites as 406 and 417 which could not have had stone buildings because both sites are too early.

23 The late-Roman burgus of Heumensoord (391) and also that in Qualburg (460) may be other exceptions.

24 The published information on site 450 gives this impression, but it could not be verified. Site 450 is therefore omitted on fig. 24.

25 Only little material is available from site 461, which may have been a vicus or a *statio beneficiariorum consularis*.

fragmentary nature of the material, it is sometimes very doubtful whether a sherd is pre-Flavian or not. However, certainly pre-Flavian South-Gaulish sigillata occurs in a number of sites outside Nijmegen, a.o. 23, 48, 62, 93, 117, 126, 234, 239, 315, 331, 336, 385, 499, and 501. Of these sites only nos. 117, 126, and 315 included examples which should certainly be dated to before ± AD 50. Two of them also have Arretine ware. A complete list of all sites with Arretine ware is presented in fig. 25.

It is clear that the distribution of the earliest sigillata must be connected with the presence of the Roman army. In terms of absence/presence on sites, it seems that t.s. increasingly spread out over the entire region throughout the 1st century, but that its distribution remained restricted to a limited number of sites. In terms of quantity, no degree of comparability ever existed. In the Early-Roman Period, t.s. was virtually absent outside the Nijmegen sites and it was not until Flavian times that it occurred in at least some quantity in the surrounding region. Compared to Nijmegen, however, Flavian t.s. was still scarce in the region. This situation only changed in the 2nd century, when t.s. finally became significant in quantitative terms outside Nijmegen.

The conclusion reached above, that t.s. was used in probably 100% of the settlements, must be restricted therefore to the period after AD 100. It is not quite possible to determine exactly how long this situation lasted. For example, the relatively late dish Drag 32 is very poorly represented,[26] but the equally late mortarium Drag 45 has a wide distribution. The incidence of late-Roman t.s. is also quite instructive in this respect: of the 62 settlements[27] with late-Roman material, 29 had late (4th- or early 5th-century) t.s.. These are sites 1, 5, 7, 21, 46, 79, 114, 123, 126, 135, 143, 150, 154, 156, 168, 244, 250, 289, 291, 298, 315, 391, 399, 403, 460, 466, 495, 499, and 500.

This means that 47% of the late-Roman settlements had t.s., a figure that compares well with the general figure of 48% which was calculated earlier for all settlements. To conclude that t.s. was available to everyone up to the very end of the Roman occupation of the region is, however, not entirely justified because possible sites are omitted and it has to be taken into consideration that an unknown number of late-Roman sites may not have been recognized as such. In quantitative terms, it seems that, just as in the preceding period, people in some locations possessed much more of it than people elsewhere. Most of the late-Roman t.s. comes from the proven military sites 391, 403, 460, 466, and 499. The fact that these have been (partially) excavated may be ignored, because other sites with late t.s. (1, 7, 123, 126, 143, 150, 244, 250, 298, 315, and 399) all have large total numbers of sherds (or were excavated) as well.

Fig. 25 Sites with Arretine sigillata

No.	Name	Type of site
3	Ede – Manen	settlement
117	Driel – Baarskamp	settlement (probably military)
126	Arnhem – Meinerswijk	settlement (military)
336	Alverna – Heumenseweg	cemetery
400	Nijmegen – Kronenburgerpark	cemetery
403	Nijmegen – Valkhof	settlement (probably military)
406	Nijmegen – Traianusplein	settlement (military)
409	Nijmegen – Museum Kamstraat	cemetery (partly military?)
412	Nijmegen – Hunerberg	settlement (military)
413	Nijmegen – R.K. Kerkhof	cemetery (military)
417	Nijmegen – Kops Plateau	settlement (military)
418	Nijmegen – Kleine Kopse Hof	cemetery (military)

Imports and imitations

All sigillata was, of course, imported into the region, although at least during the 1st century imitations of it were made locally by Gallo-Belgic and/or military potters. Evidence for this was found in Nijmegen at the Hunerberg (407, 412, 416),[28] in Berg en Dal at De Holdeurn (433),[29] and in Cuyk on the site of the fort (499).[30] Other 1st-century imitations in terra rubra or terra nigra may, in principle, also have been produced locally, but because no evidence for this has been found so far, importation of most of this pottery from further south must be assumed.

26 This may have been caused by an abnormal distribution, because both in relative and in absolute numbers this dish seems to be well represented only on military sites.

27 Possible late-Roman settlement-sites have not been included.

28 Haalebos/Thijssen 1977.

29 Holwerda 1944.

30 Bogaers 1966, 67.

As far as imitation is concerned, it is worth noting that the most obvious imitations of terra sigillata, in both form and fabric, occur only in periods when the real product was scarce or not even available. The conclusion reached above, that in our region terra sigillata was in principle available to everyone after AD 100 to the end of the 3th century, is supported by the fact that clear imitations are virtually restricted to the 1st century on the one hand and to the 4th and 5th–6th centuries on the other.
The 1st century imitations are especially forms in terra rubra and also in fine Nijmegen ware.[31] As was to be expected, they occur in greater quantities only at the Nijmegen sites. Imitations in the 5th and 6th centuries are the red-brown slip ware: the so-called *rotbraun gestrichene Ware* discussed in paragraph 6.2.2,[32] and African red slip ware, which so far is lacking from the eastern river area. A Nijmegen concentration for *rotbraun gestrichene Ware* has not be established. The few examples known so far are all from the river area and are almost certainly imports, possibly from Mayen or Trier.
As far as the centres of manufacture of regular terra sigillata are concerned, it is impossible to assess their relative importance for the supply of the eastern river area. Most of the t.s. sherds available for study are surface finds. Material from unpublished excavations could not be included and much of the decorated and/or stamped sigillata mentioned in the site-catalogue could not be traced or was not accessible for inspection. Therefore, the material presented in the lists of decorated and stamped sigillata is only a tiny fraction of the material recovered and unsuitable for further interpretation. It is presented only as a first inventory of sigillata from known findspots, which would otherwise have been left unpublished. It will become useful for detailed analysis only after the addition of the unpublished sigillata from excavations.
The inventory of decorated sigillata[33] has been compiled by grouping the material according to production centre. Within the different groups, sherds have been listed according to site of origin, followed by the type of vessel concerned. The stamps are recorded in alphabetical order, followed by the site and the type of vessel.

Fig. 26 South Gaulish terra sigillata. Scale 1:2. ▷

Parts of broken stamps are marked [ ].
Although, in principle, the information on a particular stamp is derived firstly from Oswald's work,[34] this has been replaced in quite a few instances by more up-to-date information, especially for those stamps whose identical dies have already been published by B.R. Hartley.[35]

LIST OF DECORATED TERRA SIGILLATA (Figs. 26–32)

South Gaulish terra sigillata

1 Site 62. Drag. 37. Ovolo probably from Biragillus (Oswald 1948, Pl. XXX, 1) and similar to ovolo from Mascuus (Simon 1962, *Abb.* 1, 10). Leaf Karnitsch 1959, *Taf.* 16,3? (Mercato). For style see Knorr 1912, *Taf.* XXI and Vermeulen 1932, Pl. XVIII,1 (identical ovolo and leaf, but wrong parallels). *Early-Flavian.*

2 Site 62. Drag. 29. Upper frieze with metope decoration by means of wavy line.

3 Site 62. Drag. 29. Horizontally imbricated pinnate leaves Knorr 1919, *Taf.* 58, D (Modestus) and 1952, *Taf.* 2, C (OF ARDACI) and *Taf.* 48, A (OF PASSENI), Glasbergen 1948 b, *afb.* 1,7 (Murranus). *Claudius-Nero.*

4 Site 75. Drag. 29. Lanceolate bud Hermet 1934, Pl. 12, 40. Upper frieze bordered by bead line. For style, see a.o. Knorr 1952, *Taf.* 83, B. *Flavian.*

5 Site 109. Drag. 37. Satyr with grapes Oswald 597, see Karnitsch 1959, *Taf.* 16, 6 and Bloemers 1978a, *Abb.* 96, 129. Pan Oswald 714, see Karnitsch 1959, *Taf.* 15,6 and 16,2. *Style of Mercato. Flavian.*

6 Site 117. Drag. 37. Basal wreath like Knorr 1952, *Taf.* 30, C, E (GERMANI). Standing figure possibly Diana. *Late-Flavian.*

7 Site 117. Drag. 29. Upper frieze with metope decoration: sloping wavy lines and arrowheads, see a.o. Knorr 1919, *Taf.* 64, F (OF PASSENI) and 1952, *Taf.* 83, A (OF SEVERI). *Early-Flavian.*

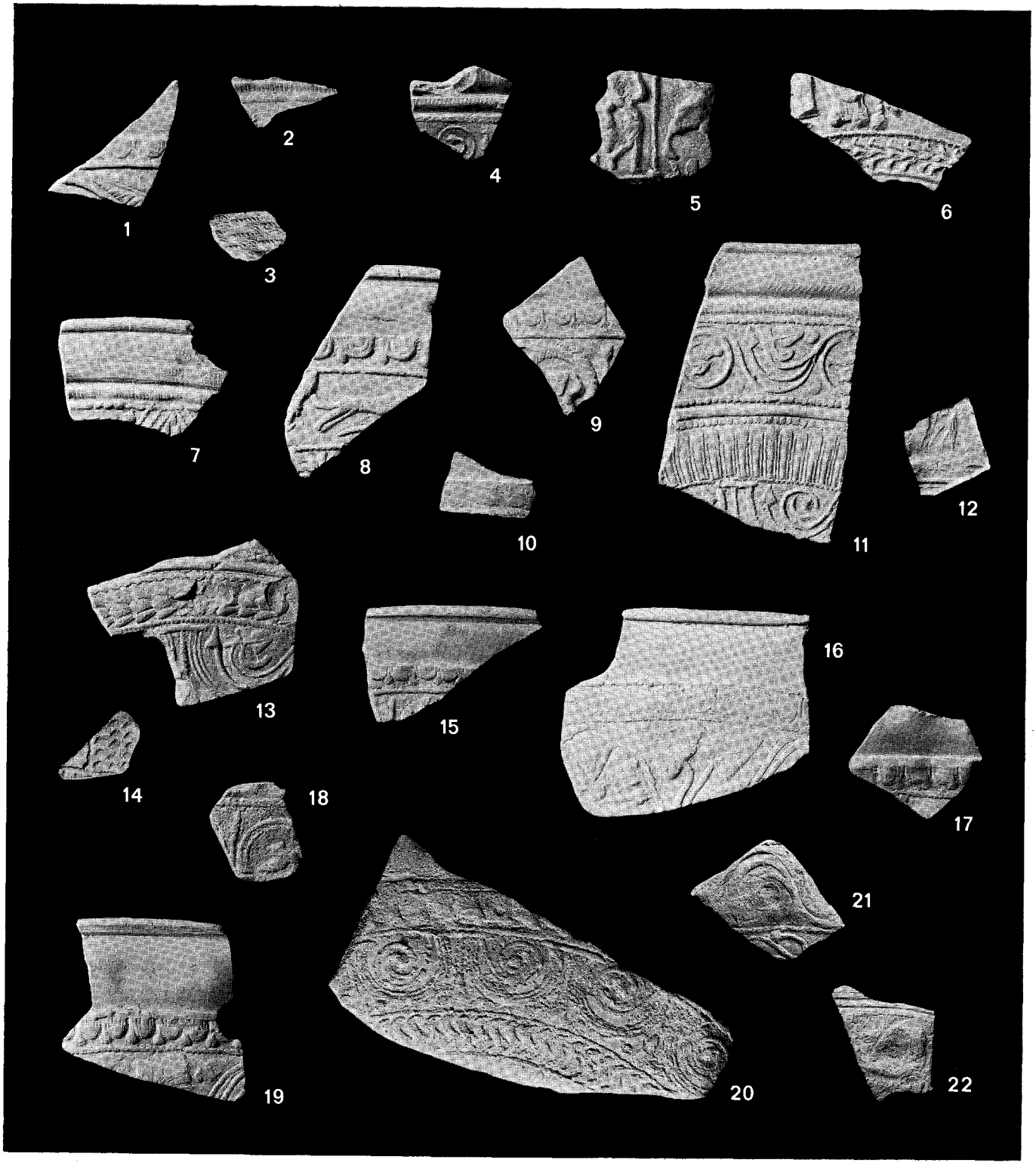
31 See paragraphs 6.2.3 and 6.2.6.

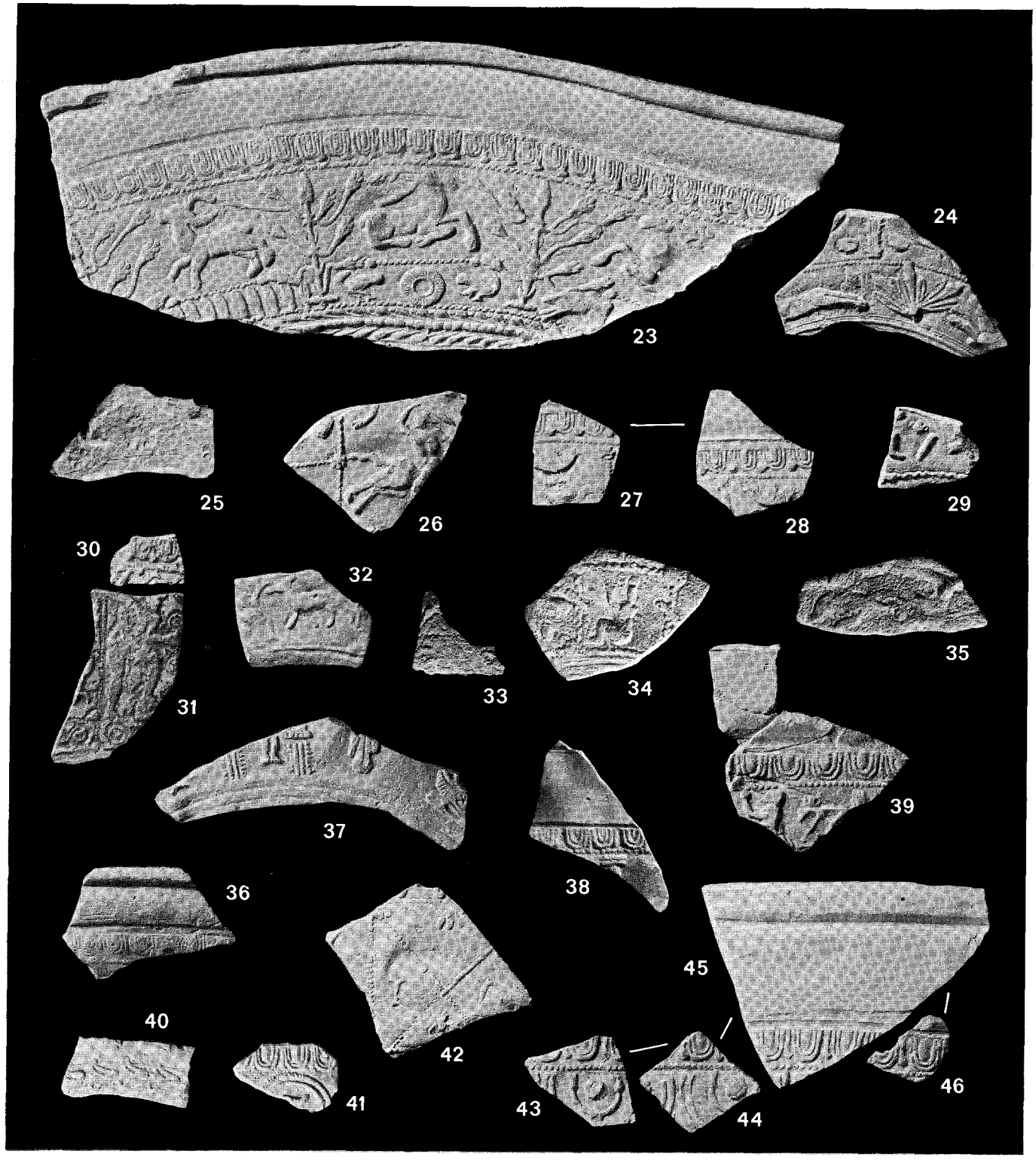
32 It is, however, questionable whether this late sigillata-like ware should be called 'imitation'. For an interesting approach to this problem see Rigoir/Rigoir/Vertet 1973.

33 I am greatly indebted to a number of people for their assistance and advice in compiling this list. I thank C.G.A. Morren (Apeldoorn), L. Bakker (Bonn), and my colleagues from the ROB, J.A. Trimpe Burger and W.J. van Tent, for their generous help.

34 Oswald 1931.

35 In a number of different publications. As far as possible, his die-numbers are mentioned.


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◁ Fig. 27 Terra sigillata from southern (23) and central (24–36) Gaul, Blickweiler/Eschweilerhof (37–38), and La Madeleine (39–46). Scale 1:2.

8 Site 117. Drag. 37. Ovolo like Hermet 1934, Pl. 35bis, 31. Leaf-like ornament Knorr 1919, *Textb.* 7 and Karnitsch 1959, *Taf.* 15,1 (Mercato-style). Dog Oswald 1925, see Karnitsch 1959, *Taf.* 14,1. *Flavian*

9 Site 117. Drag. 37. Ovolo Knorr 1919, *Taf.* 57, 19. Satyr Oswald 597 (cf. no. 5) under arch Karnitsch 1959, *Taf.* 16, 1 and 2, with ornament Knorr 1919, *Textb.* 12. Compare Müller 1968, *Taf.* 4, 65 and esp. Vanderhoeven 1978, *Taf.* 91, 775 which could be from the same mould. *Style of Mercato. Flavian.*

10 Site 117. Drag. 37. Small fragment with unidentifiable ovolo. *Flavian*

11 Site 126. Drag. 29. Upper frieze with compound-stalked scroll. Cordate leaf and stipule Knorr 1952, *Taf.* 36, D (Masclus), *Taf.* 44, B (Murranus), *Taf.* 50, A (Pontus) and Knorr 1919, *Taf.* 64, F, G (Passenus). Cuneiform leaf a.o. Hermet 1934, Pl. 73, 3 and Haalebos 1977, *Taf.* 32, 24. Small circles a.o. Knorr 1919, *Taf.* 22, C with birds Oswald 2249 and 2294. Oswald 2249 also on Knorr 1919, *Taf.* 22, C, Oswald 2294 possibly: compare Knorr 1952, *Taf.* 33 (Modestus) for better parallel. Rosette with one-sided wear: Knorr 1919, *Taf.* 63, B, C and 1952, *Taf.* 48, A (all Passenus). Middle frieze with shortened elongated tongues Mary 1967 *Taf.* 13, 22; 15, 1 and 20; 16, 15, Knorr 1952, *Taf.* 39, C and 40, B. Lower frieze with festoons Hermet Pl. 57, 2 and 5, Knorr 1919, *Taf.* 64, F and 1952, *Taf.* 36, C, connected by astralagus Knorr 1919, *Taf.* 64, G. Small rosette Knorr 1919, *Taf.* 64, G and 1952, *Taf.* 48, B (both Passenus). *Probably product of Passenus. Nero - Early-Vespasian.*

12 Site 126. Drag. 30. Diana Oswald 104. Compare Karnitsch 1959, *Taf.* 11, 2 and Vermeulen 1932, Pl. 18, 8. *Flavian.*

13 Site 135. Drag. 29. Middle frieze with triple leaves Knorr 1919, *Textb.* 12 (Passenus, Rufinus, and Secundus), compare Knorr 1912, *Taf.* 9, 4. Lower frieze cuneiform leaf and stipule Knorr 1952, *Taf.* 39, C, D, E and 48, C (Meddilus and Passenus), small leaf Knorr 1919, *Taf.* 62, 66 (Passenus), Knorr 1952, *Taf.* 39, E (Meddilus), festoon Knorr 1952, *Taf.* 39, D (Meddilus). *Vespasian.*

14 Site 194. Drag. 37. Metope decoration with arrowheads. *Flavian.*

15 Site 234. Drag. 37. Ovolo Knorr 1919, *Taf.* 57, 19 (cf. no. 9). 'Flabellum' Knorr 1919, *Taf.* 57, 11. See Knorr 1919, *Taf.* 57, H and Karnitsch 1959, *Taf.* 14, 1, 2 and 15, 2. *Style of Mercato. Flavian.*

16 Site 250. Drag. 37. Ovolo Knorr 1919, *Taf.* 57, 19 (cf. no. 9 and 15). Goose Oswald 2286. Compound-stalked scroll with leaves Knorr 1919, *Textb.* 12 and 20. Bush from triple leaves Knorr 1912, *Taf.* 21, 9, Müller 1968, *Taf.* 2, 24 and Karnitsch 1959, *Taf.* 14, 1 and 3. *Style of Mercato. Flavian.*

17 Site 250. Drag. 37. Ovolo probably Karnitsch 1959, *Taf.* 17, 3. Mercator-group. *Flavian.*

18 Site 295. Drag. 29. Arch Knorr 1919, *Taf.* 94, B (Nero). The cobs between the arches point to a relatively late date. *Vespasian.*

19 Site 355. Drag. 37. Ovolo Knorr 1919, *Taf.* 65, 9 (PAVLLI). Geese Knorr 1919, *Taf.* 65, 13. Large leaf Hermet Pl. 8, 20, Knorr 1919, *Taf.* 99, C. Similar decoration Knorr 1919, *Taf.* 99, A. *Vespasian-Titus*

20 Site 356. Drag. 37. Ovolo probably Knorr 1919, *Taf.* 57, 19 (cf. no. 9, 15, 16). Frieze of garlands a.o. Haalebos 1977, *Taf.* 34, 68 and Vanderhoeven 1976, *Taf.* 68, 536. For hanging ornament see also Ulbert 1959, *Taf.* 40, 3. Basal wreath of S-shaped ornaments. *Mercator-group. Flavian.*

21 Site 356. Drag. 37. Compound-stalked scroll with 10-point rosettes and heart-shaped leaves Vanderhoeven 1976, *Taf.* 44, 326 (Niger). *Nero-Vespasian.*

22 Site 356. Déch. 67. Pig Knorr 1919, *Taf.* 70, 6 (Sasmonos) and 73, 28 (Secundus). *Vespasian.*

23 Site 385. Drag. 37. Ovolo Karnitsch 1959, *Taf.* 12, 2 (M. CRESTIO). Deer walking to left Oswald 1748, Hermet 1934, Pl. 27, 8. Deer lying to left Oswald 1746 = Hermet 1934, Pl. 27, 2. Deer lying to right Oswald 1700 = Hermet 1934, Pl. 27, 1. Compare Knorr 1919, *Taf.* 74, C (Secundus). Tree c. Karnitsch 1959, *Taf.* 10, 8, with flowers Knorr 1919, *Taf.* 35, 64 (Germanus). Pigeons Oswald 2248 and 2293, compare Karnitsch 1957, *Taf.* 7, 1 (Germanus-style). Palissade Hermet 1934, Pl. 81, 13. Basal wreath of pomegranates as on Vanderhoeven 1978, *Taf.* 87, 724–726. *Successors Germanus-style. Flavian.*

Central Gaulish terra sigillata

24 Site 48. Drag. 37. Double frieze style. Column on upper frieze probably caryatid Oswald 1199A. Lower frieze with looped leaves and point central ornament in group of 3 Stanfield/Simpson 1958, fig. 22, 7. Animal (bear?) on the left Oswald 1616. Animal (panther?) on the right Stanfield/Simpson Pl. 82, 6 (Sacer). *Probably product of Sacer. IIb.*

25 Site 59. Drag. 37. Rosette Rogers 1974, C194 and unidentified acanthus-leaf under animal jumping to the left(?).

26 Site 117. Drag. 37. Horseman riding to the right Oswald 247, Stanfield/Simpson p.xxxvii. Apparently Oswald 246 is also possible: compare Stanfield/Simpson Pl. 34, 411 (Ranto style) to Pl. 40, 469 (Ioenalis style). The S-shaped ornament is also on Pl. 40, 469, but wavy lines are the only borders used by Ranto, while they are uncommon for Ioenalis: for wavy lines with rosette-binding see Pl. 34, 402, 408, 409, 411 (Ranto style) and Pl. 41, 485 (Ioenalis style). *Ranto or Ioenalis. IIa.*

27 Site 117. Drag. 37. Ovolo Rogers 1974, B 47 (Criciro). Acanthus leaf Stanfield/Simpson fig. 23, 10 with astralagus fig. 23, 2, compare Pl. 86, 13 (Attianus). Criciro and Attianus are associated. *IIb-c.*

28 Site 117. Drag. 37. Probably from the same mould as no. 27. *IIb-c.*


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◁ Fig. 28 Terra sigillata from La Madeleine (47–51), Sinzig? (52), and the Argonne (53–68). Scale 1:2.

29 Site 224. Drag. 37. Small triple leaf Rogers 1974, G112, Stanfield/Simpson fig. 15, 3 and astralagus. See also Pl. 66, 20, 22 and esp. Pl. 65, 8 (G.I.VIBIVS/GELENVS). The bold wavy line Rogers 1974, A27 is also characteristic. *IIb*.

30 Site 233. Drag. 37. Ovolo Stanfield/Simpson fig. 42, 4 = Rogers 1974, B 264 (Cettus/Small S potter) and beaded border. Sherd joins with no. 31.

31 Site 233. Drag. 37. Jupiter Oswald 13, Stanfield/Simpson Pl. 154, 14, 18 (Pugnus; also used by Albucius, Sacer, Cinnamus and the small S potter). Small S ornament Stanfield/Simpson fig. 42, 1. *Small S potter. IIB.*

32 Site 250. Drag. 37. Surface severely worn. Deer walking to left Oswald 1763 which is characteristic for Ioenalis: Stanfield/Simpson Pl. 35, 412. Tree-trunk Rogers 1974, U 281. *IIa.*

33 Site 356. Drag. 37. Basal wreath composed of small ram's horns like Stanfield/Simpson Pl. 3, 21 (Potter X-2) or Pl. 23, 287, 288 (Potter of the rosette), bordered by a wavy line. *IIa.*

34 Site 356. Drag. 37. Reaching nude figure Stanfield/Simpson Pl. 65, 1, 13 (not in Oswald) (G.I.VIBIVS). Minerva Oswald 126 A, Stanfield/Simpson Pl. 65, 1. This fragment is probably from the same mould as Stanfield/Simpson Pl. 65, 1. G.I.VIBIVS/GELENVS. *IIb.*

35 Site 356. Drag. 37. Small bear running to left Oswald 1627 (Attianus, Advocisus, Paullus, Small S potter, Cinnamus) under panther running to left Oswald 1540 (Butrio, Advocisus, Cinnamus). See Karnitsch 1959, *Taf.* 79, 5 (Cinnamus) and esp. Stanfield/Simpson Pl. 113, 16 (ADVOCISI). *IIB.*

36 Site 356. Drag. 37. Ovolo related to the small-bowl ovolo of Cinnamus Stanfield/Simpson fig. 47, 2. Because of the wavy line border probably a product of his supposed predecessor (Stanfield/Simpson p. 152 and fig. 19, 3). *Potter X-7. IIb (?).*

Terra sigillata from Blickweiler and Eschweilerhof

37 Site 59. Drag. 37. Ornament Knorr/Sprater 1927, *Taf.* 81, 68 under columns like *Taf.* 59, 6, flanking Venus *Taf.* 72, 12. To the right caryatid-like figure *Taf.* 77, 7 and basket or vase *Taf.* 81, 75. Compare *Taf.* 59, 6 and *Taf.* 70, 4. *Style of the Avitus-group.*

38 Site 126. Drag. 37. Ovolo Knorr/Sprater 1927, *Taf.* 82, 32, bordered by square bead line *Taf.* 82, 39. Double bar ornament *Taf.* 81, 69 over palm-leave *Taf.* 80, 35. See Holwerda 1923, *Afb.* 78, 17. *Style of the Avitus-group.*

Terra sigillata from La Madeleine

39 Site 48. Drag. 37. Ovolo Ricken 1934, *Taf.* 7, A. Satyr or faun Oswald 618, Ricken *Taf.* 8, 12 and the curious ornament Fölzer 1913, *Taf.* 25, 115. See esp. Fölzer 1913, *Taf.* 2, 33. *Ware with ovolo A.*

40 Site 117. Drag. 37. Square ornament with X decoration Holwerda 1923, *Afb.* 74, 25. Basal wreath of calyces Holwerda 1923, *Afb.* 74, 2, 4, 9, 12, 13, 18–20, 23–25 (C.C. SACRI), Haalebos 1977, *Taf.* 41, 200. (C.C. SACRI). *Sacer.*

41 Site 117. Drag. 37. Ovolo Ricken J. (Ricken 1934, *Taf.* 10, 12). Small double circle Ricken 1934, *Taf.* 9, 9 and Fölzer 1913, *Taf.* 2, 8 and 17. *Ware with ovolo J.*

42 Site 117. Drag. 37. Triple leaf Ricken 1934, *Taf.* 7, 13 and calyx *Taf.* 7, 12 in net-decoration of beaded lines. For decoration see Holwerda 1923, *Afb.* 72, 5, Müller 1968, *Taf.* 10, 243 and esp. Schönberger/Simon 1966, *Taf.* 7, 228, which may have a comparable ovolo (Ricken, *Taf.* 7, 10?). *Style of Virtuus.*

43 Site 126. Drag. 37. Ovolo Ricken 1934, K1. Rosette probably Ricken, *Taf.* 7, 3 or 4, in ornament Ricken, *Taf.* 7, 51, in double half-circle Ricken, *Taf.* 8, 11. Compare Haalebos 1977, *Taf.* 40, 195 for similar decoration. No. 44 is from the same bowl. *Ware with ovolo K1.*

44 Site 126. Drag. 37. Ovolo Ricken 1934, K1. For other ornaments see no. 43 from the same bowl. *Ware with ovolo K1.*

45 Site 126. Drag. 37. Ovolo Ricken 1934, K. *Ware with ovolo K.*

46 Site 126. Drag. 37. Ovolo Ricken 1934, K. *Ware with ovolo K.*

47 Site 126. Drag. 37. Rampant goat Oswald 1854, Ricken 1934, *Taf.* 8, 6, Haalebos 1977, *Taf.* 58, 552 and Holwerda 1923, *Afb.* 75, 13. Figure also used elsewhere, see Ricken/Fisher 1963, T 125. Flower Fölzer 1913, *Taf.* 25, 70, also on mould *Taf.* 1, 27. A combination of both elements on Ricken 1934, *Taf.* 10, 13. The flower was also used in Lezoux (Rogers 1974, J 143) and Blickweiler (Knorr/Sprater 1927, *Taf.* 81, 6). *Ware with ovolo K3/L.*

48 Site 126. Drag. 37. Beaded lines with rosettes Ricken 1934, *Taf.* 7, 4. Rosette Ricken, *Taf.* 7, 1 and acanthus leaf Ricken, *Taf.* 7, 25 over unidentified ornament (fruit-basket ?). Feet of gladiator? Basal wreath of triple leaves Ricken, *Taf.* 7, 11. *Ware with ovolo B/C.*

49 Site 133. Drag. 37. Ovolo Ricken 1934, *Taf.* 7, C. Double half-circle Ricken, *Taf.* 9, 5 and 14, Holwerda 1923, *Afb.* 72, 36. Astralagus Ricken, *Taf.* 7, 8. *Ware with ovolo C.*

50 Site 135. Drag. 37. Ovolo Ricken 1934, *Taf.* 7, C? Arch Ricken, *Taf.* 7, 121 and *Taf.* 8, 1. *Ware with ovolo C?.*

51 Site 147. Drag. 37. Bird Ricken 1934, *Taf.* 7, 111 = Fölzer 1913, *Taf.* 25, 61. Small panther (or cat) Ricken, *Taf.* 7, 102. Leaf probably Ricken, *Taf.* 7, 52. Compare Ricken, *Taf.* 9, 13. *Ware with ovolo C.*

Terra sigillata from Sinzig (?)

52 Site 244. Drag. 37. Small leaf on twig Fischer 1969, *Taf.* R, O2, which was also used in other production centres.

Terra sigillata from the Argonne

53 Site 59. Drag. 37. Deer running to the left Ricken 1934, *Taf.* 13, 27. Half-circle Ricken, *Taf.* 13, 20 around grapes

Ricken, *Taf.* 13, 14. Wavy line with rosette Ricken, *Taf.* 13, 1, tree *Taf.* 13, 15 with leaves *Taf.* 13, 10 and grapes *Taf.* 13, 14. Basal wreath with rosettes *Taf.* 13, 3. See esp. Müller 1968, *Taf.* 17, 499 which is probably from the same mould. *Style of Gesatus and Tribunus* (see also no. 55).

54 Site 59. Drag. 37. Ovolo possibly Chenet/Gaudron 1955, X1 or X2. Hercules Oswald 748A under arch, see Holwerda 1923, *afb.* 82, 11, Morren 1957–8, *afb.* 16, 112, and Chenet/Gaudron, fig. 58, 6. Double leaf Fölzer 1913, 387 on 'feathered' column as on Ricken 1934, *Taf.* 13, 53. See also Schönberger/Simon 1966, *Taf.* 5, 190. Figure to the right dancer Ricken, *Taf.* 13, 42. *Ware with ovolo G.*

55 Site 59. Drag. 37. Tree Ricken 1934, *Taf.* 13, 15 with grapes Ricken, *Taf.* 13, 14. See Müller 1968, *Taf.* 17, 449. Probably from the same bowl as no. 53. *Style of Gesatus and Tribunus.*

56 Site 75. Drag. 37. Ovolo Ricken 1934, G = Fölzer 1913, 465. Pig Ricken, *Taf.* 12, 44. See Haalebos 1977, *Taf.* 66, 578. *Ware with ovolo G.*

57 Site 117. Drag. 37. Small horse(?) to the left Holwerda 1923, *afb.* 80, 12 = Hofmann 1968, 262. Large rosette similar to Hofmann 462 (Germanus?).

58 Site 117. Drag. 37. Deer running to the left Ricken 1934, *Taf.* 13, 27. Tree Ricken, *Taf.* 13, 15 with leaves *Taf.* 13, 6. Individual leaves Ricken, *Taf.* 13, 10. Compare Fölzer 1913, *Taf.* 7, 3. *Style of Gesatus and Tribunus.*

59 Site 126. Drag. 37. Bacchus Ricken 1934, *Taf.* 13, 39 with leaf Ricken *Taf.* 13, 16. Compare Schönberger/Simon 1966, *Taf.* 4, 173 and 177. *Ware with ovolo A/B.*

60 Site 126. Drag. 37. Ovolo Ricken 1934, *Taf.* 13, B. Half-circles Ricken, *Taf.* 13, 20, containing rosettes *Taf.* 13, 3, which also occur between the half-circles and at the bottom row. Two other rows formed of the triple leaves Ricken, *Taf.* 13, 7 and 8. *Ware with ovolo B.*

61 Site 135. Drag. 37. Ovolo Ricken 1934, G = Fölzer 1913, 465. See no. 56. *Ware with ovolo G.*

62 Site 151. Drag. 37. Ovolo c. Fölzer 1913, 456. Dog running to the left Fölzer 376. From the same mould as the unpublished sherd Aardenburg 1973-A05-607.

63 Site 212. Drag. 37. Ovolo Ricken 1934, C. Metope decoration with rosette-binding Ricken, *Taf.* 12, 1. *Ware with ovolo C.*

64 Site 233. Drag. 37. Ovolo as Fölzer 1913, 461. Bacchus Chenet/Gaudron 1955, fig. 58, B; 62, A; 63, 7 = Hofmann 1968, 40. *Germanus or Tocca?*

65 Site 234. Drag. 37. Ovolo Chenet/Gaudron 1955, T 3 and Holwerda 1923, *afb.* 80, 1–18.

66 Site 289. Drag. 37. Ovolo Ricken 1934, A or B. Surface severely worn. *Ware with ovolo A/B.*

67 Site 289. Drag. 37. Ovolo Chenet/Gaudron 1955, R 5. Compare Chenet/Gaudron, fig. 60, J. *Germanus or Tocca?*

68 Site 298. Drag. 37. Ovolo Ricken 1934, G = Fölzer 1913, 465. Medallion Fölzer 236 in plain circle: Ricken, *Taf.* 13, 53 and Holwerda 1923, *afb.* 82, 13. 'Feathered' column Ricken, *Taf.* 12, 58, *Taf.* 13, 53 and no. 54, with ornament Fölzer 432. See Fölzer *Taf.* 7, 4 (Amenus). Figure on the left Fölzer 357, in the middle Ricken, *Taf.* 12, 34, on the left possibly Ricken, *Taf.* 12, 28. Similar decoration on Chenet/Gaudron 1955, fig. 63, 20 and esp. Holwerda 1923, *afb.* 82, 6. *Ware with ovolo G.*

69 Site 298. Drag. 37. Ovolo as no. 62. Figure Ricken 1934, *Taf.* 12, 31. Lion to the right possibly Chenet/Gaudron 1955, fig. 25, 62. Bear (?) to the left. Basal wreath Holwerda 1923, *afb.* 81, 15, 18, 20.

70 Site 298. Drag. 37. Deer running to the left Fölzer 1913, 377. Triple leaves Ricken 1934, *Taf.* 13,7 and rosette *Taf.* 13, 1. Basal wreath of rosettes Ricken, *Taf.* 13, 3. Compare Müller 1968, *Taf.* 18, 501. *Ware with ovolo A/B.*

71 Site 315. Drag. 37. Dog jumping to the right Ricken 1934, *Taf.* 13, 30. Basal wreath of rosettes Ricken, *Taf.* 13, 1. *Ware with ovolo A/B.*

72 Site 315. Drag. 37. Ovolo Fölzer 1913, 456. Calyx Ricken 1934, *Taf.* 13, 12 and arch, see Chenet/Gaudron 1955, fig. 55, J with similar ovolo (Gesatus).

73 Site 315. Drag. 37. Hare running to the left Ricken 1934, *Taf.* 13, 28 and Chenet/Gaudron 1955, fig. 57, J, chased by dog Chenet/Gaudron, fig. 57, J, S, and fig. 58, L. Basal wreath with triple leaves Ricken, *Taf.* 13, 8 like Müller 1968, *Taf.* 17, 441 and Chenet/Gaudron, fig. 60, H. *Ware with ovolo A/B.*

74 Site 356. Drag. 37. Hercules Oswald 748A, under arch like Ricken 1934, *Taf.* 13, 54 or Fölzer 1913, 443 with astralagus. See no. 54. *Ware with ovolo G?.*

75 Site 441. Drag. 37. Surface very worn. Animal running to the left (Ricken 1934, *Taf.* 13, 28??). Basal wreath of rosettes Ricken, *Taf.* 13, 3. Compare no. 70 and Müller 1968, *Taf.* 17, 447, 449, and 455. *Ware with ovolo A/B.*

Terra sigillata from Trier

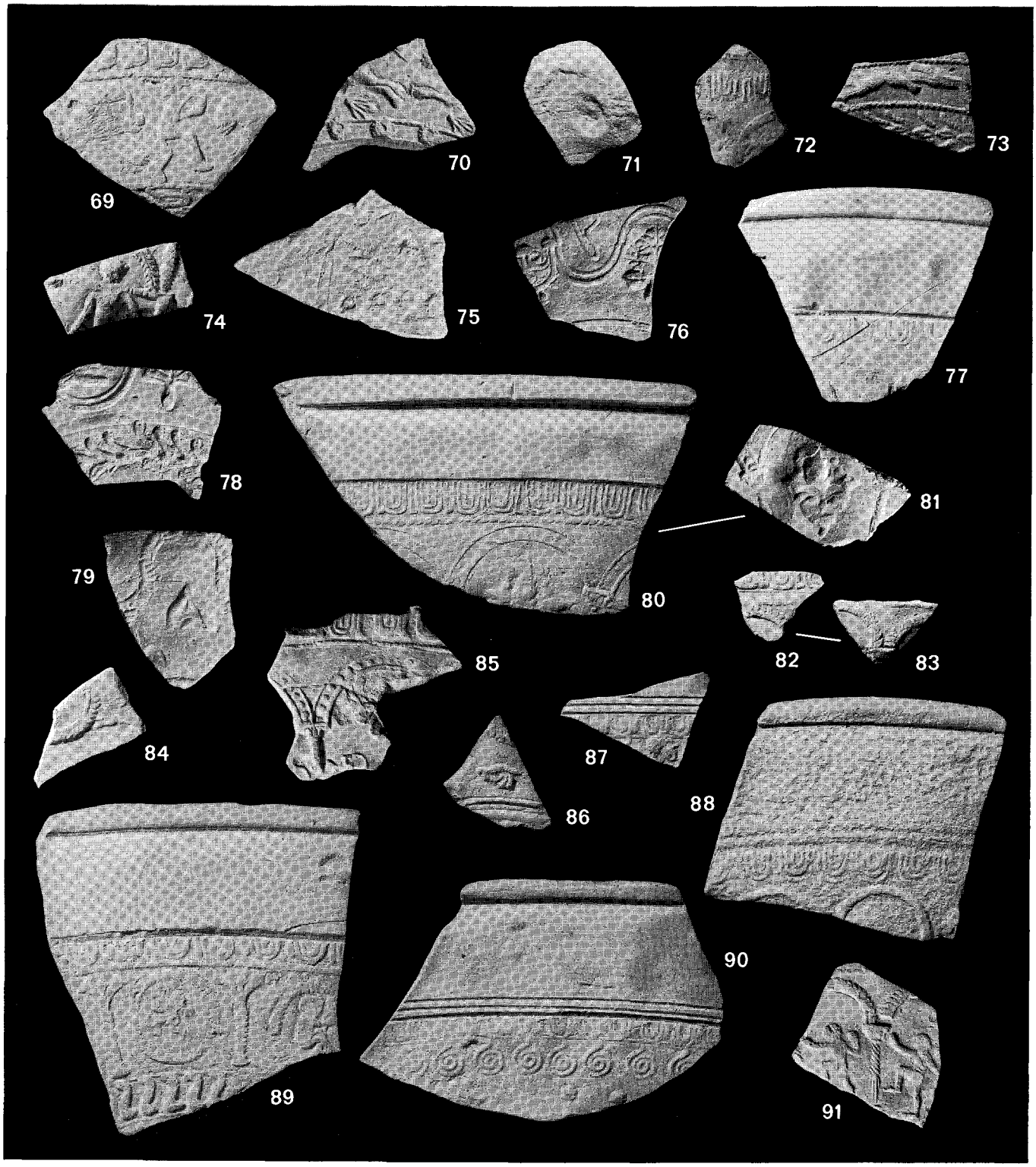
76 Site 48. Drag. 37. On the left pelta, Holwerda 1923, *afb.* 84, 10. On the right leaf Fölzer 1913, 748 and Kalee 1967, fig. 3, 3 and 4. For style compare Holwerda, *afb.* 84, 11. *Comitialis.*

77 Site 48. Drag. 37. Ovolo Fölzer 1913, 941. Palm-leaf c. Fölzer 762, see Holwerda 1923, *afb.* 84, 25. Lion walking to the left Fölzer 585. On the left possibly one-armed man Fölzer 516. Compare Holwerda, *afb.* 84, 25 and esp. Oelmann 1914, *Taf.* 8, 12 (COMITIAL F). *Comitialis.*

78 Site 48, Drag. 37. Leaf Kalee 1967, fig. 3, 1 and 3–5, also on Fischer 1973, *Abb.* 80, 5. Double circle Fölzer 1913, *Taf.* 9, 29 and Kalee 1972–3, *Abb.* 8, 26. Basal wreath as on Haalebos 1977, *Taf.* 49, 345. *Comitialis.*

79 Site 117. Drag. 37. Half-circles Fölzer 1913, 808 = Gard 1937, K 59. The very rare ornament in the middle also occurs on Holwerda 1923, *Afb.* 87, 46, where it is associated with the leaf Fölzer 755. *Censor-group.*

Fig. 29 Terra sigillata from the Argonne (69–75) and Trier (76–91). Scale 1:2. ▷


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80 Site 117. Drag. 37. Ovolo Fölzer 1913, 946. Arches Fölzer 800 over leaf Fölzer 755. Columns Fölzer 871. *Censor-group.* See no. 81.

81 Site 117. Drag. 37. Probably from the same bowl as no. 80. Arches Fölzer 1913, 800, supported by columns Fölzer 871 over leaf Fölzer 755. *Censor-group.*

82 Site 117. Drag. 37. Ovolo Fölzer 1913, 946. Arch Fölzer 808 = Gard 1937, K 59. See no. 79 and 83 (probably from the same bowl). *Censor-group.*

83 Site 117. Drag. 37. Arch Fölzer 1913, 808 = Gard 1937, K 59 on pillar Fölzer 871. Probably from the same bowl as no. 82. *Censor-group.*

84 Site 117. Drag. 37. Arch Fölzer 1913, 808 = Gard 1937, K 59, see nos. 79, 82–83. Rabbit running to the left Fölzer 677. See Haalebos 1977, *Taf.* 72, 663. *Censor-group.*

85 Site 117. Drag. 37.Ovolo Fölzer 1913, 946. Arches Fölzer 809 on column Fölzer 873. Stamp CẼNSO[R], compare Fölzer 967. *Censor-group.*

86 Site 117. Drag. 37. Circle Gard 1937, K 37 and lobster Fölzer 1913, 699. *Dubitatus-Dubitus?*

87 Site 117. Drag. 37. Ovolo Huld-Zetsche 1972, E 6b and togatus Huld-Zetsche M 57 = Fölzer 1913, 536. See Huld-Zetsche, *Taf.* 31, C 86, *Taf.* 33, C 96, and *Taf.* 36, C 114. *Werkstatt I, Stufe C.*

88 Site 117. Drag. 37. Ovolo Huld-Zetsche 1971, E 15 = Fölzer 1913, *Taf.* 21, 1. Arches Fölzer 1913, *Taf.* 23, 24. *Werkstatt II, Stufe D.*

89 Site 117. Drag. 37. Ovolo Huld-Zetsche 1971, E 16 = Fölzer 1913, 944. Amor Fölzer 545 and sitting figure Fölzer 543 in circles. Columns composed of the upper part of Fölzer 724 and the lower part of Fölzer 799. Basal wreath of ornaments Fölzer 895, see Müller 1968, *Taf.* 41, 1090. For decoration compare Fölzer, *Taf.* 13, 15. *Werkstatt II, Stufe E.*

90 Site 117. Drag. 37. Ovolo Huld-Zetsche 1972, E 6 with spirals Huld-Zetsche, O 34 = Fölzer 1913, 886. Column Huld-Zetsche O 6 = Fölzer 860. Quadriga and charioteer holding whip, see Huld-Zetsche, *Taf.* 38, C 126. *Werkstatt I, Stufe C.*

91 Site 117. Drag. 37. Arches Fölzer 1913, 808, see nos. 79, 82–84. On pillar Fölzer 871, see no. 83. Fighter Fölzer 525. *Censor-group.*

92 Site 126. Drag. 37. Ornament Fölzer 1913, 752 flanked by fighters Fölzer 525 and 524. Basal wreath Fölzer 904. See Fölzer, *Taf.* 21, 29 and Haalebos 1977, *Taf.* 68, 617. *Werkstatt II.*

93 Site 126. Drag. 37. Ovolo Huld-Zetsche 1971, E 15. Bear running to the right Fölzer 1913, 605 and pig to the left Fölzer 599. Compare Fölzer, *Taf.* 21, 27. *Werkstatt II.*

94 Site 126. Drag. 37. Ovolo Gard 1937, R 24. Fir-cone Gard P 53, Holwerda 1923, *afb.* 84, 10. Leaf Gard P 43 and Simon 1968, *Abb.* 3, 13 (MAIAAVS), also Kalee 1972–3, *Abb.* 8, 24 (COMITIALI), in circle. Double leaf Gard P 51. Basal wreath with ornament Fölzer 1913, 766? (see Kalee, *Abb.* 9, 46). *Comitialis.*

Fig. 30 Terra sigillata from Trier. Scale 1:2. ▷

95 Site 126. Drag. 37. Arches Fölzer 1913, 809 on column Fölzer 873, see no. 85. On the left probably leaf Fölzer 755, on the right circle Fölzer 830. Basal wreath of double leaves Fölzer 907. Compare Fölzer, *Taf.* 17, 5. *Censor-group.*

96 Site 126. Drag. 37. 'Herringbone' arch or circle, which is common for Amator. See Holwerda 1923, *afb.* 83, 9–15. *Amator?*

97 Site 126. Drag. 37. Ovolo Fölzer 1913, 941. Jumping deer to the left Fölzer 636 = Gard 1937, T 62. Circle Fölzer 830, see no. 95. Compare Haalebos 1977, *Taf.* 74, 703. *Afer-group.*

98 Site 126. Drag. 37. Ovolo Gard 1937, R 20 (also in Rheinzabern, Ricken/Fischer 1973, E 10). Dog to the left Gard T 91, Ricken/Fischer T 138a. Shell Gard 162 = Fölzer 1913, 711 (not in Rheinzabern). *Atillus?*

99 Site 135. Drag. 37. Two zones, bordered by beaded line Huld-Zetsche 1972, O 73. In upper zone deer Huld-Zetsche T 16 = Fölzer 1913, 635. In lower zone panther Huld-Zetsche T 7 = Fölzer 591. This sherd could be part of the link between the decorations Huld-Zetsche B 45 (= Fölzer, *Taf.* 14, 33) and B 46. *Werkstatt I, Stufe B.*

100 Site 151. Drag. 37. Ovolo Fölzer 1913, 946 = Gard 1937, R 2, bordered by beaded line. *Dexter, Censor and successors.*

101 Site 231. Drag. 37. Unidentified ovolo: the egg-element, composed of three lines like Gard 1937, R 26/27, is accompanied by a tongue with angular end. Ovolo bordered by a wavy line Gard R 46 = Fölzer 1913, 930 (Maiiaaus). The lion walking to the left Gard T 2 (Dubitus; not in Oswald or Fölzer), see Trimpe Burger 1977, *afb.* 8, 15. Small circle Gard K 3 (Maiiaaus). *Maiiaaus?*

102 Site 234. Drag. 37. Surface very worn. Deer jumping to the left Gard 1937, T 78, Holwerda 1923, *afb.* 83, 18. On the left tree Gard P 23. Lower border Gard R 43. Possibly from the same mould as Gard, *Taf.* 12, 5. *Dexter.*

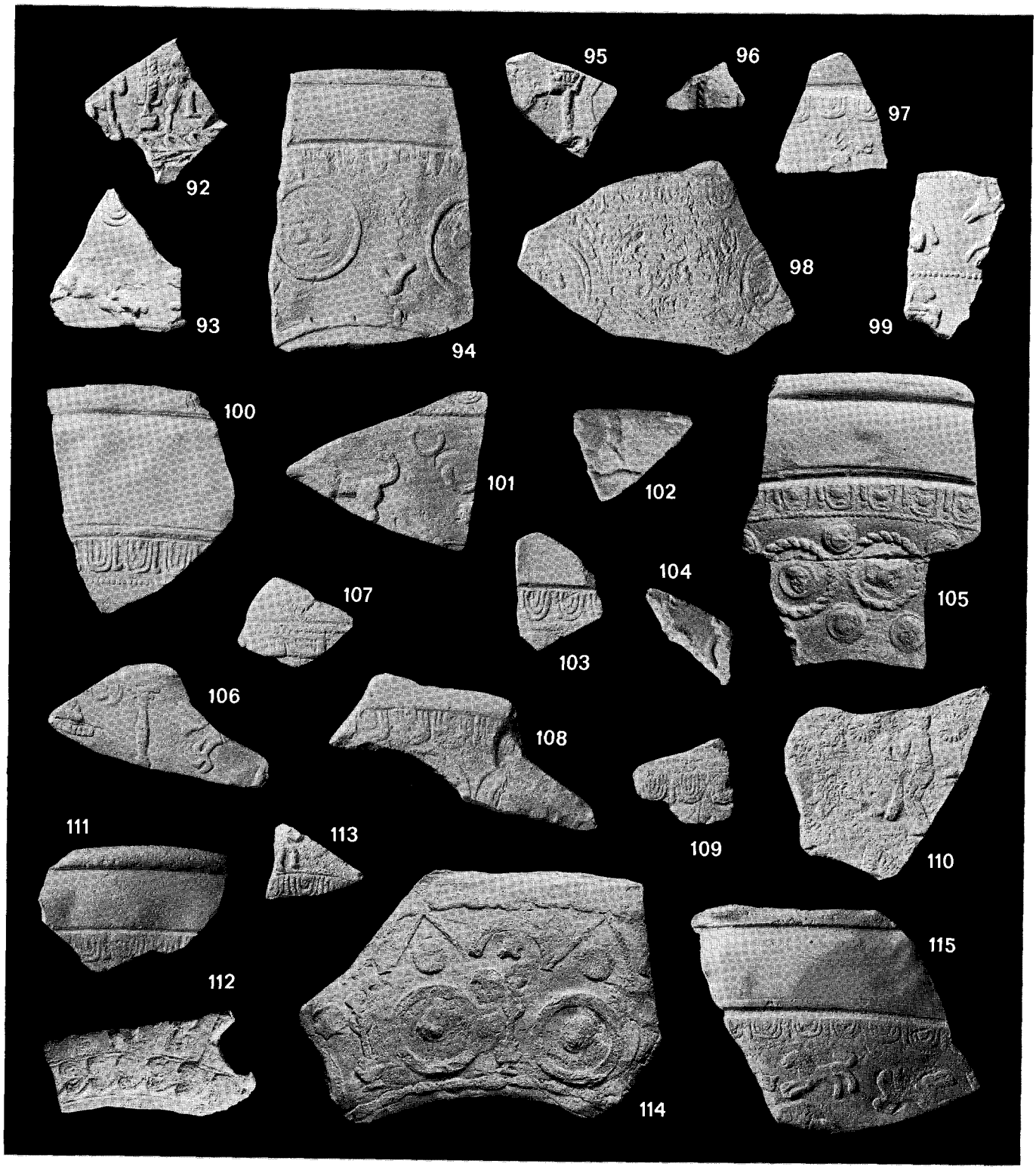
103 Site 248. Drag. 37. Ovolo Huld-Zetsche 1971, E 13 = Fölzer 1913, 956 and Gard 1937, R 28. *Werkstatt II, Stufen A, B, F. Maiiaaus-group.*

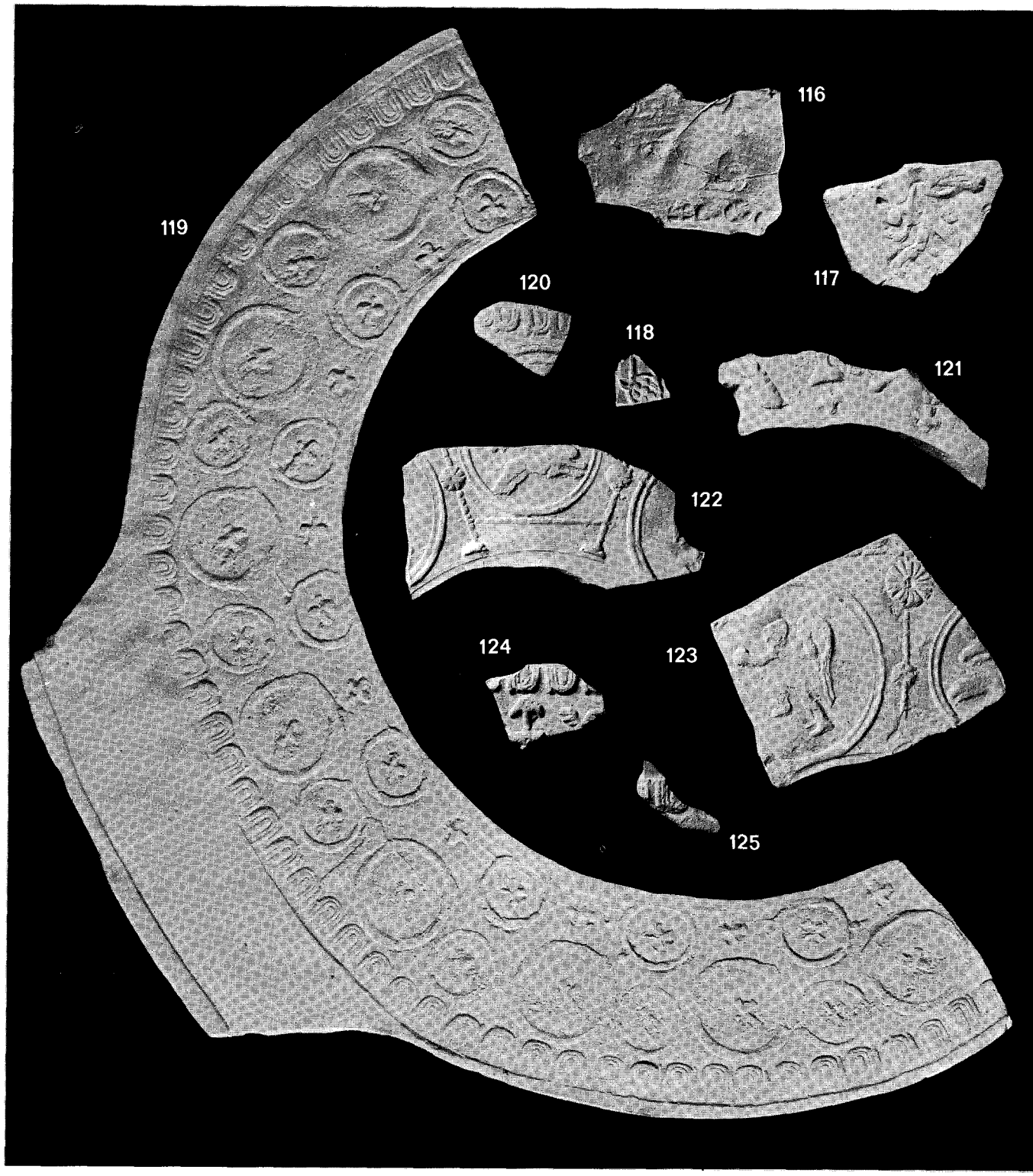
104 Site 250. Drag. 37. Philosopher Huld-Zetsche 1972, M 8a = Fölzer 1913, 470. Probably *Werkstatt II or Maiiaaus.*

105 Site 289. Drag. 37. Ovolo Fölzer 1913, 948 (Dexter). Medallions Gard 1937, M 2 in circle Fölzer 836. On the upper part medallions Gard M 1 = Fölzer 557, on the lower part medallions Gard M2. Compare Fölzer, *Taf.* 15, 14. *Dexter.*

106 Site 289. Drag. 37. Figure walking to the left Fölzer 1913, 560 = Gard 1937, M 47. Column Fölzer 875 = Gard V 17. Medallion Fölzer 557 = Gard M 1. Arch Fölzer 808 = Gard K 59. *Dexter/Censor.*

107 Site 289. Drag. 37. Ovolo Gard 1937, R 14. *Amator, Atillus, and Pusso.*


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◁ Fig. 31 Terra sigillata from Heiligenberg and Ittenweiler (116–118) and Rheinzabern (119–125). Scale 1:2.

108 Site 289. Drag. 37. Ovolo Fölzer 1913, 954 = Gard 1937, R 19. Concentric circles Gard K 13. *Dubitatus.*

109 Site 289. Drag. 37. Ovolo Fölzer 1913, 938 = Gard 1937, R 25. *Dubitatus-Dubitus group.*

110 Site 315. Drag. 37. Venus to the left Fölzer 1913, 475 and satyr to the right Fölzer 537. Rosette as on Fölzer, *Taf.* 21, 9 and leaf as Fölzer *Taf.* 20, 10. *Werkstatt II.*

111 Site 315. Drag. 37. Ovolo Fölzer 1913, 946 = Gard 1937, R 2. *Dexter, Censor, and successors.*

112 Site 356, Drag. 37. Standing leaf Fölzer 1913, 752. Metope-border Fölzer 921 and basal wreath Fölzer 916. Compare Fölzer, *Taf.* 22, 9. *Werkstatt II, Stufe E.*

113 Site 356. Drag. 37. Fighter Fölzer 1913, 506 and basal frieze with ovolo Fölzer 944. Compare Müller 1968, *Taf.* 40, 1060 and 1064 and esp. Fölzer, *Taf.* 22, 36. *Werkstatt II, Stufe F.*

114 Site 356. Drag. 37. Ovolo Fölzer 1913, 945. Upper frieze shell Fölzer 709 = Gard 1937, T 171 and ornament Fölzer 857 = Gard V 111 under arch Fölzer 808 = Gard K 59. Lower frieze with identical shells, vase with flower Fölzer 726 = Gard P 64 and medallion Fölzer 557 = Gard M1 in circle like Fölzer 833 = Gard K 10. Basal frieze of astralagi like Fölzer, 877. Compare Schönberger/Simon 1966. *Taf.* 14, 286. *Dexter, Censor.*

115 Site 356. Drag. 37. Ovolo Fölzer 1913, 941. Lion to the left Holwerda 1923, *afb.* 84, 4 and Kalee 1972–3, *Abb.* 8, 24 (COMITIALI), and 36, the right one turned upside down. Deer running to the left and looking back Oelmann 1914, *Taf.* 8, 10 (COMITIALI) and 14 (MAIIAA[VS]), Haalebos 1977, *Taf.* 50, 355 (COMITIALI) and *Taf.* 49, 343 with mould-graffito LVCIVS, Kalee, *Abb.* 8, 30 and 36, Holwerda *afb.* 84, 4 and Bloemers 1978a, *Taf.* 97, 311/5054. *Comitialis.*

Terra sigillata from Heiligenberg and Ittenweiler

116 Site 48. Drag. 37. Ornament Ricken/Fischer 1963, O 166 (Janu(ariu)s, Reginus), also known from Ittenweiler, Knorr 1907, *Taf.* 32, 1. Triple leaf Ricken/Fischer P 135, also used on the Janu-ware from Heiligenberg. Basal frieze of rosettes Forrer 1911, *Taf.* 32, 3 and 5, also on Simon 1977, *Abb.* 1, 3 (all 'F-*Meister*').

117 Site 117. Drag. 37. Flute-player Oswald 614, also known from Lezoux (Birrantus) and in a smaller form from Rheinzabern (Ricken/Fischer 1963, M 167). See Knorr 1907, *Taf.* 21,1 (Cebisus) and Forrer 1911, 200 (135), 202 (146) and 205 (170, 176), all Verecundus.

118 Site 234. Drag. 37. Rosette Forrer 1911, *Taf.* 28, 8–9 and Karnitsch 1959, *Taf.* 84, 4, 5, 9. *Probably Janu(ariu)s.*

Terra sigillata from Rheinzabern

119 Site 10. Drag. 37. Complete bowl with illegible stamp. Ovolo Ricken/Fischer 1963, E 23. Ornament Ricken/Fischer O 53, partly in circle K 9, partly in circle K 14. Compare Ricken 1948, *Taf.* 204, 19 F, 24, 25 F and 26 F. *Julius II-Julianus I.*

120 Site 23. Drag. 37. Surface very worn. Ovolo Ricken/Fischer 1963, E 18.

121 Site 48. Drag. 37. Ornament Ricken/Fischer 1963, O 53. Tree-like pillar Ricken/Fischer O 161 and pillar O 170. Compare a.o. Ricken 1948, *Taf.* 216, 4 F and *Taf.* 217, 6. *Julius II-Julianus I.*

122 Site 117. Drag. 37. Two lions jumping to the right Ricken/Fischer 1963, T 21a in circle K 19, compare Ricken 1948, *Taf.* 171, 2. Small rosette Ricken/Fischer O 70 and large double circles K 16. From the same bowl as no. 123. *Verecundus I.*

123 Site 117. Drag. 37. Two lions in circle as on no. 122. Large rosette Ricken/Fischer 1963, O 56 and small rosette O 70. Man with whip Ricken/Fischer M 191b in large circle K 16, compare Ricken 1948, *Taf.* 173, 15 (PEREGRINI) and Karnitsch 1959, *Taf.* 165, 6. *Verecundus I.*

124 Site 117. Drag. 37. Ovolo Ricken/Fischer 1963, E 26. Ornament with arrowhead O 214 and double leaf P 145. Compare a.o. Ricken 1948, *Taf.* 126, 19 and *Taf.* 135, 1. *Style of Belsus III, Atto, and Attilus.*

125 Site 126. Drag. 37. Small fragment with damaged ovolo, probably Ricken/Fischer 1963, E 23, E 25, or E 26.

126 Site 126. Drag. 37. See under no. 129. Sherd joins with no. 127.

127 Site 126. Drag. 37. See under no. 129. Sherd joins with no. 126.

128 Site 126. Drag. 37. See under no. 129. Sherd joins with no. 129.

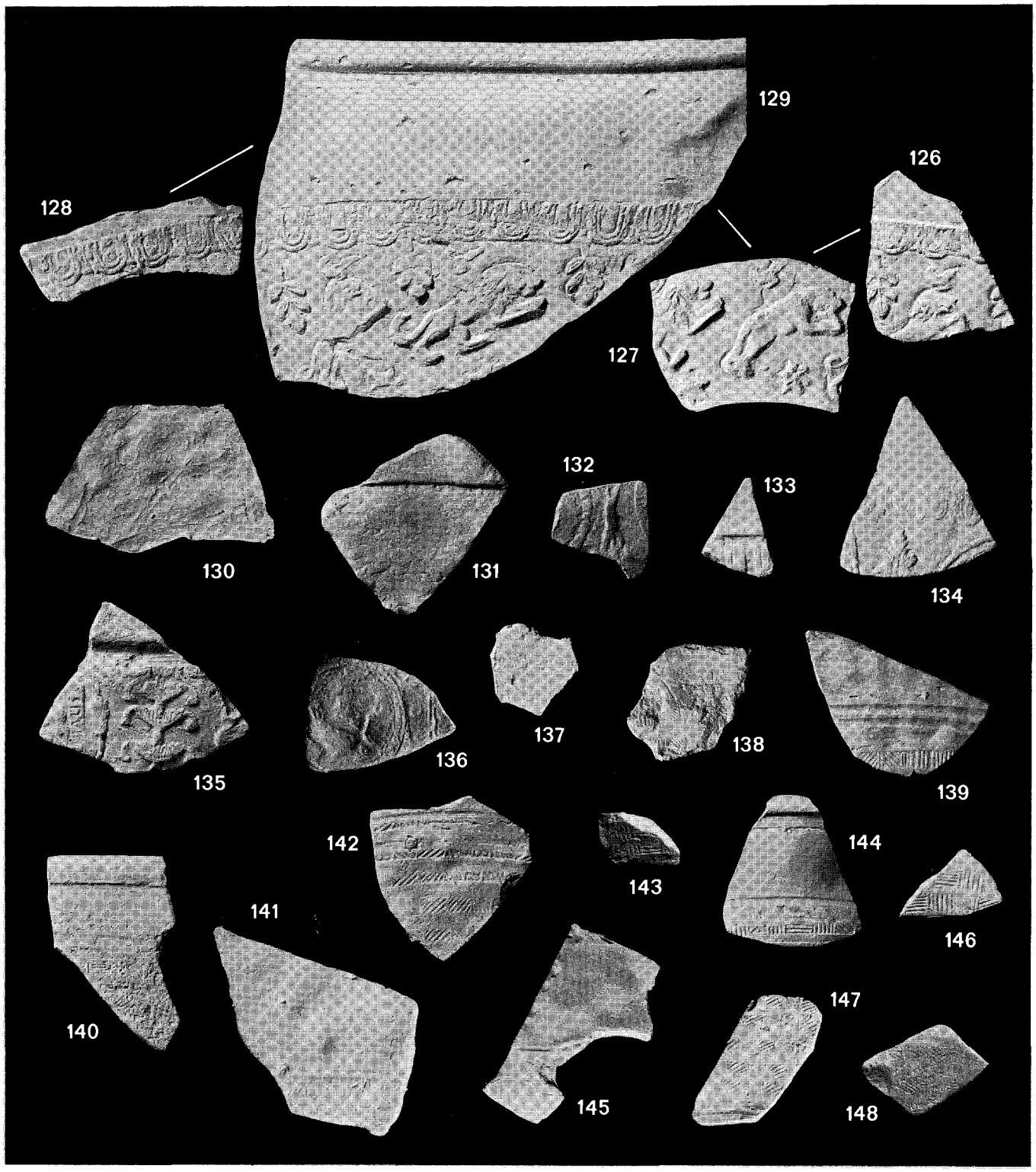
129 Site 126. Drag. 37. Large fragment of bowl to which nos. 126–128 also belong. Ovolo Ricken/Fischer 1963, E 7. Leaf Ricken/Fischer P 79, lion standing up T 20 and bird to the left looking back T 245b. Lion jumping to the right T 17 and bear jumping to the right (only on no. 127) T 59. At the bottom rosettes O 34b. For decoration compare Ricken 1948, *Taf.* 177 and Karnitsch 1959, *Taf.* 168, 6. *Helenius.*

130 Site 250. Drag. 37. Surface very worn. Triple arch, probably Ricken/Fischer 1963, O 133–136. Crossed beaded lines and in between small dog jumping to the left T 141 a–c. For style compare Ricken 1948, *Taf.* 186, 7F and 10 (Augustalis).

131 Site 289. Drag. 37. Surface very worn. Ovolo Ricken/Fischer 1963, E 60. Dog running to the left possibly Ricken/Fischer T 139.

132 Site 289. Drag. 37. Doubled ornament Ricken/Fischer 1963, P 116 b/c, between circles K 30. Compare Ricken 1948, *Taf.* 232, 13. *Style of Victor.*

133 Site 289. Drag. 37. Small fragment with ovolo probably Ricken/Fischer 1963, E 45.


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◁ Fig. 32 Terra sigillata from Rheinzabern (126–136) and late-Roman sigillata from the Argonne (137–148). Scale 1:2.

134 Site 315. Drag. 37. Ovolo Ricken/Fischer 1963, E 25/26. Leaf Ricken/Fischer P 75a, on the right double circle. Compare Ricken 1948, a.o. *Taf.* 119, 9 and 10 (ware with ovolo 25/26), *Taf.* 134, 5 (Atto) and *Taf.* 178, 9 (Attilus).

135 Site 356. Drag. 37. Sherd with stamp [C]ERIALIS F, Ricken 1948, *Taf.* 256, Cerialis b. Ovolo Ricken/Fischer 1963, E 44. On the left possibly goddess Ricken/Fischer M 31. Small pointed leaf Ricken/Fischer P 42. Pillar composed of calyces P 112. On the right ornament like Ricken, *Taf.* 54, 10. Compare Ricken, *Taf.* 54, 10, 12, 14, and 15. *Cerialis II.*

136 Site 377. Drag. 37. Sherd with stamp COM[ITIALIE] (retro), Ricken 1948, *Taf.* 256, Comitialis b. Sitting Apollo with lyre Ricken/Fischer 1963, M 72 in double circle K 20a. Compare Ricken, *Taf.* 104, 17 and *Taf.* 105, 12 F. *Comitialis VI.*

Late-Roman terra sigillata from the Argonne

137 Site 79. Chenet 320. Ovolo-decoration, Hübener 1968, Group 1. *IVA.*

138 Site 79. Chenet 320c. Chequered decoration, Hübener 1968, Group 4. Exact pattern indeterminable.

139 Site 126. Chenet 320. Chequered decoration, pattern Chenet 1941, no. 293.

140 Site 135. Chenet 320. Chequered decoration, pattern Unverzagt 1919, no. 81, which may belong to Hübener 1968, group 4, see p. 261.

141 Site 135. Chenet 320. Chequered decoration, new pattern, not in Unverzagt 1919 or Chenet 1941. Hübener 1968, group 2.

142 Site 150. Chenet 320. Chequered decoration, pattern Unverzagt 1919, no. 68, Hübener 1968, group 6.

143 Site 244. Chenet 320. Chequered decoration, Hübener 1968, group 3, possibly pattern Unverzagt 1919, no. 24.

144 Site 315. Chenet 320. Chequered decoration, pattern Unverzagt 1919, no. 40. Hübener 1968, group 5.

145 Site 315. Chenet 320. Chequered decoration, pattern Unverzagt 1919, no. 88.

146 Site 315. Chenet 320. Chequered decoration, pattern Unverzagt 1919, no. 29. Hübener 1968, group 4.

147 Site 315. Chenet 320. Chequered decoration, pattern Unverzagt 1919, no. 38. Hübener 1968, group 6.

148 Site 369. Chenet 320. Chequered decoration, pattern Chenet 1941, no. 257, Hübener 1968, group 8. Produced in Châtel-Chéhéry, *after Va.* The same decoration is present on a fragment from site 156 (see ch. 5). Is is also known from fragments found in Ferwerd and Bayum, see Boeles 1951, *afb.* 38, 4 and 5.

LIST OF STAMPS ON TERRA SIGILLATA (Fig. 33)

1 AMABILIS, Site 341, Drag. 27. Amabilis of La Madeleine. Amabilis used a great number of stamps which are, however, easily distinguishable from the Rheinzabern Amabilis stamps. Schönberger/Simon 1966, *Abb.* 1,9 is an identical stamp. Amabilis stamps were found in the ditch of the Saalburg-*Erdkastell* (Schönberger/Simon, p. 15), which means the potter must have been producing already before AD 125/6. *IIb.*

2 ANISATUS F, Site 316, Drag. 18/31. Anisatus of Heiligenberg and Pont-des-Rêmes. Identical stamps are known from De Meern (Jongkees/Isings 1963, *afb.* 12,1), Zwammerdam (Haalebos 1977, *Taf.* 22,14) and Ems (ORL B 4, *Abb.* 2,2). *Hadrian-Antonine.*

3 ATEI·XAN, Site 336, Ha 8. Stamp in tabula ansata-frame. Cn. Ateius Xanthus. See Oxé/Comfort 1968, 176. *Ia.*

4 BVCCI[VS·F], Site 353, Drag. 18/31. Buccius of Lezoux or La Madeleine. Compare similar stamps from Tongeren (De Schaetzen/Vanderhoeven 1964, Pl. 3, 22–25). Apparently the damaged V, which makes the stamp look like BACCI[(retro), is not common. *Traian-Antonine.*

5 CAL[AV]A F, Site 332, Drag. 31?. Calava, probably of Lezoux, possibly eastern Gaulish. Similar stamps from Neuss (Schönberger/Simon 1966, *Abb.* 1,17), Cannstatt (Goessler/Knorr 1921, *Taf.* IX, 36) and Tongeren (De Schaetzen/Vanderhoeven 1964, Pl. 3,36. An example from Straubing (Walke 1965, *Taf.* 40,102) seems to be identical except for the slight damage to the frame on the left. Oswalds dating (Vespasian-Trajan) is too early, see Goessler/Knorr 1921,55. *IIA.*

6 CELSINVSF, Site 357, Drag. 31/Lud. Sa. Celsinus of Ittenweiler. Also known from Faulquemont and Sinzig. Similar stamps are known from Arnsberg (ORL B 16, p. 24, no. 5) and Walldürn (ORL B 39, p. 17, no. 2). An identical stamp was found in Zugmantel (ORL B 8, *Taf.* 29,64). *Hadrian and a little later* (cf. Schönberger/Simon 1966, 17).

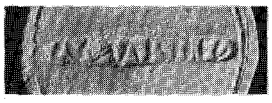
7 CILLVTIVSF, Site 59, plate. Cillutius of Rheinzabern. Identical stamps in Arentsburg (Holwerda 1923, *Afb.* 68/69, 42), Asciburgium (Vanderhoeven 1974, p. 13, 25) and Valkenburg, Per. V (Glasbergen 1948a, 226, no. 39). *Antonine.*

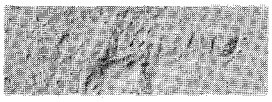
8 CILLVTIVSF, Site 306, Lud. Sa. Cillutius of Rheinzabern. Identical to no. 7.

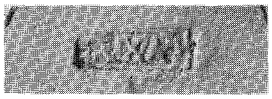
9 CIRRVS F, Site 151, Large plate. Cirrus of Faulquemont. Identical stamps from Faulquemont (Delort 1948, Pl. 1, 20) and Asciburgium (Vanderhoeven 1974, 14, no. 29). *c. AD 110–140.*

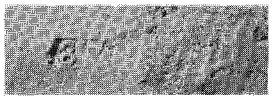
10 CLA, Site 126, Ha. 2?. Clarus was a legionary (?) potter working north of the Alps. Identical stamps known from Lyon, Köln, Xanten, and Vechten (Oxé/Comfort 1968, no. 443, b, g, h^1, and l). *Ia.*

11 OF·CRES, Site 357, Drag. 27. Crestio or Crestus of La Graufesenque. A possibly identical stamp is known from Richborough (Bushe-Fox 1949, 227, no. 21h), found in a pit dated to c. AD 90. Judging from the shape of the cup, our specimen must be earlier: *Nero-Vespasian.*

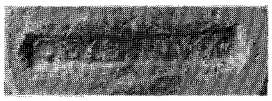

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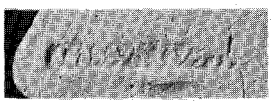

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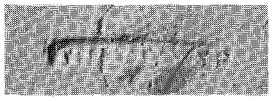

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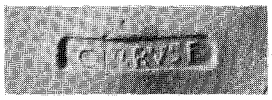

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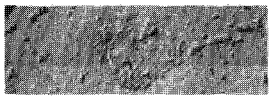

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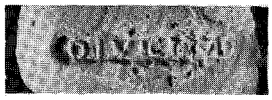

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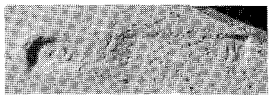

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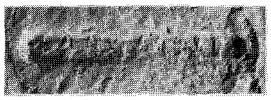

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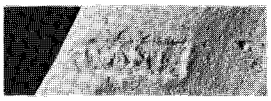

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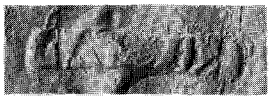

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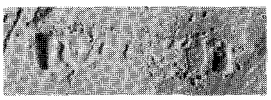

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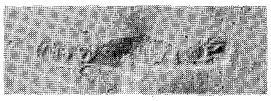

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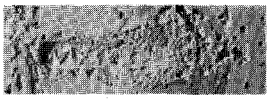

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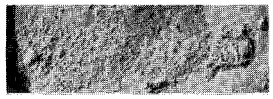

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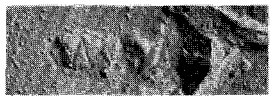

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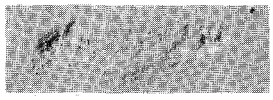

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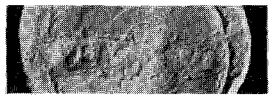

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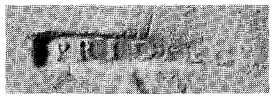

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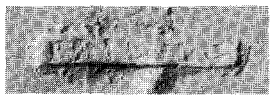

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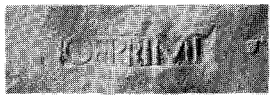

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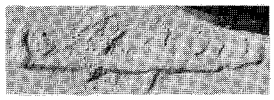

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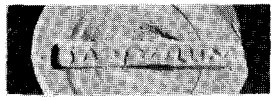

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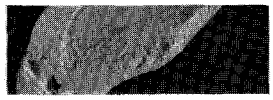

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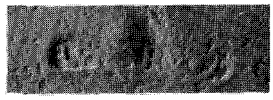

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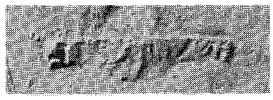

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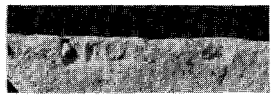

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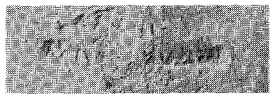

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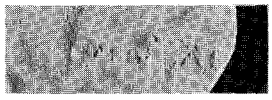

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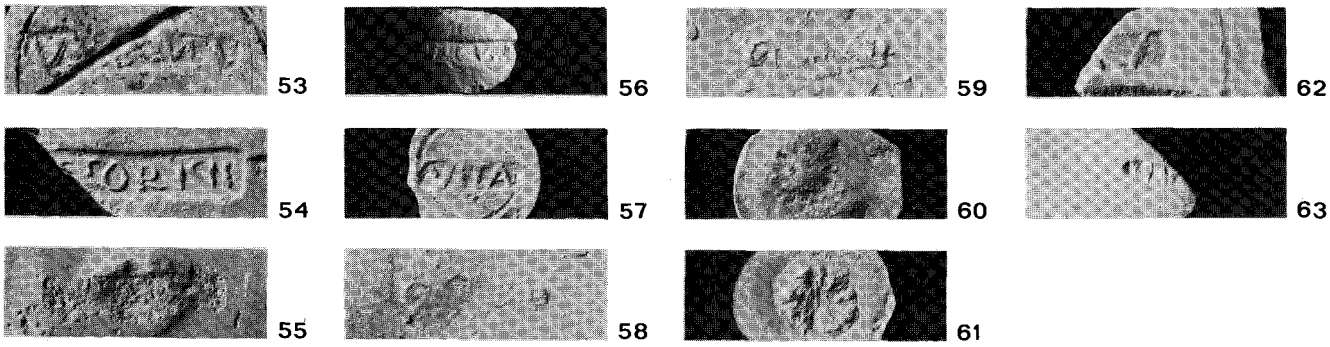


Fig. 33 Stamps on terra sigillata. Scale 1 : 1.

12 [OF C]RESTI, Site 355, Drag. 27. Crestus of La Graufesenque. Similar stamps from Tongeren (De Schaetzen/Vanderhoeven 1964, 64, no. 16, Vanderhoeven 1975, 222–223) Vanderhoeven 1975, 222 is possibly identical. *Flavian.*

13 [CRVM]MVS F, Site 117, Drag. 32. Crummus of Rheinzabern. An identical stamp was found in Niederbieber (Oelmann 1914, *Taf.* 9, 19). According to Oswald 1931, 99 the stamp should be dated to the *'Late Antonine' period.*

14 DIVICI·M·, Site 353, Drag. 33. Divicus of Lezoux. Oswald assigned Divicus to Lezoux, which is probably correct (cf. Dannell 1971, 305). Identical stamps were found in Zugmantel (ORL B 8, *Taf.* 29,107) and Cannstatt (Goessler/Knorr 1921, *Taf.* 9, 62–63). Period according to Oswald: Hadrian-Antonine, according to Dannell: *c. AD 125–160.*

15 DIVICI·M·, Site 353, Drag. 33. Divicus of Lezoux. Identical to no. 14.

16 DOCCALI, Site 336, Drag. 31. Doccalus of Lezoux and the Moselle district. Stamps from Doccalus occur in great quantities in Lezoux. They were also found in Rossum (Leemans 1842, no. 181), Zwammerdam (Haalebos 1977. p. 102, 102) and Tongeren (De Schaetzen/Vanderhoeven 1964, Pl. 5, 43, Pl. 6, 1–2 and Vanderhoeven 1975, 269). The stamp from Geldermalsen (Stuart 1968, *afb.* 6, 47) seems to be identical. Period according to Oswald: Hadrian. In Chesterholm a date of after AD 158 has been established (Haalebos 1977, 102). *Hadrian-Antonine.*

17 DONATI·M[, Site 336, Drag. 18/31. Donatus of Lezoux. A stamp from Donatus was recorded at Tongeren (De Schaetzen/Vanderhoeven 1964, Pl. 5, 47), an identical example at Faimingen (ORL B 66c, *Taf.* 7, 44). *Flavian-Antonine.*

18 DONTIO IIICI, Site 154. Drag. 27. Dontio of La Graufesenque (and Les Martres-de-Veyre?). The stamp is well known from a great number of sites. Apart from Vechten, where 21 examples were identified, it occurs a.o. in Neuss (Niessen a.o. 1904, 340, nos. 7940 and 8127), Heerlen (Vanvinckenroye 1967–8, Pl. 3, 3) and Nijmegen (Vermeulen 1932, Pl. 20, 30 and Stuart 1977, fig. 15,123). Definitely identical stamps (Hartley, die Dontio 6a) a.o. from Fishbourne (Dannell 1971, 306, 38) and Valkenburg-De Woerd (Bloemers/Sarfatij 1976, nos. 42–43). This stamp is dated by Dannell and Hartley to *c. AD. 70–100.* Shortly after AD 100, Dontio may have moved to Les Martres-de-Veyre.

19 GERMÂNI, Site 344, Drag 31. Germanus (?) of Lavoye (?). The reading of this stamp is uncertain. Oswald mentions Germanus-stamps in this form only for La Graufesenque (which in this case is impossible) and Westerndorf (which seems hardly likely). When the identification is correct, Oswald's dating to the Hadrianic period is probably too early. *IIB* seems to be a better estimate (cf. Hoffmann 1968).

20 [GRA]NIANI, Site 336, plate. Granianus, probably of Lezoux. Apart from the sites mentioned by Oswald, stamps from Granianus have been found in Colchester (Hull 1963, fig. 48,18) and Straubing (Walke 1965, *Taf.* 42,183). *Antonine.*

21 [OF I]VCVNDI, Site 126, Drag. 29. Iucundus of La Graufesenque. In view of the position of the stamp, it has to be amended to *Of* Iucundi. This stamp is discussed by Vegas 1966. There are probably two potters by this name, this stamp is from the second (Flavian) potter. Compare Knorr 1952, *Taf.* 31,E and several examples from Tongeren (Vanderhoeven 1975, 362–4) and Valkenburg-Woerd (Bloemers/Sarfatij 1976, 61–65). Vegas 1966, *Abb.* 1, c (from Pollentia) is probably identical. Stamps in this form are dated by Vegas to *AD 70–80.*

22 IVCVNDI, Site 331, Drag. 27. Iucundus of La Graufesenque. See no. 21. Because the indication 'Of' is lacking, this should be a stamp from the (Tiberian?-) Claudian potter. The form of the cup itself does not point to a very early date. Very similar (but smaller) stamps from Nijmegen, Stuart 1977, 136–7. *Claudian.*

23 LOLIMA (retro). Site 428, Drag. 31. Lollius of Lezoux. One example of a stamp from Lollius in Tongeren (De Schaetzen/Vanderhoeven 1964, *Taf.* 15, 21). *Vespasian-Hadrian.*

24 LVCIVSF, Site 353, Drag. 18/31. Lucius of Boucheporn. The potter also worked in Chémery-Faulquemont. There are two very similar stamps, one of them somewhat larger than our stamp, which was found in Faulquemont (Delort 1948, *Taf.* 1,33), compare Haalebos 1977, *Taf.* 23,143. The smaller stamp was used in Boucheporn (Lutz 1977, Pl. 12,19), compare Vanderhoeven 1974, 55 and 57. Lutz (1970, 322 and 1977, 40) as-

sumes that Lucius worked a short time in Faulquemont, after a long period in Boucheporn. Vanderhoeven 1974: c.AD 125–150, Hartley (in Bloemers/Sarfatij 1976, 77): c.AD 100–140. Lutz (1977, 196) concluded that Lucius must have been working in Boucheporn before the end of the first century.

25 MAIOR·FE, Site 336, Drag. 31. Maior of Blickweiler. Identical stamps from Arentsburg (Holwerda 1923, *afb.* 68/69, 104), Valkenburg (Glasbergen 1955, 251) and Zwammerdam (Haalebos 1977, *Taf.* 23, 155). Oswald: Traian–Hadrian. In Blickweiler (compare Knorr/Sprater 1927, 109, 13) it is dated to c. AD 130–145.

26 MARC[EL]LIVS, Site 353, Drag. 18/31. Marcellius from Lezoux. The stamp was originally in a somewhat better condition than is shown on fig. 33. Because of the length of this variant of a Marcellius-stamp, we must be dealing with Marcellius from Lezoux. The fragment recorded at Valkenburg-Woerd (Bloemers/Sarfatij 1976, 83) may be from an identical stamp. Examples from Fishbourne (Dannell 1971, 310,61), Stockstadt (ORL B 33, *Taf.* 19, 82) and Tongeren (Vanderhoeven 1975, 433) are somewhat smaller. The Marcellius-stamps from Lavoye (Chenet/Gaudron 1955, Fig. 51, Gc-d) are clearly very different. Oswald: Traian-Hadrian, Dannell: c. AD 130–160 and Hartley (in Bloemers/Sarfatij 1976): *c. AD 130–150.*

27 MA[(retro), Site 428, Drag. 18/31. Marcellus from Ittenweiler or Lavoye? In both potters' centres no retro-stamps of Marcellus have been recorded. It could well be therefore that we are dealing with a (retro!) abbreviation for *manv* (from an unknown potter).

28 [OF]MASO, Site 126, Hofh. 9. Maso (?) of La Graufesenque. It is not clear whether this stamp could be a variant from one of the OF MASC(V)L(I) stamps. Compare Hermet 1934, Pl. 111,101 and Stuart 1977, fig. 19,180. *Claudian.*

29 MECCO FEC, Site 353, Drag. 18/31. Mecco of Rheinzabern. The same stamp in Pfünz (ORL B 73, *Taf.* 8A, 90) and Arentsburg (Holwerda 1923, *afb.* 68/69, 116). *Hadrian-Antonine.*

30 MEDDI[C FI], Site 35, Drag. 31 (?). Burnt fragment of dish. Meddicus of Chémery-Faulquemont and Trier. There are two very similar variants, compare Bloemers/Sarfatij 1976, nos. 88–89 and 90 (Hartley 8a and 8b). Nos. 88–89 are identical to our stamp, which was also found in Tongeren (Vanderhoeven 1975, 451) and Saalburg (Schönberger/Hartley 1970, *Abb.* 2, 38). Oswald: Traian; Hartley: *c. AD 100–130.*

31 MII[ ]IIDOI, Site 10, Lud. Sa. Oswald mentions a Melledo from Lezoux (= Malledo?). *IIB/IIIA.*

32 NASS[OF], Site 336, Drag 31. Nasso of Sinzig and Lavoye (later Heddernheim?). Identical stamps in Niederberg (ORL B 2a, 15, no. 19) and Wiesbaden (Ritterling/Pallat 1898, *Taf.* 8,86). Very similar or identical stamps in Sinzig (Fischer 1969, *Taf.* 6 B, 13), Tongeren (De Schaetzen/Vanderhoeven 1964, Pl. 9,23), Valkenburg (Glasbergen 1955, 286), Zugmantel (ORL B 8, *Taf.* 30, 250), Butzbach (ORL B 14, 21, no. 16 and Müller 1962, *Taf.* 4, 22), Hesselbach (Baatz 1973, *Abb.* 41, 60), Inheiden (ORL B 17, *Taf.*, nr. 9), Neuss (Schönberger/Simon 1966, 23, 78) and Zwammerdam (Haalebos 1977, *Taf.* 24, 187).The main period of production is usually dated to IIb (cf. Schönberger/Simon 1966, 23, 78). Oswald: Hadrian-Antonine. *IIb.*

33 SILVIPATI, Site 336, Drag. 15/17. C. Silvius Patricius of La Graufesenque. According to Dickinson/Hartley/Pearce (1968, 144) these stamps belong to a potter with the *tria nomina* of citizenship, rather than a partnership of C. Silvius & Patricius. Haalebos (1977, 116, 256) and Stuart (1977, 36, 298) apparently prefer Oswald's interpretation of a partnership. (A stamp from this potter was also found at site 501.) A similar stamp from Baden Baden: Fritsch 1910, 95, 275. *Flavian.*

34 PERECRINV, Site 319, Drag. 18. Peregrinus of La Graufesenque. The stamp Hermet 1934, Pl. 112,122 is almost certainly identical. Oswald mentions several other findspots for the 'PEREGRIV' stamp. *Domitian.*

35 PLAC[ID]VS, Site 122, Drag. 27. Placidus of Blickweiler. The stamp from Blickweiler (Knorr/Sprater 1927, 110, 21a) is identical. Compare also similar (identical?) examples from Saalburg (Schönberger/Hartley 1970, *Abb.* 2, 52), Neuss (Schönberger/Simon 1966, 24, 88) and Asciburgium (Vanderhoeven 1974, 76). *c. AD 100–140.*

36 PRID·FEC, Site 389, Lud. Tq. Pridianus of La Madeleine. Identical stamps from Neuss (Schönberger/Simon 1966, *Abb.* 2, 92) and possibly Arentsburg (Holwerda 1923, *afb.* 68/69, 152a) and Tongeren (Vanderhoeven 1975, 550). Compare Haalebos 1977, *Taf.* 24, 208 (smaller) and Vanvinckenroye 1967–8, *Taf.* 5, 23 (larger). A Pridianus stamp was found in the ditch of the Saalburg-*Erdkastell*, which implies the potter was producing already before AD 125/6. *IIb.*

37 PRID·FEC, Site 389, Lud. Tq. Pridianus of La Madeleine. Identical to no. 36.

38 PR[, Site 336, Drag. 18/31. Probably Pridianus of La Madeleine. The stamp could be identical to one from Zwammerdam (Haalebos 1977, *Taf.* 24, 208). See under no. 36 for other variants and dating.

39 OFPRIMI, Site 385, Drag. 18. Primus of La Graufesenque. Identical stamps from Tongeren (Vanderhoeven 1975, 556) and Nijmegen (Stuart 1977, 231). Similar stamps in Verulamium (Hartley 1972, fig. 81, 21), Neuss (Mary 1967, *Taf.* 33, 30) and La Graufesenque (Hermet 1934, Pl. 112, 132b). According to Hartley this stamp is certainly pre-Flavian. Oswald: Claudius-Vespasian, Mary: AD 40–70, Hartley: *c. AD 50–65.*

40 Q[V]IИTVƨЯ, Site 126, Drag. 31. Quintus of Trier (?). Identical stamps in Niederberg (ORL B 2a, 15, no. 22) and Valkenburg-Woerd (Bloemers/Sarfatij 1976, 132) Hartley dates this stamp (die VII, Ia) to the *late second-early third century.*

41 SANVILLIM, Site 117, cup, probably Drag. 33. Sanuillus of Lezoux. Probably identical to a stamp from Heerlen (Vanvinckenroye 1976–8, Pl. 6, 23). *Traian-Antonine.*

42 SECVN[DINI], Site 126, Drag. 18/31. Secundinus of Lezoux. There are several Secundini known from Lezoux and Les Martres-de-Veyre. A very similar stamp was found at the Saalburg-*Erdkastell* (Schönberger/Hartley 1970, *Abb.* 3, 64 = Hartley, die Vh). Compare also two more different examples

from Cannstatt (ORL B 59, *Taf.* 4, 155–6) and one from Zwammerdam (Morren 1958, 73, 44). Oswald: Domitian-Trajan, Hartley (for die Vh only): *c. AD 125–145.*

43 [SEC]VNDI, Site 336, Drag. 27. Secundus of La Graufesenque. A probably identical stamp from Valkenburg (Glasbergen 1955, 329). Oswald: Claudius-Vespasian, Mary 1967: 25–60 AD. On account of typological characteristics of the cup, our stamp is certainly pre-Flavian: *Claudius-Nero.*

44 SECVND[I], Site 346, Drag. 24/25. Secundus of La Graufesenque. An identical stamp from Neuss (Mary 1967, *Taf.* 34, 26). Compare also Glasbergen 1948a, 120 for a similar stamp from Valkenburg. Oswald: Claudius-Vespasian, Mary 1967, AD 25–60. *Claudius-Nero*

45 ƎIARVS F, Site 428, Lud. Tq. Siarus of Eastern Gaul (?). Oswald mentions a stamp F SIARVS F on a cup Drag. 33 from Reims. The original stamp is somewhat clearer (especially the I and A) than the picture on fig. 33 suggests. *IIB.*

46 TA[, Site 336, Drag. 31. Burnt fragment. Possibly Tauricus from Lezoux.

47 TER[T]IVSF, Site 428, Drag. 31. Tertius of Lavoye or Rheinzabern. This stamp has originally been published (NKNOB 1972, 107–8) as TETRICVSF. A similar stamp from Rheingönheim (Ulbert 1969, *Taf.* 9, 68). Other stamps from Tertius a.o. in Arentsburg (Holwerda 1923, *afb.* 68/69, 189) and Tongeren (De Schaetzen/Vanderhoeven 1964, Pl. 13, 30). A probably identical stamp from the same site could not be traced. *Hadrian-Antonine.*

48 TOCCAF, Site 150, Drag. 33. Tocca of Lavoye, Avocourt and Blickweiler. Because most of the A and the F are indeterminable, the exact die is difficult to establish. It is almost certainly one of the Lavoye-stamps on Chenet/Gaudron 1955, fig. 52, possibly die Lc. The main production of Tocca is in *IIb* (Schönberger/Simon 1966, 26, 114–5).

49 TOCCAF, Site 387, Drag. 33. Tocca of Lavoye, Avocourt and Blickweiler. A stamp from Heerlen (Vanvinckenroye 1967–8, Pl. 7,10) could be identical. See no. 48.

50 VRBANVSF, Site 353, Lud. Sa. Urbanus of Heiligenberg and Trier. Identical stamps from Aardenburg (1977–B–481) and Niederbieber (Oelmann 1914, *Taf.* 9, 70–71). *Hadrian-Late Antonine.*

51 VTILIS, Site 126, Ha. 2. Die of the provincial potter Utilis. The letters like Oxé/Comfort 1968, 2499 g, i, with the exception of the S which ends in a line, underlining the entire name. The stamp is in a rectangular frame with indentation at both ends (not on Oxé/Comfort 1968, Pl. 8–10). *Ia.*

52 VENICAR[VS], Site 117, Drag. 31. Venicarus of Sinzig. The stamp is identical to Fischer 1969, *Abb.* 6B, 25. Three specimens were recorded in Asciburgium (Vanderhoeven 1974, 91–93). *IIb.*

53 VERECVNDV, Site 353, Drag. 33. Verecundus of Haute Yütz, Ittenweiler, and Rheinzabern. The Ittenweiler stamp recorded by Forrer 1911, 216, 240b is identical. Also stamps from Tongeren (Vanderhoeven 1975, 743), Arentsburg (Holwerda 1923, *afb.* 68/69, 199b) and Mook (Site 383, see also Bogaers/Morren 1954, 58, 8). Vanderhoeven: *c. AD 105–130*

54 [VI]CTORFII, Site 150, plate. Victor of Rheinzabern. An identical stamp from Zugmantel (Pferdehirt 1976, *Abb.* 14, 25). Similar stamps from other sites usually have a phallus on the right. *Hadrian-Late Antonine.*

55 VIDVCVSFE, Site 389, Drag. 18/31. Viducus of Sinzig. An identical stamp is Fischer 1969, *Abb.* 6B, 26. It also has the weak spot under CVS that is mentioned by Fischer (p. 46), which is also present on the stamp from Colchester (Hull 1963, fig. 48, 42; Hull's attribution of the stamp to Oswald's Viducus iii should have been Viducus ii). Another identical stamp was found in Asciburgium (Vanderhoeven 1974, 95). Viducus also worked in Lavoye (Chenet/Gaudron 1955, fig. 53, Me-o) and Martres-de-Veyre (Terrisse 1968, Pl. 54). Our stamp is the one mentioned by Oswald (p. 335) under 'Groesbeek, Nymwegen'. *IIb.*

56]OF L C VIR[ILI], Site 117, type ?. L. Cosius Virilis of La Graufesenque. It seems probable that this is a potter with the *tria nomina* of citizenship, not a partnership. In some instances (e.g. Oswald, p. 337?) the stamp may have been misread as OFIC VIRILI, especially because the L is sometimes damaged, compare Vanderhoeven 1975, 200. On our stamp the L is slightly, the R heavily damaged (like the R on Vanderhoeven 1975, 204). There are many variants of this stamp (Vanderhoeven 1975, 198–211, Bushe-Fox 1949, 131 B-E). Oswald: Flavian, Vanderhoeven: AD 80–120(!), Hartley (for die 6b): *AD 70–90.*

57 VITA, Site 316, Drag. 27. Vitalis of La Graufesenque. Hermet 1934, Pl. 113, 179f is identical, as well as a stamp from Fishbourne (Dannell 1971, 316, 96) and stamps from Nijmegen, Zwammerdam (Haalebos 1977, *Taf.* 25, 275), and Tongeren (Vanderhoeven 1975, 790). According to Dannell the stamp always appears on form Drag. 27. The example from Tongeren, however, has been identified as Drag. 33. Oswald: Claudius-Domitian, Dannell: *c. AD 70–100.*

58 O[F VI]TAL, Site 331, Drag. 18. Vitalis of La Graufesenque. An example from Neuss (Mary 1967, *Taf.* 35, 40) may be identical. Oswald: Claudius-Domitian, Mary: *AD 45–80.*

59 OF VITAI, Site 385, Drag. 18. Vitalis of La Graufesenque. An identical stamp is reproduced by Hoffmann 1971, Pl. 10, 183. 37. According to Dickinson/Hartley/Pearce 1968, 145, stamps of this potter are attested from *Flavian or Early-Traianic* contexts.

60 Rosette with 12 leaves, Site 385, Drag. 35.

61 Rosette with 9 leaves, Site 387, Drag. 33.

62 [IVV]I/, imitation writing. Site 336, Drag. 31? Identical stamps have been found in Zwammerdam (Haalebos 1977, *Taf.* 25, 293) and the Loowaard (site 194). The fragment is burnt.

63 IIII, imitation writing. Site 336, Drag. 31. The fragment is burnt.

6.2.2 *Colour-Coated Wares*

Colour-coated ware

Although the term 'varnished' is often used for colour-coated wares, it is not used here. For the sake of terminological clarity, all pottery covered entirely with a coloured slip is called 'colour-coated', a generally accepted and more suitable description.[36]

Colour-coated ware is almost as common as terra sigillata. It has been found in 131 of the 290 settlement sites with Roman ceramics (45%), a figure very close to the 48% found for t.s. The rate of recovery in this case may not be proportional from one site to another, but the lack of colour-coated sherds in a collection of a particular site virtually always implies that such a collection is small or very small. There are only a few sites with a reasonably large and varied quantity of Roman ceramics (such as 80, 95, 329) which do not include such sherds. It may be tentatively concluded that some colour-coated pottery was used in almost every settlement during the Middle-Roman period. Late-Roman colour-coated pottery is extremely rare, although small fragments, for example, of beakers Alzei type 15/16, may not have been recognized as such.

As far as colour-techniques[37] are concerned, white paste with a reddish to yellow-brown slip (technique a) and especially with a brown-olive to black slip (technique b) are by far the most common. Red paste with black slip (technique c) is also fairly frequent at a great number of sites. The so-called *Qualitätsware* (technique d), in which the slip has a metallic lustre, is rare. Outside Nijmegen, it has only been found on sites 38, 48, 59, 117, 118, 126, 214, 289, and 299. Although other 'techniques', usually different types of painted pottery, occur occasionally in cemeteries, they are virtually absent from settlements.

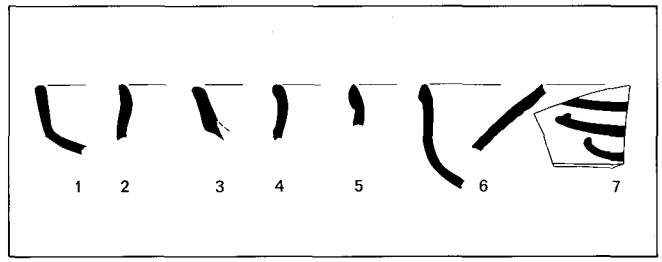


Fig. 34 Red-brown slip ware from Wijchen (1–2, 4–5, 7), Driel (3), and Qualburg (6).

Red-brown slip ware

Although intimately associated with terra sigillata, the red-brown slip ware is discussed here because technically the red-brown slip is a form of colour-coating and an imitation of 'true' terra sigillata.[38] The ware was first discussed by Loeschcke,[39] who called it *rotbraun gestrichene Ware*. It is, however, not painted but colour-coated and its production could be connected to the end of the terra sigillata deliveries of those of other colour-coated wares.[40]

The dating of this sort of pottery is somewhat obscure. It has not been found at Alzei, but it is still considered 'Roman' by Loeschcke and Hüssong/Cüppers. The latter propose a dating in the first half of the 5th century on account of the material from the *Umbau* phase of the Kaiserthermen and the latest ceramics from the Barbarathermen.[41]

The ware does, however, still appear in the second half of the 5th century, for example, in the cemetery at Haillot and some forms, generally described as 'Frankish', continue into the 6th.[42] These later wares are, however, typologically more restricted. They also have a somewhat different slip[43] and are more correctly referred to by Böhner and Pirling as 'red slip ware'. The red-brown

36 The same proposition was recently made by Stuart (1977, 40), who suggested the term *geverfd* should replace *gevernist* in the Dutch terminology. Unfortunately, this concept is dangerous, because it may be easily confused with 'painted', which Stuart refers to as *beschilderd*. Because colour-coated ware may also be painted (e.g. Niederbieber type 33b), this means that in the Dutch terminology *geverfde waar* may be *beschilderd*, which is semantically correct but terminologically rather awkward and potentially misleading.

37 The different 'techniques', which in this sense not only refer to different methods of applying colour but also to different combinations of colours of the slip and paste, are discussed by Brunsting 1937, 70–2.

38 See note 32.

39 Loeschcke 1921.

40 Cf. Hussong/Cüppers 1972, 71–2.

41 Hussong/Cüppers 1972, 72, 85.

42 Böhner 1958, 35–7; Pirling 1966, 128.

43 Pers. comm. Dr R. Pirling, Krefeld and Dr L. Bakker, Bonn. I am greatly indebted to Dr Bakker for his extensive comments on the red-brown slip ware from the river area, which have been incorporated here.

slip ware from the river area has a very thin and often chipped-off slip. The paste is light-brown to very pale-pink and tempered with very fine sand grains and occasional tiny pottery (tile?) grit. It conforms exactly to material from Boppard, Zülpich, and Köln-Deutz.[44] Sherds have been found on five sites: Ede-Veldhuizen (1), Driel-Oldenhof (123), Lienden-De Fleerde (283), Wijchen-Tienakker (315), and Qualburg (460). As far as could be established, it does not occur on the Nijmegen sites 403, 405, and 410.[45] The one example from Qualburg was published by Von Petrikovits;[46] but in view of its scarcity the few sherds of red-brown slip ware will all be discussed here. They comprise four different types, all represented in the material from the Barbarathermen.[47]

1 High-walled dish, Barbarathermen type 16. The dish is clearly related to Alzei type 11, a terra sigillata form which was itself only developed towards the end of the 4th century,[48] which probably implies an even later start of the production in red-brown slip ware. According to Hüssong/Cüppers,[49] the form with rounded rims (*Umbaukeramik* type 16) could be the earliest variety (fig. 34,1), while the rather pointed and slightly outward curving rims of Barbarathermen type 16 are a later development (fig. 34, 2–3). Types 129–130 from Krefeld-Gellep [50] are good parallels, as well as Rübenach type A3a, *Taf.* 19, 18 and 29, 2.[51] A total of six fragments has been recorded: one rim from site 1 (determination not entirely certain), two rims (fig. 34, 1–2) and one wall-fragment from site 315, one wall-fragment from site 283, and a rim from site 123 (fig. 34, 3).

2 Rim-sherds of the bowl Barbarathermen type 24 (fig. 34, 4–5) which is related to Böhner's type 3. Apart from the parallels cited by Böhner and Hussong/Cüppers,[52] examples from Düsseldorf-Oberlörick[53] and Mainz-Finthen[54] can be mentioned. It should be noted that both sherds are very small and display no white painting. They were found at site 315.

3 Bowl with flattened lip, Barbarathermen type 23a (fig. 34, 6). The bowl is evidently related to the terra sigilla type Chenet 320 and is indeed sometimes decorated with rouletting (type 23b). Comparable bowls are, for example, Krefeld-Gellep type 283[55] and Rübenach, *Taf.* 43, 9.[56] The sherd was found in *Schicht* 3 at site 460.[57]

4 Wall-sherd of a lid Barbarathermen type 19 (fig. 34, 7) from site 315. The lid has clear wheel-marks on the inside. On the outside, white painting is present, probably from a tree-like decoration similar to Hussong/Cüppers, *Taf.* 20, 19. Other examples are known from Haillot,[58] Düsseldorf-Oberlörick,[59] and Mainz-Finthen.[60] Later *rotgestrichene Ware* with white painting is discussed by Neuffer-Müller.[61]

6.2.3 Gallo-Belgic Ware

The ancestry of the rather heterogeneous group of pottery termed 'Gallo-Belgic' or 'Belgic' ware [62] can be traced back primarily to La Tène forms. It was produced by Gaulish potters under the influence of Roman techniques and forms, a process which must have started around the time of Caesar's campaigns.[63] It seems obvious that it was introduced into the river area by the army during the Augustan campaigns, because there was no local industry tailored to meet Roman demands. That does not necessarily imply, however, that all Gallo-Bel-

44 This material has been incorporated in Dr Bakker's dissertation.

45 ROB-excavations under the direction of Dr J.H.F. Bloemers. Comparable material, especially red-painted (or, partially, colour-coated?) flagons and jugs like Alzei types 17–19 are, however, present here as well as on other sites, for example nos. 126, 315, 321, and 466.

46 Von Petrikovits 1937, 331, type Qualburg A.

47 Hussong/Cüppers 1972, 85–93.

48 *Op cit.*, 66.

49 *Op. cit.*, 72, note 369.

50 Pirling 1966, 128.

51 Neuffer-Müller/Ament 1973, also on pp. 30–1.

52 Böhner 1958, 36; Hüssong/Cüppers 1972, 91–2.

53 Müller 1959, *Abb.* 53, 2.

54 Stümpel 1978–9, *Abb.* 45, 14.

55 Pirling 1974, *Typentafel* 7.

56 Neuffer-Müller/Ament 1973.

57 Von Petrikovits 1937, *Abb.* 24, 12.

58 Breuer/Roossens 1955–56, 20.

59 Müller 1959, *Abb.* 53, 1.

60 Stümpel 1976–7, 301–2, *Abb.* 51, 1; 1978/9, 357, *Abb.* 45, 14–1.

61 Neuffer-Müller 1962.

62 The term was invented by H. Dragendorff (1895, 87), who considered Gallia Belgica as the primary area of production.

63 Vegas 1975, 14–20 Also, as is stressed by Greene (1979, 103, note 2), the influence may well have been mutual and not from one side only.

Fig. 35 Kilns producing Roman (1) and/or Gallo-Belgic (2) wares in the first half of the 1st century.

Site	Finds	Date	Pottery	Literature
Asberg, Kiesgrube Liesen	2 kilns	c. AD 50	?	Bechert 1973, 162–3; 1974, 188–9
Bonn, Metternicher Hof	1 kiln	IA	1, (2)	Lehner 1901, 221–2
Cuijk, Grotestraat/Kerkstraat	2 kilns	Claudian	2	Bogaers 1966, 67
Haltern, Hauptlager	5 kilns	Augustan	1	Stieren 1932; Beck 1968, 183
	refuse pits	Augustan	1	Von Schnurbein 1974
Kalkar, Bornsche Feld	1 kiln	Ib	1	Hinz 1961; Gechter 1979, 111
Köln, Neumarkt	2 refuse pits	Ib	1	Fremersdorf 1950, 64–6
Köln, Lungengasse	12 kilns	Ib	1, 2	La Baume 1958
Köln, An der Rechtschule/ Römergasse	1 kiln	Ib	1, 2	La Baume 1962–3
Köln, Waidmarkt	1 kiln	Ib	1	La Baume 1964
Köln, Severinstrasse	1 kiln	Ib	1, 2	La Baume 1964
Köln, Georgstrasse	1 kiln	Ib	1, 2	La Baume 1964
Neuss, Lager B/C	17 kilns	IA	1, 2	Filzinger 1972, 50–71; Bruckner 1975
Neuss, Gagelweg	2 kilns	Ib	1, 2	Filzinger 1972, 50–1, 71–96
Niederberg-Auf dem Klinkenberg	1 kiln	IA(?)	2 (?)	Bruckner a.o. 1959, 423–7
Nijmegen, Hunerberg	1 kiln (?)	Augustan	1	Brunsting 1964, 308
Oberaden, Beckinghausen	2 kilns	Augustan	1	Albrecht 1938, 22–3
Xanten, Fürstenberg	2 kilns	Augustan	1, 2	Hagen 1912, 343–62
Xanten, Fürstenberg	1 kiln	Ib	1 (2)	Lehner 1930, 23–5
Xanten, CVT	1 kiln	c. AD 50	?	Von Petrikovits 1952, 71

gic pottery was imported, or that it was made exclusively for military use.[64]

Especially in the first half of the 1st century, a considerable amount of pottery may have been imported from Gaul, but even for this period the evidence for local production of both Roman and Gallo-Belgic pottery (often indeed from the same kilns) is quite impressive. For the lower German region, kilns have been excavated at the sites listed in fig. 35.

It is certainly no coincidence that almost all these sites are directly linked to the presence of the army, but this situation did not continue. Later in the 1st and in the 2nd centuries, kilns producing Gallo-Belgic wares (among others) may also be found in civil or less obviously military contexts. A number of these have been compiled by Filzinger.[65] A few kilns should also be mentioned in the Batavian area. Apart from the large military pottery industry at De Holdeurn (site 433) which apparently produced cork-urn types,[66] and the equally military kilns in Cuyk mentioned in fig. 35, there is a kiln site attested for Ulpia Noviomagus (site 399)[67] and one in Halder.[68] Furthermore, on at least two sites in the river area, near Hien (59) and Lienden (250), terra nigra sherds were found which have been interpreted as wasters and probably point to local production.

Frequency

Gallo-Belgic wares are fairly common in the river area, even more than would appear from the catalogue where they are lacking at quite a few sites. The reason for this is, however, only a matter of definition, especially where the later terra nigra wares are concerned. These terra nigra-like wares very often have a light or dark-grey colour and are frequently tempered with fairly rough sand or other grit. Wall-sherds of this fabric are often indistinguishable from those of coarse wares. Indeed the argu-

64 Cf. Greene 1979. See also below, 162.
65 Filtzinger 1972, 102–5.
66 Holwerda 1944, Pl. III.
67 Daniëls 1927, 90–3: Nijmegen, Maasplein.
68 Willems 1977.

ment could be turned around by the observation that the fabric of coarse pottery-types (e.g., the cooking-pot Stuart 201) is 'terra nigra-like'. All this is hardly surprising, because in the kiln at Halder, for example; cooking-pots Stuart 201 and cork-urn types Brunsting 16 C were fired in the same kiln and made in the same fabric.[68]

Without going into very detailed and, in the absence of, for example, petrological studies, very hazardous and probably ambiguous definitions of fabric-categories, it seemed advisable to conduct the inventarisation along typological lines. Therefore, unless clearly defined wares could be discerned, as in the case of 'true' terra nigra and 'Rupelian' ware (see below, type T1), the pottery in the catalogue has been grouped typologically, with the result that types like Stuart 201 or 218 and Niederbieber 89 are never included in the 'Gallo-Belgic' category because of their fabric. As a consequence, the coarse and possibly terra nigra-like but typologically indeterminable wall-sherds were not included either.

Although until now no kilns have been located in the smaller settlements in the river area, this coarse pottery is frequent enough to be considered a possible local product. One of the characteristic types generally manufactured in this fabric are the coarse 'cork-urn' types (Brunsting 16C), cooking-pots with inturned, thickened rims of various shapes.[69] These occur frequently in Nijmegen, where they were produced: both in Ulpia and possibly,[70] in a slightly different orange coarse ware, at De Holdeurn. They are also fairly frequent, occasionally even in large numbers, in the region and have been recorded for sites 38, 39, 43, 59, 62, 79, 81, 90, 93, 96, 105, 117, 126, 144, 194, 214, 231, 232, 235, 239, 272, 278, 280, 306, 315, 336, 341, 385, and 525, a number that will certainly be augmented when material from unpublished excavations is included.

Although comparable pottery was already present in Haltern,[71] and produced in the early kilns at the Lungengasse in Köln,[72] the type was retained at least during IIA and probably even as late as the 3rd century.[73] The kiln at Halder has been dated to the early-Flavian period.[74]

The hand-made, but typologically identical, true cork-urns with heavy organic tempering (Haltern 91A) are not used over such a long period. In fact, they are both spatially and chronologically restricted. The main area of the distribution is on the lower Rhine as far as Mainz and down to Belgium, but the area in which they most frequently occur is between Nijmegen and Köln.[75] In general, the cork-urns can be dated to the first half of the 1st century, when they are very often found on many sites.[76] This period should perhaps be extended to 'pre-Flavian' in view of the frequencies at Neuss,[77] but in any case they also occur even later than that as already pointed out by Loeschcke and more recently repeated by Schönberger/Simon.[78] From the mid-1st century, or at least from the beginning of the Flavian period, the numbers of hand-made cork-urns must have diminished rapidly.

As was to be expected, the number of sites with cork-urns in the area is restricted. Outside Nijmegen they occur on nos. 38, 93, 96, 117, 126, 214, 232, 244, 250, 299, 336, 341, 356, 385, and 501. In general, these have been taken to indicate the probable existence of those sites in IA, a decision which is supported by additional evidence for most of them.

As far as other early Gallo-Belgic pottery is concerned, the distribution of terra rubra and girth-beakers also requires discussion here. Terra rubra is chronologically restricted to pre-Flavian times and most of this ware was produced in the first half of the 1st century. While common at the early Nijmegen sites, it is virtually absent in the region. Examples have been recorded at sites 96, 151, 292, 298(?), and 433. Apparently, it was hardly available outside the main centre.

Surprisingly, the same is not true for girth-beakers, which have a restricted but relatively wide distribution: sites 43, 48, 93, 95, 100, 105, 117, 123, 126, 150, 310, 362, 396, 440, 454, 490, and 501. Because of the frag-

69 See e.g. Holwerda 1941, Pl. XVII and Willems 1977, *afb.* 9.

70 See Haalebos/Thijssen 1977, 102. The pots could be earlier, see a.o. Schönberger/Simon 1976 (Rödgen), type 58, variety A.

71 Loeschcke 1909, types 58 and 91B.

72 La Baume 1958, *Abb.* 17.

73 Brunsting 1937, 126; De Laet a.o. 1969, fig.117 (IIIA); Oelmann 1914, *Abb.* 54, 11.

74 Willems 1977, 125.

75 Vegas 1975, 39. See also Schönberger/Simon 1976, 105 and note 646.

76 Vegas 1975, 39.

77 Filtzinger 1972, 8.

78 Loeschcke 1909, 294; Schönberger/Simon 1976, 105 and note 644.

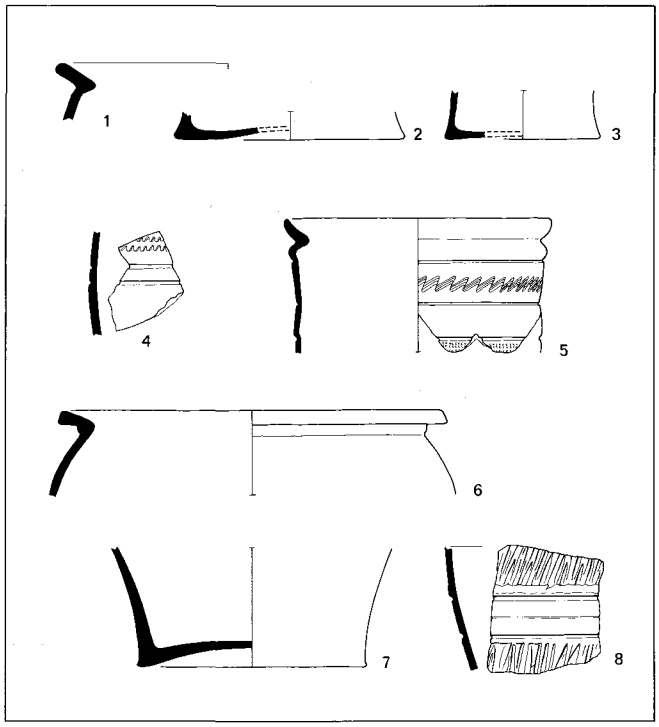


Fig. 36 Examples of girth- and butt-beakers in the river area: 1 butt-beaker Ha 84/5 from site 123, 2–3 butt-beakers, probably Ha 84, from site 126, 4 butt-beaker Ha 85 from site 126, 5 girth-beaker Ha 87 from site 310. All these are coloured orange to chocolate-brown. Nos. 6–8, which may be from one vessel (site 310), seem to be of the form Ha 84, but they are definitely terra nigra-like.

mentary nature of the material, all orange- to chocolate-coloured vessels with sharply everted rims and various decorations (Haltern types 84, 85, and 87) are grouped together as 'girth-beakers', although, strictly speaking, only Ha 87 should be so called. A differentiation between girth- and butt-beakers proved to be impossible in several cases. The butt-beaker Haltern 85 was also made in terra nigra ware, but, apart from one example from site 501, it has not been recorded elsewhere. Complete specimens of Ha 84 were recorded for sites 396, 440, and 454, of Ha 87 for sites 440 and 490, fragments of Ha 84 (?) for 100 and 105, and Ha 85 (?) for 126 (see fig. 36).

Although girth- and butt-beakers were very common under Augustus and Tiberius, they remained in use during the entire pre-Flavian period as is testified by e.g. the material in Neuss, Hofheim, and Colchester,[79] the terra nigra form Ha 85 even later.[80] Imitations in other wares are also common, for example, in Colchester,[81] but also elsewhere.[82] In Nijmegen, Flavian imitations in 'fine Nijmegen' ware of Ha 84 with 'checker board' decoration are a possibility.[83]

In general, just as in the case of Ha 91A, the presence of Ha 84, 85, and 87 has led to a fairly early dating of the sites where they were found and that is also in most cases confirmed by other evidence. This use of the material is further justified by the fact that if an example of it turns up in a (relatively) small collection from a site, it is more likely to have been used at that site in a period when it was comparatively frequent than during periods before or afterwards, when it was less frequent.

It appears from the distribution of these early Gallo-Belgic wares that they were not exclusively used by the army. Of course, in quantitative terms almost all the material can be ascribed to the army: the Nijmegen sites and also nos. 105 (Elst), 117 (Driel), 126 (Meinerswijk), 433 (Holdeurn), and 501 (Cuyk), for which close links with the army have been established or assumed. That leaves only a minute percentage for the remaining sites (probably less than 1 % of the total number found in the area), but the fact that they exist should not be ignored.

Terra nigra-like wares

The terra nigra-like wares and their relation to coarse wares have already been discussed. Although many types could be said to have been manufactured in a terra nigra-like ware, there are only three for which this is consistently true.[84] All three qualify as possible local products. These are the cork-urns Brunsing 16C and two large, wide-mouthed jars described in the catalogue as types 1 and 2 (T1, T2).

79 For Neuss see Filtzinger 1972, 6, 9 and Vegas 1975, 20–2, for Hofheim Ritterling 1912, 354–6, and for Colchester Hawkes/Hull 1947, 232–4, 237–41.

80 Filtzinger 1972, 6 (Hofheim type 126).

81 Hawkes/Hull 1947, 232 ff.

82 See Schönberger/Simon 1976, 174 and notes 167–8.

83 That was at least the impression gained by S.L. Wynia and myself in 1976, while sorting the material of the 1957–67 excavations of the Roman fortress in Nijmegen (directed by Professor H. Brunsting); a clear example is a sherd from find no. Ca59/115.

84 These have, therefore, been recorded under Gallo-Belgic wares in the catalogue.

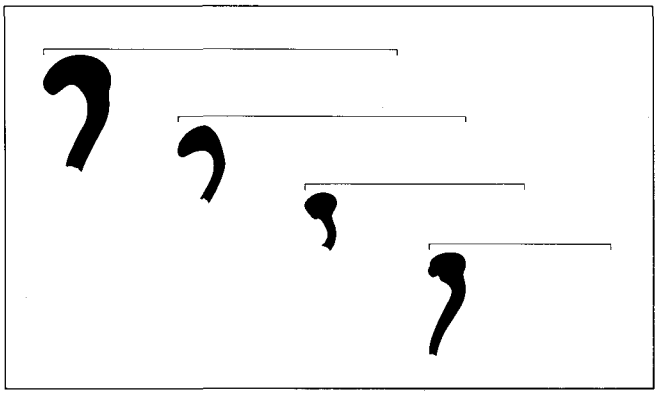


Fig. 37 Type 1 jars with different varieties of rims from Hien (site 59).

The large T1 storage jars are generally referred to as 'Arentsburg pots', since they were first described by Holwerda (1923, 124, type-nos. 141/2). As far as the ware is concerned, this is a very clearly defined group, which is invariably manufactured in a light-blue or brown-grey sandy fabric, sometimes referred to as 'Rupelian', after the river Rupel, a tributary of the Scheldt. The Scheldt valley has sometimes been considered the centre of production of this group of pottery,[85] for it is frequently found in Belgian sites. With the remarkable exception of Zeeuws-Vlaanderen (Dutch Flanders) it is, however, also very frequent in the provinces of Zeeland and South Holland. Moreover, it is also common on sites in Brabant and in the river area, where rim-sherds were recorded for at least 63 sites, sometimes in massive quantities. Local production in these regions can, therefore, not be excluded.

Although smaller examples occur, most of the pots are large and, because no signs of soot-incrustation have been observed, they should probably be interpreted as storage jars. This is further supported by the frequent *post cocturam* graffiti (often ciphers) on the rims[86] which may well refer to the content.

Just like the fabric, the general profile of these jars is very uniform, but the shape of the rims may vary slightly. The type as described by Holwerda is by far the most common but other variants occur, as shown by a few rims from site 59 (fig. 37). A discussion of all the typological differentiations would not be very fruitful here. The difference between Holwerda's 'types' 141 and 142 does not really exist, and small differences from other rim-profiles are unimportant in the absence of any chronologically or spatially restricted varieties.

All the rims designated as type 1 have one feature in common: a rather heavy, *rounded* form, almost invariably thickened on the inside of the vessel. This implies that also quite a few varieties of rims of the rather rare type Holwerda 140 have been included.[87] In the absence of any clear justification for Holwerda's different datings of his types 140 and 141/2,[88] this presents no special problems. As far as the chronological position of type 1 jars is concerned, a dating from II-IIIA is possible. As was observed in the settlement in Rijswijk,[88] on several sites in Zeeland,[89] and in the river area, the type seems to be especially common towards the end of II and in IIIA. In general, it may be considered characteristic for the 'Niederbieber'-horizon.

There are several differences between T1 and T2, both in fabric and shape. While type 1 was always manufactured in only one fabric, type 2 occurs in several different terra nigra-like wares. It was made in the same light blue-gray fabric as type 1, but also in a number of other fabrics, sometimes much harder and often with larger tempering grit. While the surface is always some shade of blue- or brown-grey, the paste may be white in section. In fact, as already noted above, these fabrics are almost identical to those used for several coarse pottery types.

The typological differences between type 1 and 2 concern both the general shape and the rim profiles. Type 2 vessels are generally smaller and should probably be interpreted as cooking-pots. The rims are not round but angular on the inside, and they all have a groove for a lid. They are generally less heavy than those of type 1 and not bent down so far on the outside. While the openings of type 1 jars may have been covered, for example, with a skin, fastened with a cord under the rim, type 2 rims are clearly intended for a lid. The vessels very often have a small ridge on the transition from rim to shoulder.

A large number of variants of type 2 rims (see fig. 38) has been recorded on at least 48 sites in the river area. They

85 Trimpe Burger 1979, 45.

86 See for example Bloemers 1978a, *Abb.* 142–3 and Verwers 1975, Pl. 13, 4–5.

87 For profiles of some of these see also Bloemers 1978a, *Abb.* 105.

88 See Bloemers 1978a, 265.

89 Pers. comm. J.A. Trimpe Burger.

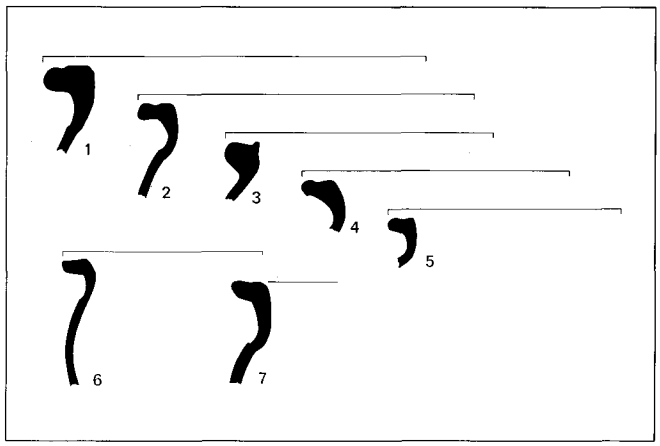


Fig. 38 Type 2 cooking-pots from Hien (site 59, nos. 1–5) and Lienden (site 250, nos. 6–7).

range from Holwerda's type 140 (fig. 38, 1) to forms related to Stuart 201 (fig. 38, 4–7), which also occasionally seems to have a neck.[90] A relation to Stuart 210, as proposed by Verwers for a few examples of this type from Wijk bij Duurstede,[91] seems less obvious.

Although type 2 cooking-pots do occur further east[92] they seem to be most common west of Nijmegen and in the coastal areas. Typological equivalents are present elsewhere, for example, the brick-red 'military cooking-pot' of the 11th legion at Vindonissa[93] or, for that matter, some of the cooking-pots of De Holdeurn described by Holwerda.[94] Of course, these apparent similarities can only be interpreted as functional equivalents because clear relations cannot be established. Type 2 cooking-pots as described here are also a far from homogeneous group. It could well be that in the future several chronologically and spatially separate types can be differentiated. As long as kiln sites and/or petrological studies are lacking, such a distinction is very difficult. A dating of type 2 is, consequently, also problematical. For the moment, the same period as for type 1 (II-IIIA) seems reasonable, but it could also be a little earlier and it is not especially common in the 3rd century.

Late-Roman terra nigra

The only terra nigra types still used in any quantity in the river area during the Late-Roman Period are two bowls, both related to earlier forms like Hofheim 116. The development of this type from the 1st to the 5th century is discussed by Unverzagt and Van Es.[95] A great number of 2nd- and 3rd-century examples are also discussed and illustrated by Bloemers.[96]

The relatively thick-walled low bowls with a curved neck and a wide, low foot (Alzei type 24/25 and 26) are very rare. These have been recorded for sites 283, 315, 391, 460, and for site 466.[97] It should be noted, however, that fragments of the bowls are so similar to their earlier predecessors that they have only been recognized at late-Roman sites. It is possible, indeed probable, that single sherds in other collections may have been dated too early. The same could be true of the smaller bowls or rather cups with curved or straight neck and high foot of Chenet type 342,[98] but the rim and especially the foot of this vessel is much more characteristic. It was probably used during the 3rd century[99] but is particularly common during the 4th and 5th centuries. Several examples from site 123 are illustrated on fig. 39. The type was found at several other sites, namely, nos. 1, 5, 7, 12, 16, 17, 21, 150(?), 292, 315, 403, 405, 410, 460, 466, and 499.

The occurrence of this terra nigra cup in the river area is not altogether surprising. Van Es[100] already pointed to a (then still slight) concentration of the cups 'in the Nijmegen-Wageningen-Rhenen area'. They are also, with late-Roman terra sigillata, the only late-Roman pottery exported in any quantity to the north,[101] which gives them a special importance because normal kitchen wares are exceedingly scarce in those regions during this period.[102] The relative frequency of Chenet 342 cups found outside the empire indicates, of course, a preference, but it could also signify that it was fairly expensive as well.[103]

90 Cf. Gose 1950, 535.
91 Verwers 1975, fig. 9, 22–3 and p. 106.
92 Cf. Cüppers 1962, *Abb.* 40, b.
93 Ettlinger 1977, 49–50, group 7.
94 See Holwerda 1944, Pl. 4.
95 Unverzagt 1916, 25–9; Van Es 1967, 158–68.
96 Bloemers 1978a, 256–61.
97 They are probably also present in Nijmegen.
98 Chenet 1941, 91–4.
99 Bloemers 1978a, 260–1 and Van Es 1967, 168.
100 Van Es 1967, 552.
101 See also Van Es 1981, 268, fig. 209 and Van Es/Verlinde 1977, 25, fig. 9.
102 An inventarization of the Roman imports in the province of Overijssel, for example, yielded only two sherds of late-Roman coarse pottery types: Van Es/Verlinde 1977, 46, d and i. At a large settlement like Wijster somewhat more sherds were found (Van Es 1967, 169–72).

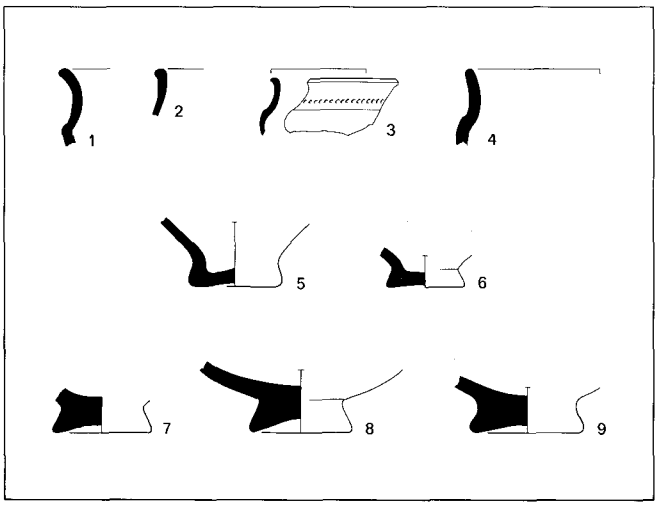


Fig. 39 Fragments of terra nigra cups Chenet 342 from Driel-Oldenhof (site 123).

The latter argument is supported by the fact that the cups are relatively rare on sites south of the Rhine, where people could apparently acquire a whole range of late-Roman pottery, and also by the clear association of Chenet 342 cups as exports outside the empire alongside all sorts of more or less expensive exported goods such as coins, metal objects, glass, and terra sigillata. It could well be, therefore, that it was a prestige item both inside the empire and beyond the frontier. However, these arguments may prove completely invalid if it should turn out that the cups were actually also produced outside the empire, as proposed by Mildenberger.[104] In this respect, the border region itself might just as well be proposed as the production area. The concentration of cups in and around the eastern river area, some of which are clearly connected to a group of early-Medieval terra nigra,[105] may also be taken to indicate local production.[106]

6.2.4 Smooth Ware

Smooth ware includes the smooth-walled pottery which consists predominantly of flagons and the *Schwerkeramik*, thick-walled pottery which may have a rather coarse surface and may be further divided into amphorae, dolia, and mortaria. In general, it has been established that smooth-walled pottery is almost always found on middle-Roman sites and that mortaria and amphorae also occur very frequently. On early-Roman sites, these are both considerably less common, while on late-Roman sites only thick-walled pottery is present because smooth-walled flagons were replaced by other wares towards the end of the 3rd century.[107] Early flagons are not frequent outside Nijmegen. They were found at only six sites and are of four different types:
One-handled flagon Ha 45: sites 234 and 440
One-handled flagon Ha 47: sites 117, 126, and 142
One-handled flagon Ha 48: site 118
Two-handled flagon Ha 52: site 126
Of course, these are only the typologically earliest flagons and quite a few examples belonging to the Hofheim-horizon can also be dated to the Early-Roman Period, as is attested if not by the type site itself[108] then at least by the kilns discovered beneath the Lungengasse and An der Rechtschule in Köln.[109] These kilns were used for the production of types excavated at Oberaden and Haltern as well as of those at Hofheim. Some of the Hofheim forms from the river area – which have in no case been used to date a site to the Early-Roman Period – could in fact do just that. In some cases an early date was achieved by other means, but the beginning of at least a few sites may have been set at a date which is actually too late. Outside Nijmegen, Hofheim flagons which are

103 Van Es/Verlinde 1977, 24–5 seem to argue exactly to the contrary.
104 Mildenberger 1972b.
105 See paragraph 6.2.7.
106 In this respect, site 7 (Bennekom-Achterstraat) deserves special attention.
Cursory examination of the Roman material, at present at the Archaeological Institute of the Free University in Amsterdam, on 16.10.1981 revealed the presence of joining orange (oxidized) and black (reduced) sherds of Chenet 342 cups. This same phenomenon was observed on 1st-century terra nigra from the kiln in Halder (Willems 1977, 117–8 and fig. 6. For a colour picture, see also Bloemers/Louwe Kooijmans/Sarfatij 1981, 107). Although the presence of these sherds in Bennekom cannot be interpreted as incontrovertible evidence for local production of Chenet 342 cups, this is at least very probable!
107 See e.g. Von Petrikovits 1970, 400.
108 Which could have begun a little later than is usually assumed, cf. Baatz 1963, 189–90; the numismatical evidence in Ritterling's original argument, however, still carries great weight, cf. De Weerd 1977, 259–62.
109 La Baume 1958 and 1962–3. See also Stuart 1977, 15–6.

dated to between AD 25 and 70/80[110] were found on the following sites:[111] 23, 43, 75, 93, 96, 105, 117, 123, 126, 135, 150, 156, 234, 239, 250, 298, 299, 300, 336, 341, 385, 396, 459, 487, 491, and 499.
As far as thick-walled pottery is concerned, early forms are virtually non-existent outside Nijmegen. Amphorae Ha 69 (site 117) and Ha 71 (sites 126 and 515) and mortaria Ha 59/Hofh 79 (sites 43, 126, and 194) are found rarely. Late amphorae have not been identified. Only late Roman mortaria (c. Al 31) are present, but they are scarce (site 123 (see fig. 40, 8), 315, 391 (?), 460, and 499).

Production
Smooth ware was produced locally in Nijmegen, at De Holdeurn, and possibly in Halder.[112] Indications for production in the river area outside Nijmegen are lacking. For the moment, it seems probable that a very sizable part of the smooth ware in the river area was imported from elsewhere.

6.2.5 Coarse Ware
More abundantly than smooth ware, coarse ware is present at nearly all sites. It is not a very uniform ware, and a large degree of variation in fabrics can be observed. Their common feature is their tempering, usually with sand-grains, and a more or less coarse surface. But even in these characteristics there is much variability, probably relating primarily to the function of the different forms. Even though characteristic local products are available in the 'brick-red' coarse pottery from De Holdeurn and the 'mud-coloured' pottery from the Maasplein kilns in Nijmegen, these have not been catalogued separately.
Because there is as yet no ceramological analysis of the red or orange coarse wares allegedly from De Holdeurn[113] it is not at all certain that all the red- or orange-coloured pottery found in Nijmegen, let alone that from further away, was indeed produced at De Holdeurn.[114] The same is true for the 'mud-coloured' Maasplein products. A distribution map of the products from these kilns would, therefore, present a completely unreliable picture.
There are only a few coarse-ware types found in the river area which are chronologically restricted to a short period. These include, for example, the small beakers Hofh 85 (Stuart 204A) dated to AD 25–80.[115] On the other hand, several longer-lived types sometimes show chronologically significant typological variability.

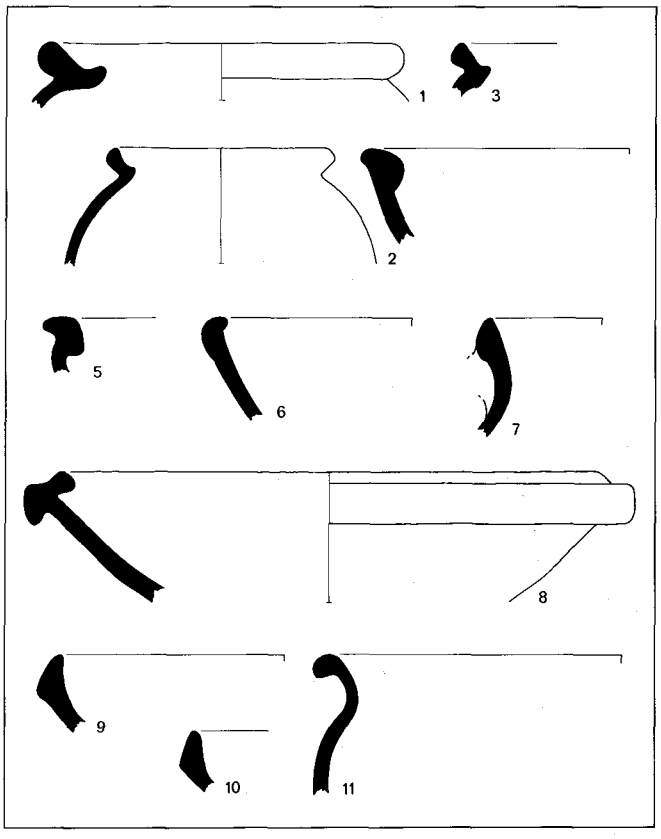


Fig. 40 Various types of late-Roman Mayen ware from Driel-Oldenhof (site 123). The bowl no. 11 is of a different fabric.

110 These are mostly Hofheim 50 and 51, but a few examples of other types do occur. Longer-lived types have, of course, been excluded.
111 Sites with the Flavian flagons Stuart 107 are not included, although Stuart (1977, 45) concluded that there is actually no sharp typological criterion for their differentiation with Hofheim 50. On the other hand, occasional finds of handles with four or more ribs are included here because they are datable to the same period, cf. Stuart 1977, 46.
112 Nijmegen: Daniëls 1927, 90–3; De Holdeurn: Holwerda 1944; Halder: Willems 1977.
113 Holwerda 1944, 3–4.
114 Cf. Haalebos/Thijssen 1977, 102, 110.
115 Stuart 1963, 2nd ed. 1977, VII.

Cooking-pot with everted rim

Such a type is, for example, the cooking-pot with everted rim Stuart 201. Its development is shown by Gose 530–539. The earliest form Gose 530 has not been recorded outside Nijmegen but for one exception, namely at the fort in Meinerswijk, where it was found in the first-period deposits. The fabric in this case is different from later wares: the pottery is tempered with very fine sand-grains, is much softer, and has a smooth rather than coarse surface.

There is one variant of this cooking-pot which occurs in relatively large numbers in the river area but which has not been described separately by Oelmann, Brunsting, or Stuart. It is recorded in the catalogue as type 4, and was also recognized by Cüppers in his description of the ceramics from the Kreis Bergheim.[116]

Type 4 is related to Stuart 201 B, the cooking-pot with a flat, everted rim (Brunsting 1B, Niederbieber 87). In this case, however, the rim is thickened at its base and is slightly (Cüppers type IIIC) or sometimes even strongly (Cüppers type IIID) concave. For examples of such rim profiles, see Cüppers, *Abb.* 19, 1–8 and 16–23 and Oelmann, *Abb.* 54, 4.[117] Cüppers relates his form IIIC to Niederbieber 87 and IIID to Niederbieber 89, probably on account of the varying depth of the concavity which undoubdedly served as a groove for a lid. In any case, the rims of type 4 are typologically between the flat everted rims Stuart 201B and the heart-shaped rims Niederbieber 89. Functionally, they are closer to the latter, and they can be considered an equivalent of the terra nigra-like cooking-pots of type 2.[118] Their chronological position could also be roughly similar. Cüppers dates his type IIIC predominantly to IIB and states that it must have disappeared during IIIA. Unfortunately, this assertion is not elaborated further and a somewhat earlier date cannot be excluded entirely.

Cooking-pot with crescent-shaped rim

The cooking-pot Niederbieber 89, which is functionally related to type 4, is another long-lived type which shows a chronologically significant typological development. The type occurs for the first time under Hadrian[119] and becomes especially popular in IIB. The gradual change of the heart-shaped rim Niederbieber 89 during the 3rd century and its development into the crescent-shaped rim Alzei 27 in the 4th century are described in detail by Von Petrikovits and Cüppers.[120] In fact this development goes on into the Early-Middle Ages.[121]

Although both Von Petrikovits[122] and Cüppers are very confident about the value of their typological criteria, these proved to be difficult to apply to the rimsherds in the collections from the river area. Probably late (IIIB-IV) variants of Niederbieber 89 have generally been indicated in the catalogue but have not been used to extend the period of occupation of a site to after AD 270, unless other evidence was available. As a result, several sites with late-Roman occupation may not have been identified as such, as is exemplified by site 59 where late variants of Niederbieber 89 were found. At first, the period of occupation was not extended beyond AD 270 because no other clearly late-Roman ceramics were identified. A few months later, however, it turned out that two bronze coins, barbaric imitations dated to IIId,[123] had also been found at that site. Consequently, the period of occupation was extended to AD 300.

The latest variants of Niederbieber 89 (Niederbieber 89e or Alzei 27d in the terminology of Cüppers and Von Petrikovits, respectively) have been described in the catalogue as Alzei 27. Early-Medieval rims are described under Merovingian wheel-turned pottery as 'crescent-shaped rims'. A few examples of Alzei 27 from Driel-Oldenhof (site 123) are illustrated on fig. 40, 1–3, together with examples of other late-Roman coarse pottery from that settlement. All pottery is tempered with large stone grit and has a very rough surface. The colours range from orange to yellow and grey, and the pottery is usually very hard. All of it was probably imported from Mayen.

Fig. 40, 4–5 are rim-sherds of the bowl Alzei 28, no. 6 of Alzei 29. The jug Alzei 30 is represented by the rim-fragment with broken-off handle no. 7. The only fragment of a late-Roman mortarium (Alzei 31) from this site is illustrated on fig. 40, 8. The rims on fig. 40, 9–10 belong to the dish Alzei 34. The fragment of a bowl (11)

116 Cüppers 1969, 97–8, IIIC and D.
117 Oelmann 1914.
118 See paragraph 6.2.3.
119 Brunsting 1937, 144.
120 Von Petrikovits 1937, 333–4; Cüppers 1969, 99–107; see also Van Es 1967, 169–72 and note 43.
121 Stamm 1962, 125; Böhner 1958, type D 12; Pirling 1966, type 159; Hussong/Cüppers 1972, *Taf.* 22, 9a, 9b.
122 See also Von Petrikovits 1971, 178–9.
123 Identification by the Koninklijk Penningkabinet, The Hague.

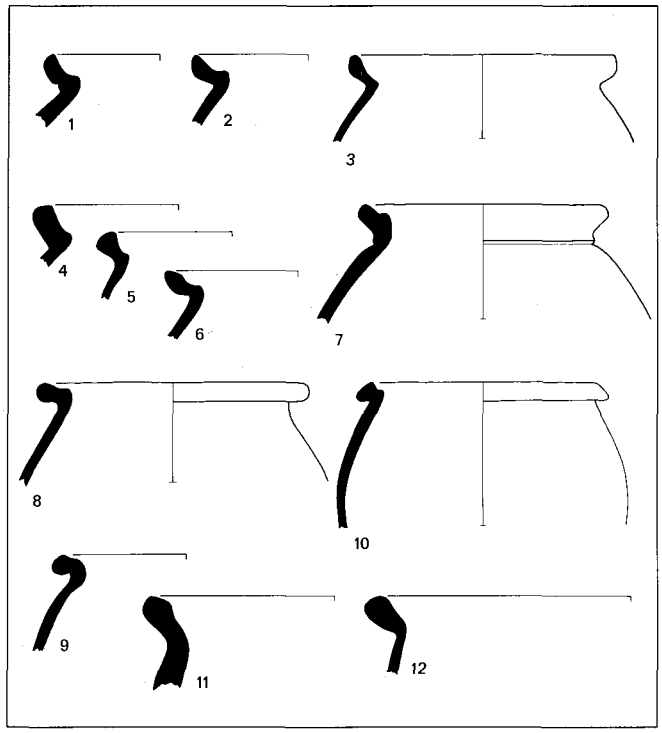


Fig. 41 Early-Medieval crescent-shaped rims from Driel-Oldenhof (Site 123).

is of a somewhat different fabric (not Mayen) with less coarse tempering and surface. It is probably related to the bowls of type 66 from the *S-Keramik* in Trier.[124]

Fig. 41 gives a small selection of different crescent-shaped early-Medieval rim-profiles from site 123. Their variation range covers other examples from sites elsewhere in the region. The rims are generally smaller than their late-Roman predecessors and are often raised very high on the outside and much less, or not at all, on the inside, which results in a very steep groove or *Kehlung* (fig. 41, 1–6). The development of the profile can be observed in the different types from Trier:[125] *Kellergänge* type 42, Barbarathermen type 36, and the Frankish type 9.

The chronological position of the different rim-profiles is not altogether clear which may, in part, be due to their infrequent use in burials.[126] The rims on fig. 41, 1–6 would seem to be the earliest. Their form is related to Barbarathermen type 36 and Flonheim type 118, which would argue for a dating in the 5th and 6th centuries. The fabric of these rims is in agreement with this, because it is still related to late-Roman fabrics. Nos. 1–5 are tempered with large stone grit and have a coarse surface. The colours range from light-brown and red-brown to grey and violet-grey. They are partly products from Mayen. No. 6 is tempered with much finer grains, has a smoother surface and light-grey colour, so that the fabric may even be described as terra nigra-like.

Rim-profiles like nos. 8–10 and no. 7 are definitely later than the first group. They belong to the *lampionvormige urnen*, (cooking-pots shaped like Chinese lamps) from the Wageningen cemetery described by Van Es. They can be dated to the 8th century.[127] The tempering generally consists of fairly large grit, sometimes sharp but mostly rounded, and the surface is in most cases rather smooth and feels greasy. Colours range from vivid orange to brown and light-grey. Products from Mayen, Trier, and elsewhere, may be represented in this group. Equally late are rims like nos. 11–12, which conform to Böhner's type D12 (Stufe IV) and the Trier type 9. They were probably used during the 7th–8th centuries. The fabric of both rims indicates a Mayen origin.

It is not certain whether the development of this pottery type continues into the Carolingian Period. Carolingian finds from the eastern river area have not been catalogued, and Hussong/Cüppers[128] were not able to point to typologically related later wares. However, rims like fig. 41, 7–10, which probably belong to the *lampion*-shaped pots, have also been identified among the Dorestad material.[129] They may be the link to other ovoid- or globular-shaped pots from the same site.[130]

Cooking-pot with thickened, cordoned rim (type 3)

Fig. 42 shows a number of rim-sherds from Driel-Oldenhof (site 123) which, because of the frequent occur-

124 Hussong/Cüppers 1972, 27 and *Taf.* 7, 66a. The form is clearly reminiscent of the terra nigra bowls Alzei 24–26. See also Hussung/Cüppers 1972, 76–7 (*Umbaukeramik*, type 35b).

125 Hussong/Cüppers 1972. See also Hussong 1936, *Beilage* 2.

126 Ament 1970, 102.

127 Van Es, 1964, 267–8. Compare, however, Pirling 1966, type 159, which is dated to the beginning of *Stufe* III (c. VIb). Especially in view of the clearly lenticular (sagging) base of this vessel, that is a surprisingly early date. A dating of such rims in the 7th century, however, is possible.

128 Hussong/Cüppers 1972, 110.

129 Van Es/Verwers 1980, 100–1, type W IXC.

130 *Op. cit.*, 84–7, especially type W IIIC.

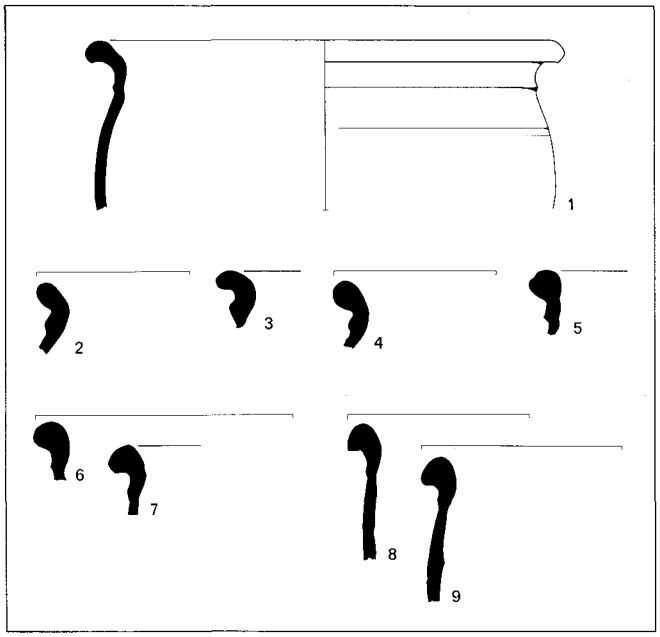


Fig. 42 Cooking-pots with thickened, cordoned rim (type 3) from Driel-Oldenhof (site 123).

rence of soot-incrustation, can be considered cooking-pots. The rims belong to sometimes rather bulbous but also frequently steep-walled bucket-shaped pots which are closely related to the Merovingian bucket-shaped pots.[131]

The everted rims are thickened (*Wulstig verdickt*) and occasionally decorated by a small groove. The most characteristic feature is a squeezed-out (occasionally possibly applied) cordon between the rim and shoulder. The latter is usually also decorated by one or more grooves. The fabric is in most cases fairly hard, tempered with large stone grit, and may have a coarse as well as a fairly smooth surface. The colours are generally red or brown to violet-grey, but lighter orange-red and yellow-brown colours also occur. Most of this material was probably produced in Mayen.[132]

This type of cooking-pot was found in the Kaiserthermen in Trier where it belongs to the *Umbaukeramik* (type 43).[133] It was also recorded among the ceramics from the Palais Kesselstatt (type 32) and the Barbarathermen (type 29),[134] but not among the finds from the *Kellergänge*. It seems, therefore, that the type appeared only at the end of the 4th century and was very common in the first half of the 5th. The origin of the type at the end of the 4th century is confirmed by finds from the fort at Alzei,[135] where only a few specimens were recorded: rims with a thick round cordon (type 33, 6 fragments) and with a less pronounced, rather pointed cordon (type 32, 1 fragment only). The latter is ascribed by Unverzagt to the latest phase of the fort. Both types were also found in fairly large quantities in Frankfurt.[136] They were studied typologically by Stamm, who suggests a development from Alzei 33 to 32 with an explicitly denied but implicitly present chronological significance, because only type 32 forms are still present in post-Roman graves dated to VB.[137]

A similar impression may also be obtained from fig. 42, where rims with small pointed cordons (nos. 7–9) belong to steep-walled pots very much like the Merovingian steep-walled vessels.[138] It is, however, contradicted by other evidence. A complete pot Alzei 33 was found in grave 792 in Krefeld-Gellep[139] with a 5th-century fibula and a franciska datable to *Stufe II* (AD 450–525), which suggests a date in VB. Moreover, in Qualburg three rim-sherds of type 32 were found in the 4th-century levels 2b-d and f, while the later levels 3 and 4 yielded three rim-sherds of type 33.[140]

Although a detailed typochronological scheme seems futile for the moment, the general chronological position

131 Hussong 1936, *Beilage* 2.

132 This was confirmed by Dr M. Redknap from the Institute of Archaeology, London, who examined the sherds in october 1981. In his opinion, all 'type 3' sherds were of Mayen origin.

133 Hussong/Cüppers 1972, 80. Compare also the rims of type 36 (*Abb.* 36), which apparently belong to a more bowl-like form of the same type.

134 Hussong/Cüppers 1972, *Taf.* 25 and 27.

135 Unverzagt 1916, 35–6.

136 Stamm 1962, 103–4 and *Taf.* 6–8, 88–109.

137 See e.g. Böhner 1958, 54, type D10 from Roden, grave 30 (*Taf.* 5, 8).

138 Although there is a clear typological relation between 'type 3' cooking-pots and the steep-walled Merovingian cooking-pots, it is worth noting that, as far as could be established, al 'type 3' vessels were manufactured in Mayen, while only a relatively small percentage of the Merovingian pots is in Mayen ware. This observation is not only valid for Driel-Oldenhof (see also note 132) but applies to all of the material from the river area.

139 Pirling 1966, 141–2, type 157.

140 Von Petrikovits 1937, 334 and *Abb.* 25, 20–21.

of the vessel is clear. It must have originated at the end of the 4th century and become popular during the first half of the 5th. Because there are no indications anywhere for a date long after AD 500, it must have disappeared around that time, towards the end of Böhners *Stufe II*.

Thus, it seems to be a pottery form which is especially characteristic for the 5th century. Surprisingly, it turned out to be fairly common in the river area. In the catalogue it has been recorded as type 3 (T 3). Rim-sherds were found at the following sites: 1, 46, 62, 63, 75, 79, 80, 81, 109, 114, 123, 126, 135, 143, 152, 161, 168(?), 234, 270, 291, 315, 420(?), and 461. It is remarkable that 16 of the 23 findspots are from sites in the Betuwe, between Waal and Rhine. Even in Nijmegen, type 3 pots seem to be lacking. As far as the dating of individual sites is concerned, the presence of these cooking-pots has been taken to indicate continued occupation during the 5th century.

6.2.6 Other Roman Pottery

Roman pottery which does not belong to the categories discussed in paragraphs 1–5 is only rarely encountered in collections from sites outside Nijmegen. Fragments from (mould-made) lamps and pipe-clay statuettes, for example, are only sporadically represented. Mica-gilt ware, although produced in Cuyk (site 499),[141] is also very rare: it was recorded only for sites 38, 126, 214, 234, 341, 385, and 505. In fact, the only category which needs to be discussed here is the so-called 'fine Nijmegen' ware.

The pottery that goes by this name is usually thin- or even very thin-walled and generally polished. It was always fired in an oxidizing atmosphere and both surface and paste usually have a vivid orange colour, although other colours occur. The pottery was introduced by the *legio X gemina*, which was ordered from Spain to Lower Germany after a brief sojourn at Carnuntum on the Danube. Both plain and decorated forms are related to legionary pottery from elsewhere[142] and they all have a fairly restricted distribution.

The fine Nijmegen ware was probably only manufactured as long as the 10th Legion was stationed in the Nijmegen fortress (site 412), but a prolonged production into the 2nd century cannot be excluded. It was certainly not manufactured before AD 70.[143] Holwerda has published a catalogue of the fine-Nijmegen ware which he termed 'Holdeurn' pottery,[144] together with the coarse 'brick-like' ware. Haalebos and Thijssen[145] have, however, shown that De Holdeurn (site 433) was not the only production centre. Fine Nijmegen ware was, according to the distribution of finds indicative of potting activities,[146] also produced in the *canabae legionis* (site 407 and possibly 416).

The distribution of this typical military ware is very restricted. According to Haalebos/Thijssen,[147] only a few sherds are known from some forts, including those at Valkenburg(?), Zwammerdam, Woerden, Vleuten, Maurik, and Cuyk. In the river area, fine Nijmegen ware has been recorded for quite a few sites outside Nijmegen and De Holdeurn, but the quantities are very small: a few sherds at the most. The original impression that it has a restricted distribution can therefore be maintained. Fine Nijmegen pottery has been identified at sites 38, 105, 117, 123, 126, 214, 222, 234, 244, 250(?), 299, 306, 336, 385, 389, 396, 500, and 513. This distribution, and especially the small total numbers of a ware which was manufactured at such a short distance away indicate that is was not, in principle, available to people in the region. It may well have been intended for or used exclusively by military personnel, in which case its presence at certain not obviously military sites would be even more interesting.

On settlements like 38, 117, 123, 126, 500, and also on 105, it would not be wholly unexpected to find exclusively military pottery, but for the other settlements (214, 222, 234, 244, 250, 299, and 513) that is another matter. All but one (244) of these have stone buildings and they also have a lot of terra sigillata.[148]

On sites 214, 222, 244, and 298 (the latter closely associated with 299), military tile-stamps were also found. The conclusion must be that fine Nijmegen ware is generally restricted to military settlements but that it does occur sporadically in some relatively wealthy non-mili-

141 Bogaers 1966, 67.

142 Ettlinger 1951; Ettlinger/Simonett 1952, 60 ff.; Greene 1972.

143 Daniëls 1955, 320–3, who is highly critical of Holwerda's (1944) datings.

144 Holwerda 1944.

145 Haalebos/Thijssen 1977, 106–8.

146 *Op. cit.*, fig. 8.

147 *Op. cit.*, 109–10.

148 See fig. 24; site 513 has only been discovered recently and little material has been collected so far.

tary settlements which may have had a special relation to the army.

6.2.7 Merovingian Pottery

As Modderman[149] has already observed, the area of the Betuwe and the Land van Maas en Waal contains many sites with Merovingian pottery. Apart from two large (12, 16) and several smaller cemeteries, there are a number of settlements that have small amounts of Merovingian pottery but some have very considerable quantities.

Smooth and coarse pottery is found on the same sites, but smooth pottery is noticeably less frequent. In general, the small sherds cannot be typologically identified, and so have been listed in the catalogue as Bö B(?) (for the black, reduced ware) or Bö C(?) (for the red or yellow-brown, oxidized ware). The same problem of identification applies to the coarse pottery, nearly always steep-walled vessels with bent-out, thickened rims of various shapes. In the catalogue such fragments are always labelled Bö D 9–12, because their variability is covered by Böhners typological series from the wide-mouthed pot D 9 to the symmetrically convex shape of vessels belonging to type D 12.

Both kinds of pottery range approximately from Böhners *Stufe* II to IV, which means it can be ascribed roughly to the period from AD 450 to AD 700. On the chronological table of the sites (fig. 23), an uninterrupted bar from AD 500 onwards conveys the presence of Merovingian pottery. This somewhat different pottery dating was employed in order to show the presence of definitely 5th-century pottery such as the type 3 vessels discussed in paragraph 6.2.5, which would otherwise have become invisible between 'late-Roman' and 'Merovingian'. On the other hand, for example, the typological range Bö D 9–12, as employed here, includes rims that may have belonged to vessels still in use during the 8th century, as exemplified by finds from Wageningen, Walsum, and Frankfurt.[150] Continuous occupation is therefore assumed until AD 750, which marks the end of the chronological table. It should be added that many, indeed most of these sites were also occupied during the succeeding Carolingian Period.

In order to give a general idea of the Merovingian pottery found on settlement sites in the river area, a selection of finds from the extraordinarity varied material from Driel-Oldenhof (site 123) is presented here. This provides a good impression of the material found elsewhere, but it also includes some remarkable finds which are of special interest.

Fabric

A Among the material from Driel-Oldenhof are a number of different pottery-types in a ware that can only be described as terra nigra. The tempering, when it is at all visible, usually consists of very fine sand-grains and occasional tiny specks of mica. The surface is entirely and carefully polished, often decorated, and fired in a reducing atmosphere. The surface is either grey or grey-brown, while the paste is slightly lighter in colour. The ware is fired fairly hard and of such high quality that it is almost identical with the polished 1st-century terra nigra, which is sometimes also grey rather than black. In the description of pottery it will be referred to as fabric A.

B Apart from this ware, there is also the usual polished black pottery as described by Böhner,[151] in which most of the Merovingian biconical pots are normally manufactured. These sherds also show a fine sandy tempering and have a grey to brown-grey or black surface, often with a somewhat lighter paste. The surface is polished, but generally less completely and certainly less carefully than ware A. Occasionally, the entire surface or parts of it feel sandy rather than smooth and the fabric is also usually softer than ware A. This has often resulted in a rather 'pock-marked' surface because innumerable small pieces have chipped off, a phenomenon that never occurs with sherds of ware A (or, by the way, with good quality 1st-century terra nigra). In the descriptions, this ware is called fabric B.

C/D As far as the coarse ware is concerned, the variation in fabrics is considerable. The tempering always consists of rather large stone grit and colours range from brick-red to white and from light-brown to dark-grey. Sometimes the surface is extremely coarse (fabric C), sometimes it is slightly smoother and the tempering material does not project; it feels rather sandy or dusty (fabric D). Fabric D is generally fired harder than fabric C which, to judge from the black and glittering grit in some sherds, certainly contains material manufactured

149 Modderman 1949; 1951.

150 Wageningen: Van Es 1964, 266–70; Walsum: Stampfuss 1939, 52; Frankfurt: Stamm 1962, 133–8.

151 Böhner 1958, 37.

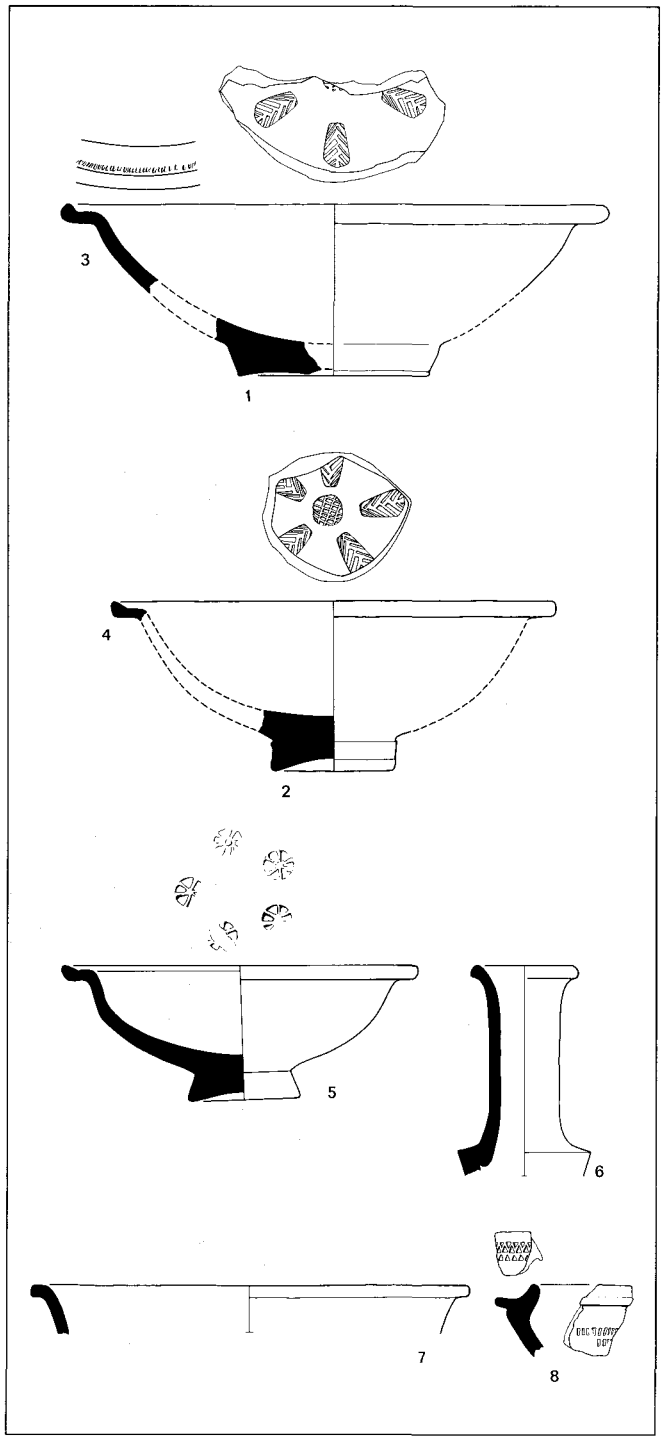


Fig. 43 Unusual forms of probably 5th-century terra nigra from Driel-Oldenhof (site 123, nos. 1–4, 6–8) and Rhenen (site 12, no. 5).

in Mayen. Some sherds in fabric D may be Trier products.

Special forms: discussion

1 Among the material from Driel are two bases of vessels with a *Fussplatte*, decorated with stamps on the inside (fig. 43, 1–2 and 44) and manufactured in fabric A; no direct parallels have been published. Fortunately, the cemetery of Rhenen (site 12) contained a bowl or dish (in fabric B) with a similar foot which, although severely chipped off on the inside, still showed a comparable decorative pattern[152] (fig. 43, 5). Because two rim-sherds (fig. 43, 3 and 4) identical with that of the bowl from Rhenen, were among the material from Driel (both in fabric A), these have been used to reconstruct two bowls, although there is no real evidence that the combined sherds originally came from the same vessels. That this reconstruction is probably correct is indicated by yet another identical bowl from grave no. 163 in the recently discovered cemetry of Elst (Utr.), only a few kilometres west of Rhenen.[153]

It can be concluded, therefore, that all these examples constitute a bowl type, characterized by a flat, notched rim, slightly convex wall, and *Fussplatte*. Rouletting is sometimes present on the rims (one example from Driel and the one from Elst) and the floors of the bowls are decorated with a central stamp and surrounded by five or six other stamps. All this is, of course, highly unusual for Merovingian pottery, although both the rouletting and the stamp-types employed are not exceptional. Both the central grille and the palm-motifs on the material from Driel as well as the rosettes on the bowls from Rhenen[154] and Elst may be encountered on other types of Merovingian ceramics.

The form of the bowls, however, is Roman. It seems to be derived from 2nd-century sigillata and is a well-known late-Roman sigillata form. Its development is discussed by Chenet.[155] It was apparently influenced by

152 I am grateful to J. Ypey for drawing my attention to this find and for his permission to publish it. Unfortunately, it is a stray find from the cemetery.

153 I thank C. Blommesteijn for drawing my attention to this new find, the excavator, W.J. van Tent, for his permission to use the dating evidence contained in the grave and its position within the cemetery, and W.A. van Es for providing that evidence.

154 The central stamp on this bowl has become completely unrecognizable because of the damaged surface.

155 Chenet 1941, 66–7, type 314.

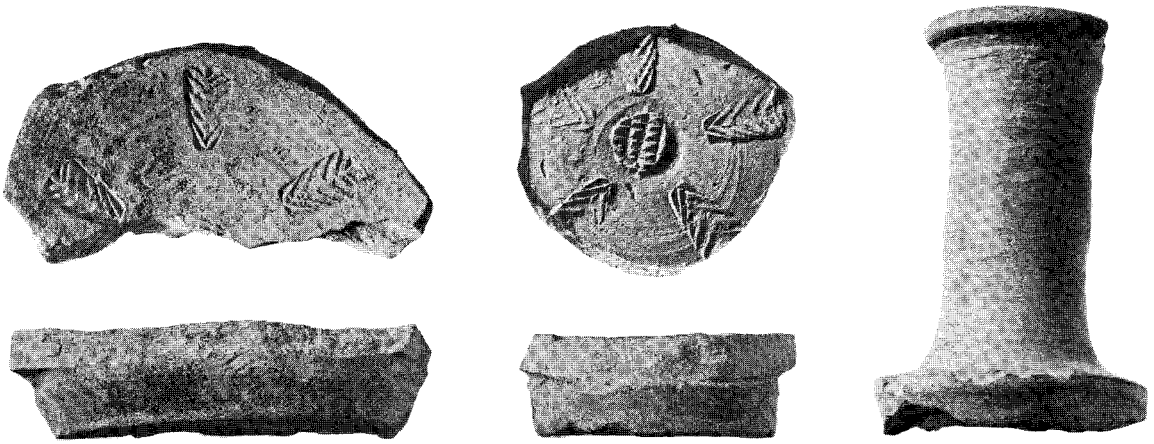


Fig. 44 Decorated terra nigra bowls or dishes and the neck of an amphora or bottle from Driel-Oldenhof (site 123). Scale 1:2.

metalware.[156] Late-Roman forms are, for example, Alzei 13, Chenet 314b, Trier-*Umbaukeramik* type 7b, and Barbarathermen type 7 in terra sigillata, and Barbarathermen type 21 in red-brown slip ware. The bowl Böhner A1,[157] in red slip ware, seems to be a Merovingian continuation of this type.[158] The wall of this vessel is, however, no longer convex from rim to base: it has developed into a *Knickwandschüssel*. A more obvious parallel seems to be provided by the dishes Rübenach A1a/A2a[159] with white painted decoration.

The closest parallels to the terra nigra bowls from Rhenen, Elst, and Driel are to be found elsewhere. Similar forms and decorations are encountered in the African red slip ware. But in view of the ware, especially the *terra sigillata paléochrétienne grise* as described by Rigoir,[160] which is of Gaulish origin, seems to be related. Our bowls conform almost exactly to Rigoir's type 3, which is directly related to forms of *terra sigillata chiara B* and *lucente*.[161] Decoration of the rim with rouletting occurs on this form, as well as stamped decorations.[162] Notably the floors of the related dishes Rigior type 1 and 2 show a decoration which is strikingly similar to our bowls.[163] In particular, a central round stamp surrounded by palm-motifs, as on the bowls from Driel, seems to be a popular decoration. Closely related bowls have also been found at several sites in Switzerland, such as Chevrens, Aire-la-Ville, and Yverdon.[164]

Notwithstanding all these similarities in form, fabric, and decoration, the bowls from the river area cannot be attributed to any of the groups of *t.s. grise* from France

156 Unverzagt 1919, 14, *Abb.* 15.
157 Böhner 1958, 35 and *Taf.* 1,1.
158 Cf. Hussong/Cüppers 1972, 91.
159 Neuffer-Müller/Ament 1973, 29–30. See also Neuffer-Müller 1962, 182–3. The type is, however, restricted to the Middle-Rhine area.
160 Rigoir 1968.
161 Lamboglia 1958; Rigoir 1968, 200; Hayes 1972, 402.
162 It should be noted that wheel-rouletted chequered decoration is also present on some late-Roman sigillata types Alzei 13, see Unverzagt 1919, 13, *Abb.* 12.
163 See e.g. Rigoir 1968, Pl. III-V; Rigoir/Rigoir/Meffre 1973, Pl. I-IV.
164 Rigoir/Rigoir 1970, 109–12.

or Switzerland. Although some of the Swiss bowls also have a *Fussplatte*, Rigoir's type 3 bowls normally have a foot-ring. Also, *t.s. grise* is essentially a (black) slip ware, which is not the case with the material from Driel.
Furthermore, as already noted above, the stamps fall into the normal range of Merovingian stamp-types and they are not exact parallels of the more elaborate stamps common on the *t.s. grise*. For the moment it does, however, seem probable that we are dealing with a local Lower-Rhenish group of pottery, manufactured by Frankish potters, but closely related to the *t.s. grise*. Such groups have also been recognized elsewhere,[165] and are not incorporated in the group of *Derivées des sigillées paléochrétiennes* proper.
As far as the dating of our bowls is concerned, the direct link with Roman forms and close correspondence to the Gaulish *t.s. grise* suggest a date in the 5th century. This would be in agreement with the general datings of the sites in Rhenen and Driel, and is confirmed by the associated finds from grave 163 in Elst. This contained, apart from the bowl and a number of smaller iron and bronze objects, a hand-made, pear-shaped Anglo-Saxon vase dated to the 4th/5th centuries and a not precisely datable franciska. Moreover, grave 163 was cut by another – and therefore (probably considerably) later – grave, which contained a bionical pot Böhner type B3b, datable to the 7th or even to the 6th centuries.
2 A second, highly unusual form from Driel (figs. 43, 6 and 44) is a neck without handles, having a bent-out rim. The neck has a very sharp, angular transition to the narrow body. It is manufactured in fabric A. Unfortunately, the complete form cannot be reconstructed, but it looks if it might be a small, pointed amphora rather than a bottle or flagon. Comparable forms seem to be lacking, although the shape of the neck is similar to the bottles Böhner C6 and Rigoir type 27.[166] This form undoubtedly belongs to the same group of pottery as the bowls described above. Because of the fabric, it should probably be dated to the same period.
3 The rim-fragment on fig. 43, 7 is also manufactured in fabric A. It belongs to an open form (diameter 23 cm) with slightly concave wall and small, flat rim. It seems probable that the fragment belongs to a biconical bowl which is typologically somewhere between the late-Roman terra nigra forms Alzei 24–26 and the earliest Merovingian biconical bowls with concave upper wall (Böhner B6),[167] which are discussed below. Consequently, a dating in the 5th century is probable.
4 Rim-fragment with flange in fabric A (fig. 43, 8), decorated with rouletting. The rouletting on the upper part of the rim is probably feather-rouletting. (*Dekoration mit dem federnden Blättchen, décoration au guillochis*[168]), while the wall is decorated by means of a wheel. It is not certain whether this rim belongs to a mortarium or to a lid. It conforms exactly to the Merovingian mortaria from Trier[169] which are, however, manufactured in a different ware and some of which could just as well be lids, as already noted by Hüssong. On the other hand, our rim is also very similar to Rigoir's *t.s. grise* lid type 31[170] (his mortaria type 29 are never decorated). But this comparison may carry the argument too far, because the type as such is well represented in collections of Merovingian material and also occurs in late-Roman terra sigillata and red-brown slip ware (cf. the lid-fragment on fig. 34, 7). It could be of 5th-century date, but, according to Hüssong/Cüppers, typologically related forms also occur in the succeeding centuries. The latest development is probably represented by the Dorestad mortarium type WXE.[171]
5 Rim-sherds of a number of dishes are illustrated on fig. 45. Of these, nos. 1–6 are of special interest because they are manufactured in fabric A (nos. 1–3) and fabric B (nos. 4–6). Typologically, they seem to be related to late-Roman sigillata and imitation sigillata, as well as to Merovingian coarse ware dishes. A relation can be proposed for no. 1 and probably no. 2 with Alzei type 11 and especially with later developments in red-brown or red slip ware.[172] Similar rim-profiles in a reduced fabric are

165 Pers. comm. Mrs J. Rigoir. See e.g. Ferdière/Rigoir/Rigoir 1972, 309 ff.
166 Böhner 1958, 48; Rigoir 1968, 209; see also several coarse ware bottles from Junkersdorf (La Baume 1967, 67–8) and Krefeld-Gellep (Pirling 1966, type 174). The Merovingian bottles are typologically related to glass bottles. The Merovingian types cited above are clearly related to the globular bottle with funnel shaped neck, Haberey 1942, type 7. For our specimen, such a prototype does not seem to be readily available.
167 See e.g. Böhner 1949, 187–9 and 1956, 110–13 for a discussion and several illustrations of various vessels. A recent summary of late-Roman types is provided by Koch 1981.
168 See e.g. Von Petrikovits 1972, 139–40 and Rigoir 1968, 193–6 for a description of the technique.
169 Hussong/Cüppers 1972, 107–9, type 8c.
170 Rigoir 1968, Pl. XX.
171 Van Es/Verwers 1980, 102–4.
172 See paragraph 6.2.2 and fig. 34, 1–3.

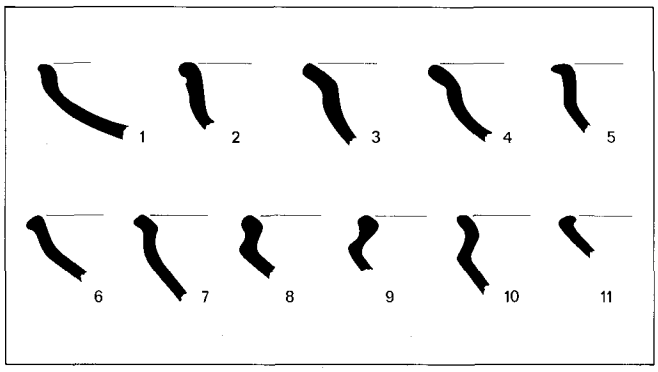


Fig. 45 Early-Medieval dishes from Driel-Oldenhof (site 123).

also regarded by Rigoir and Meffre as possible *t.s. grise* forms.[173] Apparently, such dishes do not occur in Merovingian cemeteries.

The dish on fig. 45, 3, which is not as carefully polished as is normally the case with fabric A, and the dishes nos. 4–5 in fabric B could be related to the sigillata dish Chenet 313,[174] although they have everted rather than flat rims. An intermediate step is, however, provided by the sigillata dish type 2 from the Trier *Umbaukeramik*. Whether the typological succession suggested by the development from 1 to 6 is correct, remains to be proved.[175] It does, however, show a development towards the dishes 7–10, which are manufactured in a coarse fabric and would seem to fall into the normal range of Merovingian coarse ware dishes. No. 7 (fabric C) is typologically still identical with the dishes in fabric B, but no. 8 (in fabric D) and 9–10 (in fabric C) can be identified as Böhner's types D 16a and D 16c, respectively. As far as the dating is concerned, it seems fairly certain that at least nos. 1–2 should, in view of their fabric and form, be dated to the 5th century. Nos. 9–10 could be as late as Böhner's *Stufe* IV (7th century), although, according to La Baume,[176] they should be placed in the 6th century because of their sharp profile. The conical dish on fig. 45, 11 is manufactured in fabric D. It can be identified as Böhner's type D 19, which is dated to the end of 6th century and to the 7th century. According to Pirling,[177] the dish occurs during the entire 6th century and was already in use during *Stufe* II (AD 450–525).

6 Bowls with concave upper wall. Among the material from Driel are six fragments of wide bowls with concave upper wall (fig. 46). Following Böhner,[178] these bowls are generally considered an intermediate form between the late-Roman terra nigra bowls and Merovingian biconical pots, and are dated to *Stufe* II. This dating is confirmed by several burials in Krefeld-Gellep[179] and, for example, by associations with the terra nigra cups Chenet 342 (which also occur in Driel[180]) in grave 172 in Wageningen, and grave 80 in Schwarzrheindorf.[181] The latter grave should be placed in the early 6th century, but a number of other associations in burials from e.g. Köln-Mungersdorf, Weilbach, Köln-Junkersdorf, and Rübenach have shown that bowls with concave upper walls occur at least during the entire 6th and even in the early 7th centuries.[182] These later bowls differ from the earlier ones whose width exceeds their height, because they are generally higher and less wide.

The bowls from Driel, with rim-diameters between 18 and 22 cm (with the exception of the small bowl fig. 46, 5, which is only 12 cm in diameter), certainly belong to the earlier variety. This is in agreement with the very fine, light-grey of brown-grey terra nigra (fabric A) in which they are manufactured (only fig. 46, 6 is in fabric B). The bowls are indeed very close to the best of late-Roman terra nigra products and it is altogether possible that the rim-fragment no. 5 does not belong to a bowl of the type discussed here but to the late-Roman high-footed cups Chenet 342 (see fig. 39). The fact that this is a difficult matter to decide up on shows that a connection

173 Rigoir/Rigoir/Meffre 1973, Pl. 18.

174 See esp. Unverzagt 1919, *Taf.* 1, 14 and Chenet 1941, type 313.

175 A profile comparable to that of the dishes nos. 3–5 is found in the dish Rübenach type D2 (Neuffer-Müller/Ament 1973, 44) from grave 447, which is dated to the 7th century. No. 6 (a burnt fragment) may even be later than that. If there is indeed a typological development, a straightforward chronological interpretation is not justified.

176 La Baume 1967, 66–7.

177 Pirling 1966, 146, type 172.

178 Böhner 1949, 187–9; 1958, 45, type 36.

179 Pirling 1966, 131–2, type 135.

180 See paragraph 6.2.3 and fig. 39.

181 Van Es 1964, 262 and fig. 80; Böhner 1949, 188.

182 Köln-Mungersdorf: Fremersdorf 1955, *Taf.* 8, 15, 18, and 20; Weilbach: Schoppa 1959, 30–3; Köln-Junkersdorf: La Baume 1967, 59–60; Rübenach: Neuffer-Müller/Ament 1973, 36–8.

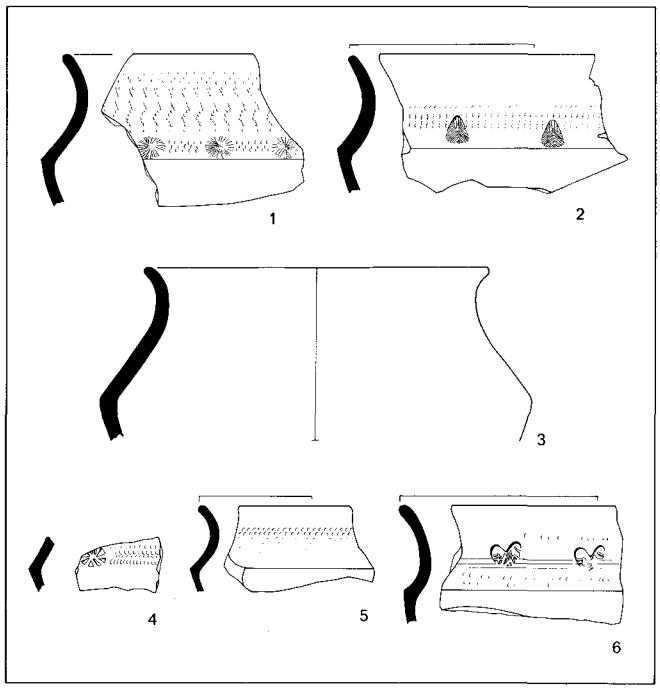


Fig. 46 Terra nigra bowls with concave upper wall from Driel-Oldenhof (site 123).

between the two forms is not completely out of the question.[183]

Without questioning the typological derivation of the bowls from the late-Roman terra nigra bowl Alzei 25, it is worth while to point out the influence of the Chenet 342 cups. The fabrics are identical and the rouletted decoration on the bowls from Driel is also frequently observed on the cups. On the other hand, the bowls with concave upper wall found elsewhere, which are also frequently decorated with stamps, almost invariably have girth-grooves instead of rouletting on the upper wall.[184]

The evidence contained in shape, decoration, and fabric definitely points to a 5th-century date for the bowls from Driel. They undoubtedly belong to the earlier variety of Böhners type B6, but they seem to form a group that could be either chronologically or spatially (or both) different.

Special forms: conclusion

The six types of terra nigra pottery from Driel seem to constitute a group which–as a whole–is different from the pottery it has been compared to. It stands apart because of its fabric (usually fabric A), its forms, and its decoration. As far as its chronological position is concerned, the 5th century is the most plausible period. On the one hand the forms are related to late-Roman forms but on the other they are clearly post-Roman and may be connected to the earliest Merovingian pottery.

The fabric is more 'late-Roman' than 'Merovingian', whereas the decorative elements are either more common to Merovingian (the stamps) or to late-Roman ceramics (feather-rouletting), while wheel-rouletting is used on both. The way of using and combining the decorative elements is again peculiar. In this respect, the admittedly far-fetched references to the Gaulish *t.s. grise*, are not pointless. The similarities in fabric and decoration with this ware are striking, and all the forms have parallels in the *t.s. grise* repertoire,[185] except for the bottle (fig. 43, 6) which is without true parallels anywhere.

However, as already pointed out for the dishes decorated on the inside, the terra nigra pottery from Driel cannot be ascribed to any of the *t.s. grise* groups published so far. Formal similarities may also be attributed to a common sigillata ancestry, but exactly because of this the terra nigra from Driel is certainly among the *derivées des sigillées* in the sense of the concept as used by Rigoir. Undoubtedly, the pottery is also an early Frankish group, for it occurs in a Frankish settlement and Frankish cemeteries. In its forms, fabric, and decoration it would seem to fit perfectly into the transition between *local* late-Roman and *local* Merovingian pottery.

In view of this, the flag of *lower-Rhenish (or Frankish) grey sigillata derivatives* does indeed cover the cargo. The inclusion of the bowls with concave upper wall and, because of the ware in which they were manufactured, of even some of the Chenet 342 cups does not present a problem because all sigillata-derived wares include

183 See Schoppa 1959, 31 and Van Es 1967, 168 , who also consider this possibility.

184 A group of typologically related bowls from Rübenach (Neuffer-Müller/Ament 1973, 38) has a rouletted decoration (probably wheel-, not feather-), but the shape of these high vessels is very different.

185 This is true even for the bowls with concave upper wall, which may be compared to the *t.s. grise* type 22. See for example the bowl from Bossey (Switzerland) in Rigoir/Rigoir 1970, fig. 14.

forms from different sources.[186] As far as the production of this fairly coherent group of Rhenish grey sigillata is concerned, nothing can be said with any certainty until petrological investigation of the sherds is carried out. In view of the restricted distribution on the Lower Rhine, local production in the river area is at least probable.

Biconical pots.

The biconical pots found in Driel are illustrated on fig. 47. Nos. 1–6 are manufactured in fabric B, although 1 and 2 are very close to fabric A. Their tempering does, however, contain occasional coarse stone grit. They should probably be dated to the 6th century and identified with Böhner's type B1a. No. 1 has a sharp wall-profile and no. 2 a very wide mouth (diameter 22 cm). They both have a stamped decoration, the stamp on no. 1 being very similar to, but not completely identical with, the stamp on the bowl with concave upper wall on fig. 46, 2. Nos. 3–6 have ordinary wheel rouletted decorations and are datable to the 6th or, more probably, to the 7th centuries. The wall-sherd illustrated as no. 7 also exhibits wheel-rouletted decoration, but it is manufactured in an oxidized, coarse fabric (fabric C). It clearly belongs to a biconical form. In contrast to the biconical pots in polished, reduced ware, sherds like this have not been found elsewere in the river area.

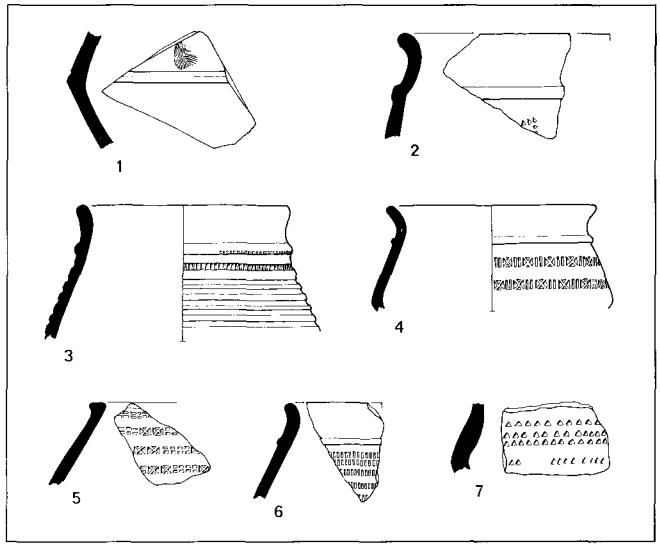


Fig. 47 Merovingian biconical pots from Driel-Oldenhof (site 123).

Coarse pottery

Merovingian coarse pottery was found in large quantities on several sites and also in Driel. Crescent-shaped and cordoned rims have already been discussed (fig. 41 and 42), so that the steep-walled cooking-pots with rims of various shapes, referred to as Böhner type D 9–12, remain to be discussed. They are manufactured in fabrics C and D and range widely in size. Only a small part of this material is of Mayen origin. The fragments on fig. 48 represent a selection from the material from site 123, which gives a good impression of the pottery found on other sites in the area.

Because the generally small fragments from settlements only occasionally allow a more detailed typological attribution, this has not been attempted. The dating may vary from Böhner's *Stufe* II to V, because the early, wide-mouthed vessels and the late, lampion-shaped pots have to be taken as one group.

This pottery is the only Merovingian type which was definitely produced in the river area. Kiln refuse and wasters were found in the village of Ubbergen (site 420) in 1979, together with Carolingian and later material and some stray middle- and late-Roman sherds.[187] The very distinctive, oxidized pottery seems to be typologically best at home at the end of the 7th or early 8th centuries and is also found on other settlements in the river area. Unfortunately, a detailed distribution cannot be provided here, because the inventarisation of finds from other sites had been largely completed by the time the material from Beek was discovered and could be examined.

Although a large amount of the Merovingian coarse ware in the area seems to be imported, as testified by numerous, clearly identifiable sherds from Mayen, there is no reason to assume that the kiln(s?) at Ubbergen is an isolated phenomenon in the area.[188] Very few early-Me-

186 The type 342 cups were, according to Chenet 1941, 91–4, also manufactured *en terre sigillée rouge-orangé*.

187 I thank the discoverers, J.R.A.M. Thijssen and W. de Mul, for drawing my attention to this find and for their kind permission to examine the material in 1980. The site will be published by Mr Thijssen.

188 The material from Cuijk (site 500) also contains clear indications for Merovingian pottery-production (pers. comm. J.R.A.M. Thijssen).

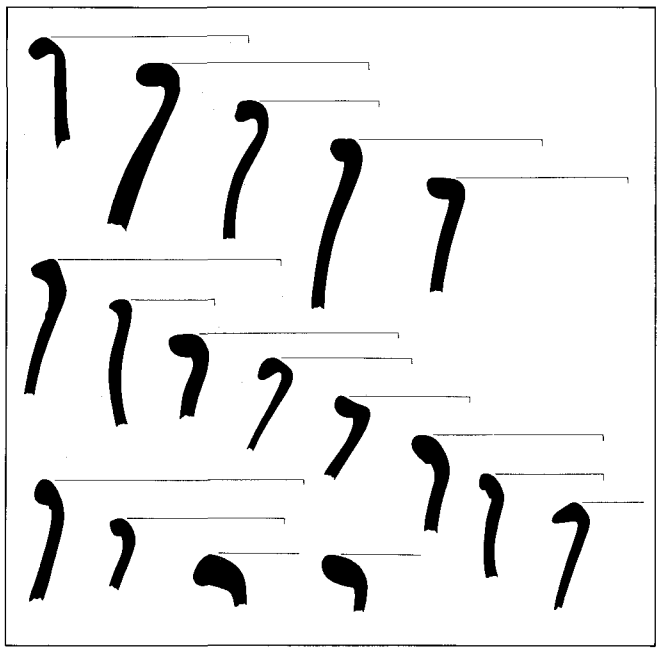


Fig. 48 Rims of Merovingian steep-walled cooking-pots from Driel-Oldenhof (site 123).

dieval sites have actually been excavated and the variety in fabrics is, in fact, larger than the two more or less general fabrics C and D described above. An example is a rather smooth-surfaced and reduced fabric, included in group D, which might be called 'terra nigra-like'. It occurs in small quantities on several sites and could be either a local or an imported product. As long as detailed typological and, as mentioned several times before, petrological studies have not been carried out, it seems rather pointless to engage in speculation on this point. After all, the present evidence is based on nothing more than the visual inspection of sherds.

6.3 HAND-MADE POTTERY

The hand-made pottery used in the river area between 250 BC and AD 750, is a major problem. Apart from the occasional 'Roman' hand-made pottery like cork-urns, this category consists almost entirely of native products, formerly often termed 'Batavian'. This is evidently a misnomer, because the pottery was used in the river area long before any Batavians lived there. In recent literature, therefore, it is replaced by the simple 'native', although some of it may have been imported from elsewhere as in the case of *streepband* and 'coastal' pottery.[189] The native pottery has been bothering archaeologists working in the area for quite some time, because it proved to be very difficult to devise a typochronological classification for it. Apart from an initial typological approach by Van Giffen and Vermeulen,[190] most subsequent writers have left the matter severely alone,[191] mainly because of the lack of clearly stratified deposits and associations with other artifacts. Unfortunately, because most of the material employed in the present study is from surface collections, the same has to be done here. A detailed analysis of the native pottery would involve a very time consuming study of the massive amount of surface-finds, with little hope of success.

Typology and chronology

Because of these difficulties, Modderman was forced, in practice, to disregard completely the differences between late-Iron Age and Roman settlements, and had to be content with a differentation based on relative proportions of native v. Roman sherds. Purely native occupation traces might date, therefore, 'to the 1st century BC'.[192] As has been established, for example, at the kiln-site (165) near Bemmel, this sort of pottery was already being made at the very start of the Late-Iron Age as defined today, that is, c. BC 250. The native pottery consists mostly of cooking-pots with an S-shaped profile and low rims turned outwards, a few dishes, and bowls, plus occasionally special forms. Some of these forms are rather long-lived and typologically so uncharacteristic that, for all we know today, isolated sherds could just as well be Middle- or even Early-Iron Age. In collections, however, the risk of error is limited, because of the presence of pottery which is characteristic for those periods, for example, the middle-Iron Age Marne ceramics with sharp profiles.

Although nothing substantial has been added to the study of late-Iron Age and Roman Period native ceram-

189 *Streepband* pottery is mentioned by Van Tent 1978, 219–20. The so-called coastal pottery is discussed below, 182.

190 Van Giffen/Evelein 1928, 26; Vermeulen 1932. See also Hulst 1981, 355–7, for a historical overview.

191 For example, Modderman 1949, 67; Braat 1949, 32. But see Stuart 1963 (types 401–403).

192 Modderman 1949, 68.

ics since Modderman's work after World War II and up to 1978, the recording of the material fortunately did not have to proceed without guidelines. A preliminary inventory of pottery types has been compiled by Bloemers and Hulst;[193] in it, a few attributes of chronological significance have been isolated, and these have been employed here in the determinations.[194]

There are at least three ways in which late-Iron Age native pottery differs from that manufactured during the Roman Period. Two of these are confirmed by Van Tent in his recent analysis of material from Jutphaas, at the moment the only really useful modern study of this subject.[195]

The first chronologically significant attribute is decoration of the top of the rim with finger-tip impressions, which seems to be especially characteristic for the Iron Age. In fact, all decoration seems to diminish from the 1st century AD onwards, although Van Tent was able to show that this may not be true for certain specific decorations.[196]

A second attribute is the faceted rim, which is regarded by Van Tent[197] and Bloemers/Hulst (based on numerous associations with 1st-century Roman pottery) as an exclusively Roman Period phenomenon. It can be employed, therefore, to date sites to the Roman Period even in the absence of any Roman ceramics. The third attribute is tempering with sand, the grains projecting from the surface and causing a sandpaper-effect. This generally hard-fired fabric became relatively frequent only during the Roman Period. Sand-tempered pottery is extremely rare in Jutphaas, so that no confirmation can be expected from there. On the other hand, the Jutphaas material indicates that organic tempering is almost completely restricted to the Roman Period. If this were valid for the entire eastern river area, it would provide a useful fourth discriminating criterion. Moreover, as appears from the summary of all attributes of native pottery from Jutphaas,[198] further research may well produce even more criteria, or at least reveal tendencies in the development from the Late-Iron Age to the Roman Period.

The latter point is stressed by Van Tent and Bloemers/Hulst. It means that except in the case of faceted rims (and, possibly, organic and sand tempering), the native material itself is reliable dating evidence only in those cases where a large sherd collection is available, and even then the outcome will often not be entirely conclusive, especially when the occupation period of a site covers a few centuries BC as well as AD.

In one way, all this is hardly surprising: it would be rather foolish to assume that native pot-making traditions would change suddenly and drastically immediately after the start of the Roman occupation. These are so different from the Roman and Gallo-Belgic methods of manufacturing pottery that they are more likely either to have been continued in the same way with some gradual change over time or to have been abandoned altogether as wheel-turned pottery became available everywhere at a reasonable price. Unfortunately, it is not easy to determine exactly what happened in the eastern river area.

Bloemers established that types of native ceramics found in the Roman Period settlement at Rijswijk continued to be used throughout the entire period of occupation into the 3rd century.[199] At least one type (Rijswijk type IV) is especially common in the (3rd-century) period III of the settlement. The fact that the pottery itself is clearly related to that of the northern Dutch coastal area and is essentially unrelated to the native pottery from the river area, is rather irrelevant. The important fact here is that in a strongly acculturated settlement, situated only a few kilometres from the civitas capital, native pottery continued to be used.

Although there are indications that native pottery ceased to be used in the eastern river area in the 2nd century,[200] the evidence from Rijswijk makes it difficult to believe this was a universal phenomenon. There are a number of sites which have so far yielded no, or hardly any, native

193 This has subsequently been developed further and will be published by Bloemers/Hulst (in prep.). See also Hulst 1981.

194 I thank R.S. Hulst for discussing with me his ideas on the chronology and typology of native pottery from the river area based on unpublished material from his excavations, and for his kind permission to use whatever data he had available.

195 Van Tent 1978. Unfortunately, this article appeared only in 1981 and its results could not be employed in the data collection. The fact that they are in accordance with the information I obtained previously from Hulst provides a wider data base and gives strong support to the tentative conclusions reached by Van Tent. Although Jutphaas is situated rather far west of our area, many properties of the pottery apparently remain the same.

196 *Op. cit.*, 220.

197 *Op. cit.*, 221–2. See fig. 21 (rim form 5). Apparently rims flattened on the side (form 3) should be added.

198 *Op. cit.*, 237, table 27.

199 Bloemers 1978a, 73–4, 190–2.

200 Pers. comm. R.S. Hulst. The sites in Jutphaas did not survive into the 3rd century. See also Hulst 1981.

ceramics,[201] and it is obvious from the early Nijmegen sites and the fort in Meinerswijk (126) that the Roman army had only little use for it. It may also well be that the inhabitants of some strongly acculturated settlements in the eastern river area actually stopped using native pottery in archaeologically detectable quantities. But all this does not constitute definite proof, which can only be obtained by the excavation of a number of sites which were less strongly acculturated (which do not, for example, have stone buildings) and were continuously occupied to at least the middle of the 3rd century.

For the moment, the following conclusions seem to be justified:

1 There was a general decline of native ceramics in favour of wheel-turned pottery throughout the Middle-Roman Period which became marked in the 2nd century. The rate of decline probably increased rapidly from the middle of the 2nd or even the end of the 1st century onwards.[202]

2 Native ware may well have disappeared completely on some sites around that time.

3 At some settlements, notably military ones but possibly also a few others which are closely related to the military forces, it was never used in any quantity at all.

As far as the Late-Roman and Merovingian Periods are concerned, information on locally made or imported hand-made pottery hardly exists. This is mainly due to the almost complete lack of excavations of non-military settlements dating to these periods and, moreover, to the absence of detailed reports on the few sites that have been excavated. These are Ede-Veldhuizen (1) and Bennekom-Achterstraat (7) north of the Rhine,[203] and Beuningen-De Tinnegieter (246) and Millingen-De Paverskamp (444) in the river area.

The hand-made pottery from surface collections has been checked for the presence of late-Roman forms comparable to those found in the Veluwe, the northern provinces of Overijssel and Drenthe, and western Germany.[204] These forms are indeed present, but in all cases they are those which were also used in earlier periods. As far as Merovingian hand-made pottery is concerned, the situation is roughly similar. The characteristic forms tend to continue into the Carolingian Period. Although the tempering of much of this Merovingian pottery is different because of the larger stone grit it contains, this is not always the case and some forms may be confused typologically with earlier forms.[205] Only in the larger collections can the hand-made Merovingian (or probably Merovingian) pottery be properly isolated. An example in point is Driel-Oldenhof (123), which yielded rim-sherds of the typical 'bowl with short neck', which occurs from the 5th(?) to the 8th (or even 9th?) centuries.[206] The only decorated example is illustrated on fig. 49.

Implications

The hand-made pottery used in the river area during the Merovingian Period and possibly also during (part of) the Late-Roman Period is different from that in earlier periods, although changes are difficult to detect in surface collections. This fact is, of course, easily explained by historical events, such as the arrival of German immigrants in the region.

In this respect, attention should be drawn again to what

201 A phenomenon already observed by Modderman, e.g. 1949, 90 (table); see also Van Tent 1978, 214 and Bloemers 1978a, Appendix 8.

202 This is the reason why in fig. 23, presenting the chronology of the sites, those with only native pottery stop at the somewhat arbitrary point of AD 100. It is most unlikely that 2nd-century sites would not have yielded at least one sherd of Roman pottery. In fact, the presence of Roman pottery is considered a *sine qua non* for Middle-Roman Period sites, so that sites with only native ceramics have been omitted from Appendix 3, although some of them may have continued for a few decades into IB. There is, however, at this moment, no way of reaching a completely satisfying conclusion on this problem. Hulst (1981, 362) states that at least in the eastern river area proper (between Meuse and Rhine), native pottery did not survive into the 2nd century.

203 The hand-made pottery from Rhenen-Utrechtsestraatweg (11), which may include late-Roman forms, was published by Van Es 1968.

204 These are discussed by Von Uslar 1938 and Van Es 1967. The overview presented by Van Es/Verlinde 1977 of the material from Overijssel is especially useful.

205 The bowl with inverted rim Van Es/Verlinde 1977, type 1.4.1 is an example of this. When the fabric is not characteristic and no decoration is preserved, a dating seems to be a doubtful enterprise. The same is true for the hand-made bowls from Den Burg, discussed by Van Es 1969 (if these forms should turn out to have been used in the river area).

206 Van Es/Verlinde 1977, 62–7, type 1.4.2. See also Van Es 1969.

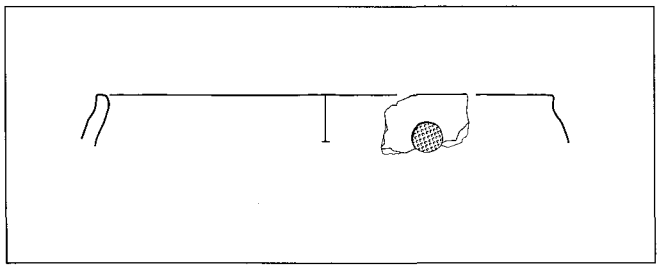


Fig. 49 Decorated 'bowl with short neck', belonging to the early-Medieval native pottery from Driel-Oldenhof (site 123).

was said above about the transition from Late-Iron Age to Roman Period hand-made pottery. In that case, there is a continuous tradition with only a few changes which are so difficult to detect that differences have only recently been recognized and these have to be interpreted with extreme caution. The only chronological groups that can be somewhat differentiated are the 'Late Iron Age' pottery on the one hand, and the 'Roman Period' material on the other, with some very gradual change in between. It is worth noting that there is no clearly observable and more or less sudden change in the 1st century BC, when another immigration must have occurred, namely, the arrival of the Batavians.

It could be argued that that change still awaits detection (Hulst (1981) leaves this problem undecided), but this seems hardly likely. Except, of course, when the pottery of the Batavians closely resembled that used by the inhabitants of the river area. But there are no reasons to assume such a close relationship. On the contrary, it seems fairly certain that the Batavians were originally closely connected to or indeed were part of the Chatti,[207] who, at least from the last decade BC onwards, lived in northern Hessen.[208] Mildenberger has published the pottery from a large number of sites in that area, which in general fits into Von Uslars's typology.[209] This pottery group is, as a whole, different from the native pottery in the river area. Although these arguments do not constitute incontrovertible evidence, it is very probable that the arrival of the Batavians in the river area did not cause the change in the native pottery production which might have been expected in view of their origin. This does, of course, have implications for the interpretation of the immigration of the Batavians, which will be discussed in a later chapter (see also p. 23–4). It also underlines the fact that the native pottery should not be called 'Batavian'.

Production

Most of the hand-made pottery was presumably locally made in or near the settlements. The kiln from Bemmel (165) may have been rather isolated, but on the other hand a probable kiln-pit from Ressen-De Woerdt (151) was located in the immediate vicinity of occupation traces.[210] The same is true for the middle-Iron Age kiln from Duiven (196).The differences between the two kilns may also be used to illustrate aspects of the production proces.

The 'kiln' from site 151 is actually nothing more than a shallow pit. The pottery was put into this pit and surrounded by fuel, a method requiring no real investment in a proper kiln. The low level of technical sophistication in combination with the proximity to the occupation traces and the small size of the pit make it very likely that we are dealing here with simple household production of pottery.[211] On the other hand, the probably more or less isolated kiln from site 165 was a true kiln. Fragments of a perforated oven-floor were discovered, separating the fire- from the stacking-chambers of an updraught kiln. Although the kiln was rather small, it represents a certain amount of labour-investment and, moreover, especially in view of its early date, a fairly high level of technical sophistication. It is reasonable, therefore, to assume specialist or at least semi-specialist activity in this case, which points to low-level industrial production. The near-by settlements are not particularly large, and it seems doubtful whether they could support a resident, specialized potter. Therefore, we may be dealing with a local semi-specialist or with an itinerant professional potter.

These two examples indicate that the production of local pottery may have been rather complicated and that at least some of the methods used trascend the level of simple household production.[212] At the moment, too little is

207 Tacitus, *Germ.* 29 and *Hist.* IV, 12. See also Sprey 1953, 16–9.

208 Nierhaus 1966, 227; Mildenberger 1972a, 102–3.

209 Mildenberger, *op. cit.*, 80 ff.

210 Braat 1949, *kaart* I and *afb.* 2–3.

211 See e.g. Van der Leeuw 1976, 389–404 for a discussion of the different systems of pottery-production.

212 In this respect, it is worth while also mentioning a number of large pits dated to the (Middle?) Iron Age which were filled with wattle-and-daub fragments, excavated at site 417 (Bloemers 1978b, 251). It is not clear whether these could be pottery kilns.

known of the pot-making techniques to go into further detail.

Apart from the two kiln sites mentioned above, evidence for pottery production was also found at other sites, either in the form of wasters, (probable) pottery slags, or (probable) parts of kilns. Except for the first category, these cannot be definitely attributed to the production of native pottery because they could be related to Roman or even later kilns as well. Probable wasters of hand-made pottery have been found at sites 212, 222, 301, 341, and presumably 479.

As already mentioned, most of the native pottery consists of cooking-pots with more or less S-shaped profiles and small rims, generally turned outwards.[213] Other forms, such as small cups and dishes, also occur, and occasionally vessels with handles and, for example, cheese presses (143, 234). Sometimes forms are encountered which seem to be hand-made imitations of Roman or Gallo-Belgic forms,[214] but these occur only very rarely. The only exception could be native imitations of the Roman (but also hand-made!) cork-urns with inturned rims, examples of which are fairly regularly present in collections, although in very small numbers. Their fabric is so different from the characteristic fabric of true 'cork-urns' that the margin of error is very small. On the other hand, they usually have a thickened or bead rim, but unless tempering with coarse stone grit or stamp decoration are present, confusion with Early-Medieval bowls with intuned rims does seem to be possible.[215]

Very little evidence is available concerning the importation of hand-made pottery. Somewhat 'foreign'-looking forms are encountered sporadically, such as rims which may have belonged to Von Uslars type 2 and possible instances of *streepband* pottery.

A more positively definable class of imported hand-made ceramics is the so-called coastal pottery, recently published by Bloemers and by Van den Broeke.[216] Sherds of this pottery are generally fairly thick but very light, which is due to their heavy organic tempering. They are always fired in an oxidizing atmosphere and usually have a very soft surface which feels dusty. The cylindrical forms described by Bloemers, which belong to the Roman Period, were connected to the western Dutch and Belgian coastal areas and are interpreted as salt-containers. Van den Broeke mentions similar fabrics but different forms, such as dishes and small cylinders, dating to the Early- and Middle-Iron Age. The coastal origin was firmly established in this case because diatom-analysis of a few sherds clearly showed that they were manufactured from marine clays, which did not apply to any of the other sherds analyzed.[217]

As both Bloemers and Van den Broeke have noticed, it is very likely that this pottery served as containers for salt, imported from the coast. The larger Roman containers may reflect a technical advance in the salt-making industry, but they could also point to an increased demand, a different distribution system and, of course, other production sites. Sherds of this pottery are found on 5–10% of the settlement sites throughout the area, and include the Early- and Middle-Iron Age as well as the Roman Period containers.

6.4 BUILDING-MATERIAL

6.4.1 Brick

Fragments of brick are present at most Roman settlement sites, usually in the form of roof-tiles (*tegulae* and *imbrices*), but other tiles (*lateres*) also occur frequently. Brick is encountered in such quantities that it is always underrepresented in collections because it is too troublesome to collect, clean, and store all the material available at a site.

For the same reason, brick may well be present on some sites for which none is recorded in the catalogue. On the other hand, it is certain that not all settlements had brick. It is possible to calculate a percentage of 'no-brick' sites from the catalogue, but that would not be a reliable figure because of the collection problem. The figure would certainly be too high, but the margin of error cannot be determined. In this respect it may be very significant that among the settlements yielding fairly large amounts of material, only very few had no brick. Good examples of this situation are site 143, with no brick at all, and site 93. On the latter settlement (Heteren-Het Lage Land), only two or three small pieces of brick were found in the 1.35 ha that were completely excavated.[218] In this case, the absence of brick may partly

213 See Van Tent 1978, 223–34.
214 A.o. site 118. See also Van Tent 1978, 232, type V A.
215 See e.g. Van Es/Verlinde 1977, 63, fig. 62, especially 41.
216 Bloemers 1978a, 372–3 and 387–8: *Schwach gebrannte zylinderförmige Töpfe;* Van den Broeke 1980, 45–6, 54–6.
217 Van den Broeke 1980, 46; Jansma 1980.
218 Pers. comm. R. S. Hulst.

be explained by the relatively early end of the occupation, probably in IIb. As is discussed below, it is reasonable to assume that brick became more widely available in the course of time, and that on settlements which were deserted at a relatively early date, brick had conceivably not yet been introduced.

As far as the general dating of brick is concerned, one may safely assume that it was virtually absent in the eastern river area during the entire Early-Roman Period. Even in Nijmegen, evidence for the use of brick in IA is scanty, although fragments have been discovered in Claudian refuse-pits during recent (1982–1983) ROB-excavations at site 403. There are also a few early (pre-Flavian) stamps (FRA and *legio XV Primigenia*), but these can just as well be dated to Ic or even later.[219] In Elst (105), the earliest brick belongs to temple I which is dated to between ± AD 50 and 69/70,[220] and at the site of the early fort in Meinerswijk the first brick occured in the layers belonging to the third period, which is dated to after AD 70.

It is clear that brick was not introduced in the area until the very end of the Early-Roman Period, during the early years of Claudius' reign. Its distribution is military, at least until AD 70, and probably all material was imported.[221] After the start of the tile-works of the 10th Legion at De Holdeurn (433) shortly after AD 70, most of the brick was produced locally and at least initially for military use only. Because of the lack of stratified sites, it is not possible to pin-point when the use of brick began at non-military settlements, and no published excavation data are available which can be used to that purpose. However, the fact that several settlements (43, 59, 214, 341, 377, 395, and 499) yielded stamped tiles from the 10th Legion indicates that at least during the Flavian period, brick started to be used throughout the area. Significantly, all but one (395) of these settlements also had stone building(s) at some point during their occupation. This may indeed be an indication of a process of distribution whereby some (richer) settlements were the first to have houses with roof-tiles, followed by a great number of others during the 2nd century, while in some settlements brick was never used in any quantity at all. This means that at least some people could not afford roof-tiles, but other explanations are also conceivable. In this respect it should be noted that the absence of tiles from Heteren-Het Lage Land may be explained chronologically (tiles not yet available for everyone) but also in another way. The settlement is of the 'post-hole swarm' type (to be discussed later); it seems reasonable to assume that houses, or rather huts, which must have been so irregularly built that their plans cannot be seen clearly after excavation, could not have carried a roof with heavy tiles.

As far a the production of brick is concerned, private tile-works seem to have played only a minor role in supplying the area, because civil tile-stamps are virtually absent (see fig. 50). On the other hand, these stamps are more common in southern areas.[222] The frequent occurrence of military tile-stamps in the eastern river area seems to indicate that it may well have been supplied entirely or primarily by the Holdeurn tile-factory,[223] supplemented only occasionally by (imported?) products of smaller civil tile-works (especially along and south of the Meuse?) Although the presence of small civil tile-works in the river area has never been established, it cannot be excluded.

Apart from the *tegulae*, *imbrices*, and *lateres* to which the foregoing discussion pertains, other forms of brick are fairly restricted. Special products, like *tegulae mammatae* and large or small tubes are almost absent outside Nijmegen. Those related to the heating system only occur on sites with stone building(s).[224] *Tubuli* have been recorded for sites 1, 59, 99, 105, 117, 214, 239, 250, 315, 355, 383 (in a grave!), 439, and 441. *Hypocaust* tiles for sites 105, 135, 182, 214, 222, 250, 315, 355, 377, 424, and 508. Secondarily used brick[225] in *opus signinum* floors is also restricted to stone buildings. It occurs on sites 59, 105, 126, 214, 219, 222, 239, 377, and 500.

6.4.2 Stone

In contrast to brick, almost all stone for building purposes had to be imported into the river area. It is diffi-

219 Because of possible secondary use in the Flavian fort. See Bogaers 1965, 13 and Haalebos 1977, 179.

220 Bogaers 1955, 174–9.

221 E.g., from the military tile-works of the *tegularia Transrhenana* (near Xanten?). See Bogaers 1969, 31, note 30; Rüger 1968, 60 ff. and Haalebos 1977, 178–9.

222 For example, the excavation of a Roman villa in Maasbracht in 1982 (Willems 1982, 15–7) yielded seventeen stamps CTEC and one AAF.

223 For a discussion of the problems connected with this proposition see paragraph 6.5.1.

224 The presence of tubuli on site 1 is the only exception.

225 For objects manufactured from brick see paragraph 6.6.4.

cult to determine where (partly) stone buildings were first erected, but it is certain that the initial use of imported stone was entirely a military affair.[226] The most frequently used materials are tuff, limestone, and also quartzitic sandstones.

The most likely place of origin for tuff is the Eifel, especially the Brohtal and the Pellenz. The winning of tuff is generally assumed to have been started there by military personnel under Claudius, only shortly before AD 50.[227] Production and transport remained exclusively in the hands of the army until the first half of the 2nd century, when private enterprise apparently became possible as well.[228]

Quartzitic sandstone[229] occurs along the Rhine and the Meuse: in the Rhineland secondary mountain chain and in the Ardennes. It was probably imported to the river area by way of both rivers. It was already used in the first temple at Elst (site 105), which implies that it must have been won by c. AD 50. Although Bogaers[230] showed that the walls of both temples were largely constructed with this material, it is more generally used in the foundations, as, for example, in most of the buildings in Nijmegen, and inside walls, in the *opus caementicium*. This would also be in agreement with Von Petrikovits,[231] who observed a clear preference for light-coloured stone.

Limestone was exploited along the upper course of the Moselle. It was already used in the first decades AD, as is testified in the river area by two large blocks of limestone from a column found in Nijmegen, which has been dated to around AD 15.[232] There is ample evidence for the role of the army in the production of limestone.[233]

Other important stone building-materials in the river area include slate and gravel. Slate is found on most sites with stone buildings, but it was probably used as roofing-material. At least one of the cargoes of the Roman barge from Druten-Boldershof (site 211) consisted of slate.[234] Just like sandstone, it probably originated either from the Rhineland secondary mountain chain or from the Ardennes.

Gravel is the only stone building-material which has been won locally.[235] Exploitable deposits occur both in the river area proper and on the ice-pushed ridges. Apart from its function in metalling roads, it was used mainly at the base of foundation trenches, possibly as a cheap replacement for quartzitic sandstone which had to be imported. Examples of this sort of construction have been found at several sites in the eastern river area as well as elsewhere.[236]

Of course, other stone material[237] was also imported but it seems to have been of minor importance. Outside Nijmegen, for which a study of the imported stone is not available, small or even minute quantities of marble (sites 83?, 177, 239, 441, and 451), travertine (site 105), and marl (site 500) have been recorded. A geologist's inspection of the finds from a number of sites may reveal other material and will certainly lead to a more balanced evaluation of the evidence.

In this respect, the collection problem arises once again, because it is fairly certain that the samples of stone in collections are incomplete and biased, if they are present at all. Unfortunately, the catalogue suffers from the same problem. It is strongly biased towards tuff and limestone which are – and in many cases could be – the only materials recorded systematically. This prevents an adequate evaluation of the different sorts of imported stone, but fortunately it does not interfere with the identification of sites with stone buildings. It has been established that in all settlements where the presence of stone buildings could be demonstrated beyond doubt, tuff was always present: there is not one site with certain or even probable presence of stone building without tuff.[238] These observations have caused certain sites, with no in-

226 Cf. Bogaers 1955, 147.

227 See Röder 1957, 228.

228 Bogaers 1955, 147; Röder 1957, 229.

229 Sometimes still referred to as *Grauwacke* or, along the Meuse, as *Naamse hardsteen*.

230 Bogaers 1955, 144.

231 Von Petrikovits 1978, 131.

232 Bloemers/Louwe Kooijmans/Sarfatij 1981, 86. The blocks were found in a secondary 4th-century context, but there is no reason to assume they were imported only in the Late-Roman Period.

233 See e.g., Bogaers 1955, 146–7 and notes 1–5.

234 Hulst/Lehmann 1974, 20–1.

235 An example near Wijk bij Duurstede is mentioned by Van der Voort/Poelman/Van Es 1979, 444–6.

236 In Druten (214), Deest (219), and Nijmegen, but also for example in Zwammerdam *(Nigrum Pullum)*, see Haalebos 1977, 30.

237 For a short overview, see Von Petrikovits 1978, 130–2.

238 There is an occasional site, such as 232 (Ewijk), which definitely has no stone buildings, but still a few pieces of tuff among the finds. These are, however, isolated finds in the vast quantities of other material found during the excavations and therefore neither significant nor comparable to tuff from the surface of other sites.

dications for stone building except some tuff among the find material, to be interpreted as 'settlements, possibly with stone building(s)' on Appendix 3. Tuff is thus considered as a first and principal indicator for stone buildings. It is easily recognizable even to the inexperienced eye, and some of it is probably always included in collections or at least mentioned in a report. A good example is site 85, which was already reported in 1844[239] as containing tuff (*duifsteen*). Modderman's survey in the Betuwe produced no fragments at this site,[240] but later finds by amateur archaeologists again included tuff.

This same site is, however, also an example of the only, but important, trouble with tuff. It was imported during the entire Middle-Roman Period, but new or secondarily used Roman tuff was also employed for building during the Middle Ages. The tuff on sites which also have Carolingian or later finds may therefore be not Roman at all, unless, of course, the presence of a Roman stone building is established by other means. This has caused several settlements to be downgraded on the certain-probable-possible scale. Sites with tuff but without Roman or Merovingian ceramics have, of course, not been incorporated in the catalogue.

Dating

As already mentioned, it is difficult to determine when stone buildings began to be erected. Limestone was imported at a relatively early date, but it did not serve as the main building material then or later. In fact, there are no indications at all for stone buildings in the first half of the 1st century, not even in Nijmegen. The first incidence seems to be the temple in Elst, which was constructed around AD 50, shortly after the army began most of its mining activities. Some building work may also have taken place in *Batavodurum*, the pre-Flavian settlement on site 403,[241] but definite proof is lacking.

Important building work in stone only started after the arrival of the 10th Legion in Nijmegen, shortly after AD 70. Inscriptions record that detachments of this legion worked in the Brohltal[242] (tuff) and in Norroy near Pont-à-Mousson[243] (limestone). The Flavian period 4 of the *castra* (site 412) was partially built in stone. During the last quarter of the 1st century, stone buildings were also erected in the *canabae legionis* (sites 407, 408, and 416), presumably at the so-called commercial ward on the Waal (403) and *Ulpia Noviomagus* (399), and also probably in the fort at Meinerswijk (126). There are no precise data for the other forts in the region, but they may have been built and rebuilt in wood until well into the 2nd century, as shown by excavations in some forts to the west (Valkenburg, Zwammerdam, Utrecht, and Vechten).

However, as the early temple of Elst also testifies, imported stone may have been more easily available in the eastern river area. Stone building which was not directly related to the army commenced in this area at least in IIA, and might therefore be directly linked to the start of non-military exploitation and transport of the necessary raw materials under Traian and Hadrian. Some examples are settlements such as 214, 355, and 377.[244] The inhabitants could apparently afford the stone at a time when most, or at least a considerable part, of the *limes*-forts were still built of wood and earth.

Data detailed enough to establish exactly when stone began to be used on most river-area sites are not available because these generally depend upon excavation. The buildings from Cuijk (500) seem to date from IIB, although a monumental inscription dated to c. AD 100 could be civilian and point to building activities.[245] The datable material from the majority of sites with stone buildings indicates that most of the activities probably took place in the 2nd century.

Outside Nijmegen, tuff has been found on the following sites (mostly settlements): 23, 39, 43, 59, 80, 81, 83, 85, 97?, 99, 105, 109, 117, 126, 135, 182, 194, 212, 214, 219, 220?, 222, 232, 234, 239, 245, 248, 250, 275, 291, 298, 315, 341, 355, 377, 422, 424, 425, 433, 439, 441, 453, 466, 492, 499, 500, 504, 508, and 513. Limestone was recorded for 48?, 59, 105, 109, 126?, 182, 214, 222, 239, 250, 315, 341, 358, 377, 391, 445, 466, 499, and 513.

At only two sites have definitely late-Roman building activities, mostly with secondary material, been demonstrated. These are the burgus near Asperden (site 466) and the fort in Cuyk (site 499). Furthermore, there are indications of late-Roman stone building at the Valkhof fort in Nijmegen (site 403) and the fort at Meinerswijk

239 Janssen 1844, 297–9.
240 Modderman 1949, no. 54.
241 Bogaers 1979a.
242 *CIL* XIII, 7694, 7697–9, 7716–8 = *ER* II, 748, 488, 744–5, 487, 431, and 746 respectively.
243 *CIL* XIII, 4624 = *ER* II, 426.
244 Hulst 1978 and Braat 1934. See also note 256.
245 Bogaers 1966, 67.

(site 126), while this is also probable for a settlement near Wijchen (site 315), the burgus of Heumensoord (site 391), and the fort at Qualburg (site 460). Apart from site 315, there are a few settlements which have yielded enough late-Roman finds to warrant the idea that (re?-) building in stone may have occured there. These are especially nos. 81, 234, and 239, but a number of others could qualify just as well. Until excavations are carried out, no conclusive arguments can be put forward on this matter.

6.5 EPIGRAPHY

Only two restricted categories of epigraphical data are discussed in this paragraph: tile-stamps and graffiti. The inscriptions found in the river area, some containing very important data concerning the region, are incorporated in the catalogue as far as their location is known or could be more or less reconstructed. They are discussed when and where their content is relevant to problems treated elsewhere in this or the subsequent article. Stamps on terra sigillata are recorded in the catalogue and separately in paragraph 6.2.1; those on other pottery may also be found in the catalogue.

6.5.1 Tile-stamps

Apart from a few civil tile-stamps, virtually all of the extraordinarily abundant specimens in the eastern river area are military stamps. In view of their importance, a separate list has been compiled (fig. 50), giving the names of the units (or tileries) whose stamps were found at a particular site.[246] The exact readings and the relevant literature may be found in or by means of the catalogue. Fig. 50 also contains stamps found in the river area which could not be attributed to a particular site but whose findspots are known approximately, as well as stamps on tiles in clearly secondary position, such as Doornenburg. The latter could conceivably have been imported in medieval times from outside the region and can therefore play no role in the discussion.

Virtually all the tile-stamps are from the Holdeurn tileworks. It is clear, therefore, that because not all tiles were stamped and the chance of finding the stamped piece of a broken tile is exceedingly small, these are more or less the tip of the iceberg[247] and indicate a considerable, indeed practically universal, dispersion of Holdeurn-made tiles all over the river area. The evidence strongly suggests that most of the tiles used in the eastern river area were manufactured at De Holdeurn, otherwise more civil stamps should have been found in addition to the single example (CTEC) from site 500 and the *M. Val. San.* stamps from the (military!) sites 391 and 412. This does, however, present a problem. There has been a long discussion about the significance of products from military tileries.[248] One thesis, advanced for the first time in 1893 by G. Wolff, holds that military brick is invariably used for military projects. It has been challenged by Staehelin, MacMullen, Vittinghoff,[249] and others, but it is also supported by the detailed arguments forwarded by, for example, Von Petrikovits, Von Gonzenbach, and Rüger.[250] As far as the proven military brick in the eastern river area is concerned, most of the sites mentioned in fig. 50 are indeed military (126, 391, 403, 412, 433, 460, 466, and 499), probably military (135, 182, 194, and 450), or associated with military affairs.

The latter category comprises settlements or other sites which can be directly related to military settlements (18A?, 38, 39, 407, 408, 'Nijmegen-Hunerberg', 416, and 434), which could conceivably be a military road-station[251] (105, 315, 391, 432, 500, and Hommersum-Viller Mühle[252]), or which may be connected in some way with public building (105, 315 + 316 and 399). Of the remaining sites, nos. 1 and 7, which are definitely not military sites in any way, are situated along an old land-route to the north[253] which must have been under military control and frequently patrolled. Nos. 319, 420,

246 I am grateful to Professor J.E. Bogaers for several important corrections and additions to the original list and to Professor J.H.F. Bloemers for his permission to incorporate unpublished data from the Nijmegen excavations.

247 A very enlightening discussion on this subject is provided by Spitzlberger 1968, 78–9 and 82. The ratio of stamped to unstamped tiles can be between 1:1 and 1:80.

248 For a summary, see Rüger 1968, 56–60.

249 Staehelin 1948, 426; MacMullen 1963, 7 and 29; Vittinghoff 1974, 121–2.

250 Von Petrikovits 1958, 1825–6 and 1960, 63–5; Von Gonzenbach 1963; Rüger 1968, chapter 5.

251 The *stationes* will be discussed in chapter 8.

252 This findspot is considered as such by Rüger 1968, 68. It has not been included in the catalogue because no other data (e.g., concerning the exact findspot, find-circumstances, and other materials) could be obtained.

253 See paragraph 3.4.2.

Fig. 50 List of stamps on tiles, including material without precisely known find-spots and secondarily deposited tiles (which may have been imported in later times from outside the area). Uncertainties are indicated by (?), possible additions by (....?).

Site no.	Site name	Stamps
1	Ede – Veldhuizen	*legio I Minervia*
7	Bennekom – Achterstraat	*exercitus Germanicus inferior*
18a	Wageningen – Westberg	*exercitus Germanicus inferior*
38	Kesteren – Nedereindsestraat	*exercitus Germanicus inferior*
		vexillarii exercitus Germanici inferioris
		legio I Minervia Antoniniana
39	Kesteren – Prinsenhof	*legio X gemina*
		exercitus Germanicus inferior
43	Kesteren – De Hoge Woerd	*legio X gemina (?)*
48	Opheusden – De Zeven Morgen	*exercitus Germanicus inferior*
59	Hien – De Wuurdjes	*legio X gemina (.....?)*
–	Zetten – Mariënborn	*legio X gemina pia fidelis*
105	Elst – dorp	*legio X gemina*
126	Arnhem – Meinerswijk	*legio I Minervia Antoniniana*
		exercitus Germanicus inferior
		vexillarii exercitus Germanici inferioris (?)
135	Huissen – Hazebergh	*legio X gemina (.....?)*
		legio XXX (.....?)
		legio XXX Ulpia victrix pia
		vexillarii exercitus Germanici inferioris
–	Doornenburg	*legio V Alaudae*
		legio XXII Primigenia
		legio XXX Ulpia victrix
		exercitus Germanicus inferior
182	Herwen – De Bijland	*exercitus Germanicus inferior*
		legio I Minervia (Antoniniana ?)
194	Loo – Loowaard	*legio XXII Primigenia*
		legio XXX (?)
214	Druten – Klepperhei	*legio X gemina pia fidelis (Domitiana ?)*
		vexillarii exercitus Germanici inferioris
222	Winssen – Het Oude Veerhuis	*exercitus Germanicus inferior*
244	Beuningen – De Heuve	*vexillatio Britannica*
298	Wijchen, De Pas – Passerot I	*vexillatio Britannica*
		legio XXX Severiana Alexandriana
315	Wijchen – Tienakker I	*vexillatio Britannica*
		exercitus Germanicus inferior
		vexillarii exercitus Germanici inferioris
316	Wijchen – Wijchens Meer	*vexillarii exercitus Germanici inferioris*
319	Wijchen – Molenberg	*vexillarii exercitus Germanici inferioris*
–	Wijchen	*exercitus Germanicus inferior*
		vexillarii exercitus Germanici inferioris
		Didius Julianus consularis
326	Wijchen – Palkerdijk	*vexillarii exercitus Germanici inferioris*
341	Lunen – dorp	*legio X gemina*
377	Mook – Kloosterberg	*legio X gemina*
		legio X gemina (.....?)
391	Malden – Heumensoord	*legio X gemina (?)*
		M. Val. San. (?)
395	Nijmegen – Hulzen	*legio X gemina*
399	Nijmegen – Waterkwartier	*legio X gemina*

Site no.	Site name	Stamps
		legio X gemina pia fidelis
		legio XXX Ulpia victrix
		vexillatio Britannica
		exercitus Germanicus inferior
		vexillarii exercitus Germanici inferioris
		Didius Julianus consularis
403	Nijmegen – Valkhof I	*legio X gemina*
		legio XXII
		exercitus Germanicus inferior
		vexillarii exercitus Germanici inferioris
		numerus Ursariensium
407	Nijmegen – Hunerberg west	*legio X gemina*
		legio X gemina pia fidelis
		legio X gemina pia fidelis Domitiana
		vexillatio Britannica
408	Nijmegen – Schildersbuurt	*legio X gemina*
		legio X gemina pia fidelis
		legio X gemina pia fidelis Domitiana
		legio XV Primigenia
		legio XXX Ulpia victrix
412	Nijmegen – Hunerberg	*legio VI victrix*
		legio VIIII Hispana
		legio X gemina
		legio X gemina pia fidelis
		legio X gemina pia fidelis Domitiana
		legio XV Primigenia
		legio XXX Ulpia victrix
		vexillatio Britannica
		exercitus Germanicus inferior
		vexillarii exercitus Germanici inferioris
		TRA
		M.Val.San.
–	Nijmegen – Hunerberg	*Didius Julianus consularis*
416	Nijmegen – Hunerberg oost	*legio X gemina*
		legio X gemina pia fidelis
		legio X gemina pia fidelis Domitiana
		legio XXII Primigenia
		legio XXX Ulpia victrix
		vexillatio Britannica
420	Ubbergen – Rijksstraatweg (1)	*vexillatio Britannica*
–	Ubbergen – Rijksstraatweg (2)	*legio X gemina*
424	Beek – De Ravenberg	v[
432(?)	Zyfflich	*legio I Minervia*
		legio XXX Ulpia victrix
		exercitus Germanicus inferior
		vexillarii exercitus Germanici inferioris
433	Berg en Dal – De Holdeurn I	*legio I Minervia*
		legio I Minervia Antoniniana
		legio I Minervia pia fidelis Antoniniana
		legio X gemina

Site no.	Site name	Stamps
		legio X gemina pia fidelis
		legio X gemina pia fidelis Domitiana
		legio XV
		legio XXX
		legio XXX Ulpia victrix
		legio XXX Severiana Alexandriana
		legio XXX Antoniniana
		vexillatio Britannica
		exercitus Germanicus inferior
		vexillarii exercitus Germanici inferioris
		Didius Julianus consularis
		Junius Macr. consularis
434	Berg en Dal – De Holdeurn II	*exercitus Germanicus inferior*
437	Niel – Pfarrkirche	*legio I Minervia Antoniniana*
		LE[
–	Wyler – Hochstrasse	*legio XV Primigenia*
–	Düffelward	*legio X gemina*
449	Keeken – Hufschenhof	*legio X gemina (.....?)*
450	Rindern – Friedhof	*legio I Minervia*
		legio I Flavia Minervia
		legio I Minervia pia fidelis (?)
		legio X gemina pia fidelis Domitiana (?)
		legio XXI rapax (?)
		legio XXII Primigenia pia fidelis
		vexillarii exercitus Germanici inferioris
460	Schneppenbaum – Qualburg	*vexillarii exercitus Germanici inferioris*
		numerus Ursariensium
466	Asperden – Versunkenes Kloster	*legio XXX*
–	Hommersum – Viller Mühle	*legio I Minervia*
		legio I Minervia Antoniniana
		legio X gemina
		legio XXX Ulpia victrix
		exercitus Germanicus inferior
499	Cuijk – centre I	*legio X gemina*
		[CAP *(?)*]IO
500	Cuijk – centre II	*legio X gemina*
		exercitus Germanicus inferior
		CTEC
529	Ubbergen – Berkenlaan	*legio I Minervia Antoniniana*

437, and 529, and also the material from Zetten-Marienborn, Doornenbrug, Wijchen, Ubbergen-Rijksstraatweg(2), Wijler-Hochstrasse, and Düffelward have to be left out of consideration because the find-circumstances are unknown and/or the material is in secondary position.[254]

This leaves thirteen settlement-sites for further consideration[255]: sites nos. 43, 48, and 59 in the Betuwe; 214,

254 Note that some of the material already mentioned, such as the tile-stamps from 432, 437, and Viller Mühle, could also be included in this category.

255 This is, of course, only a minimum-number, because quite a few of the previously mentioned sites may eventually prove not to have been military at all. In January 1983, a small excavation at site 95 (see chapter 5, note 16) also yielded a (round) military stamp, presumably EX GER INF or VEX EX GER INF.

222, 244, 298, 326, and 341 in the Land van Maas en Waal: 377, 395, and 424 on the ice-pushed ridge between Nijmegen and Kleve, and 449 in the Duffelt. All these sites are essentially civil settlements, many of them with stone buildings. Nos. 48, 244, 326, 395, and 449 have no indications of any building in stone. In fact, the thirteen sites constitute three different kinds of settlement. The five sites previously mentioned are native villages or large farmsteads. Sites 43, 59, 214, 222, 298, and 341 can be interpreted as *villae rusticae*, whereas nos. 377 and probably 424 are *villae urbanae*.

As far as the rural sites are concerned, there is no indication whatsoever that the military tiles in any of these settlements were used secondarily, because all stamps are associated with contemporary pottery, in most settlements even a lot of it. Only in the case of site 377 is there a possibility of re-used tiles.[256]

When the thirteen sites are considered, it is improbable that these settlements were military. The stamps at the *villae urbanae* can, if they are not in secondary position, be 'explained' by the fact that these estates could have been owned by officers or other high officials from Nijmegen. Site 424, and also probably sites 425 and 426 where military tile-stamps have not been found so far, may have been situated inside a *territorium legionis* belonging to the Nijmegen forts, and it is possible that the same is true for site 377.[257] For the other sites, however, this is hardly likely, except for the clearly native settlement 395 which should also be inside the *territorium legionis* when 377 is included.

In her analysis of the distribution of military tiles from Vindonissa, Von Gonzenbach was able to offer an interpretation for a similar situation where a number of *villae rusticae* were apparently built with military material. These are situated on the fertile soils of the Swiss upland plain, in the hinterland south of Vindonissa. Just as in the eastern river area, all sites are at least as early as the tile-stamps, so that secondary use becomes unlikely.

Von Gonzenbach explained this situation by introducing the concept of an 'area for military use' (*militärisches Nutzland*), which comprises the Roman concepts of *territorium legionis* and *prata cohortis*.[258] The area in question was safe, fertile, and easily accessible, and Von Gonzenbach attempted to show that its appproximate boundaries were stable for a long time, and that it must have been excluded from the civitas Helvetiorum. Under Claudius the villae were built by the army to provide food and fodder; the total area of c. 35 x 40 km also included large wooded areas.

This analysis of the distribution of military tiles led Von Gonzenbach to conclude that seemingly non-military villae were in fact built and owned by the army. Especially because of the fortunate circumstances in the area around Vindonissa, such as a fair rate of recovery of sites and a good periodization of sites and find material, there seems little reason to doubt her conclusions.

Similar favourable circumstances, however, also occur in the eastern river area, but the situation there is more complicated. First, it seems impossible to reconstruct a *militärisches Nutzland*, because that would encompass the entire area north of the Meuse, which would be most of the entire Civitas Batavorum and then one would also wonder what would be left for the town of Ulpia Noviomagus! But apart from these considerations, it is perfectly clear that the Land van Maas en Waal and the Over-Betuwe were very densely settled before and during the Roman occupation by a native population. It is worth noting that Von Gonzenbach[259] may have dismissed the problem of native settlement too easily when she assumed that the natives who occupied the area became tenants on the new estates. In any case, the relevance of her analysis of the 'Roman' sites is restricted by the fact that pre-Roman and contemporary 'native' sites are virtually left out of consideration.

This simplification does, however, not necessarily render her conclusions regarding the ownership of the villae invaluable. But it could definitely modify the concept of the *militärisches Nutzland*, which need not be as exclusively military as is often assumed. As far as the eastern river area is concerned, not only the distribution maps but – in the case of the river area – also the classical sources indicate that it should not be considered com-

256 Site 424, with only a fragment of a stamp and little pottery, cannot be evaluated in this respect. Braat (1934, 13) put the construction of the villa at site 377 at c. AD 125, and concluded that the LXG stamps, therefore, had been used secondarily. The material found during his small excavation is, however, to scanty to warrant such a definite statement. Most of the material can be dated to the 2nd century, but, apart from the tiles, there is other pottery which could also point to a Flavian construction date or to a preceding phase.

257 This possibility was also considered by Bogaers 1962–3, 82.

258 Rüger 1968, 51.

259 *Op. cit.*, 126.

pletely military territory. Tacitus' remarks on the special status of the Batavians, which was continued on the old conditions after the revolt of AD 69–70,[260] clearly contradict such a drastic measure as the creation of a military territory over a large part of the Batavian area.[261] In addition to all this, the evidence of the 6 LXG stamps (43, 59, 214, 341, 395, and 449) and the later military stamps from 214 and other sites is probably too scanty to assume the institution of a military territory at any time.

Von Gonzenbach's interpretation of the Swiss evidence cannot, therefore, be applied to the eastern river area, even though it was a frontier zone and thus under tight military control. This last observation could still lead to an interpretation of the settlements with military tile-stamps as military property, but there are other arguments which contradict such a conclusion. Three of the settlements involved (48, 244, and 395) have yielded so much material that it can be concluded that they never had any buildings in stone. More significantly, five settlements were already in existence in the Late-Iron Age or even before that (43, 244, 326, 341, and 449) and one (48) at least before AD 50. This already indicates that these were native settlements, not *villae rusticae* newly built by the army for its own purposes. Also, site 298 existed at least half a century (or more) before the earliest stamp (VEX BRIT) found on it can be dated; the earliest material from site 222, with an EX GER INF stamp, is Flavian.

There are only two sites, 59 and 214, which could have been built by the 10th Legion after AD 70. However, the excavations at site 214[262] have revealed that if the settlement did not already exist before AD 70, the buildings were certainly constructed in a native tradition. As the excavator puts it, the conclusion is justified that '*kurz nach 70 n. Chr. in Druten auf dem Gebiet der heutigen Klepperhei eine Siedlung entstand, die, wenn sie nicht sogar an einer, an diesem Platze bereits bestehenden Siedlung anschloss, in jedem Fall ihre Wurzeln in der einheimischen Tradition hatte, daneben aber bereits unverkennbare Zeichen von Romanisierung trug.*' There is no reason to assume that the situation at site 59 is any different from Druten.

In conclusion, it can be said that the available evidence indicates that stamped military brick was used by, and was thus available to, part of the native population. From a quantitative point of view, it has to be admitted that the number of stamps on military sites far exceeds that on civil settlements. Although the same is true for the total amount of brick, a correction for this factor – which cannot be calculated for lack of quantitative data – would presumably still indicate a relatively higher incidence of military stamps on military sites. The assumption that this is true does not, however, change the conclusion. Also, the why, when, and how of the stamping of tiles are unknown, and cannot lead to interpretations of this difference. In the end, it may even be the other way around and observed differences may lead to a better insight into the various motives for and frequencies of tile-stamping.

This situation, although desirable, is probably not attainable. From the presence of military and the absence of civil stamps, and also the central location of site 433, it has been concluded that virtually all brick on civilian settlements came from De Holdeurn. To conclude that is one thing, but to provide an explanation is quite a different matter. Not only the mechanisms relevant to the stamping of tiles are important in that case, but also those by which the products passed from producer to consumer.

In this respect, it may be important that the majority of stamps on civilian sites seems to be early (LXG, VEX BRIT) and not late ((VEX) EX GER INF). This could be due to changing distribution mechanisms and a shift from site 412 (the fortress) to site 399 (the town) as the centre for trade or barter. But, again, this remains mere speculation. The reason could be nothing more than a change in stamping-habits. Also, the phenomenon may not even exist, because a great number of stamps from many sites have been excluded only because their presence might in some way be attributed to the army, without any real evidence that this was indeed the case.

This implies that the evidence for military stamps in a civilian context may be even larger than has been assumed so far. The data from the eastern river area certainly

260 Tacitus, *Hist.* IV, 12,3; IV, 17,2; V, 25,2 and *Germ.* 29. See also Bogaers 1955, 186–8 and 1960–1, 263–5.

261 Similar conclusions about military territories in general, described as *irrige Verlegenheitskonstruktionen* (Vittinghoff 1976, 77), and the interpretation of tile-stamp distributions in particular have recently been put forward by Vittinghoff. In a detailed and well-documented article (1974) he not only rejects the use of tile-stamp distributions (1974, 121–2) to reconstruct a *militärisches Nutzland*, but also presents far-reaching criticisms of the entire concept as developed by Rüger (1968, chapter 5).

262 Hulst 1978, esp. 149–50.

support Vittinghoff's ideas, and it has to be concluded that although it may be true in some areas,[263] Wolff's hypothesis is not universally applicable.

This is also demonstrated by the intuitively even obvious fact that all sorts of material from the army were bound to spread over the surrounding countryside. In this respect, it is useful to point to the other 'military' characteristics of sites with tile-stamps, whose native origin and character is beyond doubt. For example, only sites 43 and 326 do not appear in fig. 24 because they have less than ten sherds of t.s. and this is significant only for site 43.[264] Several have fine Nijmegen pottery or exceptionally early pottery. This may point to a special relation between the army and the inhabitants.

6.5.2 Graffiti

Under 'graffiti', only a limited class of inscriptions will be discussed. These are the *post cocturam* inscriptions on pottery. It would be useless to discuss the names, signs, or other texts engraved before the pottery was fired, because they contain no information about the people in the river area. Unless, of course, the site of production was also located in the area, such as the *ante cocturam* graffiti on the Nijmegen ware.[265] Painted inscriptions should, in principle, be included. They are, however, very rare. Only a few examples are known on pottery from the legionary fortress from Nijmegen. Inscriptions on materials other than pottery are also included, as long as they do not refer to the production process (outside the river area).

Much interesting and useful data can be obtained from the study of graffiti,[266] but the available evidence from the eastern river area is too scanty to allow a fruitful discussion. Just as with so many other data, probably 90% or more of all graffiti discovered so far in the river area are from Nijmegen and only a small part of these has been published to date. Apart from a more general overview by De Waele, collections of graffiti were published by Breuer, Brunsting, and Stuart.[267]

This is not a surprising situation, especially when the material studied by Bakker and Galsterer-Kröll clearly showed that most of the graffiti were found in a military context,[268] even when the composition of the collection, which is clearly biased towards finds from military settlements, is taken into account. This may be due to a number of factors. First, graffiti occur especially on valuable pottery, notably terra sigillata, which is definitely more abundant in military (or closely related) settlements.[269]

Second, officials and military personnel were more literate than the civilian population. This may be true, it can even be taken for granted for the Early-Roman Period, but it is partly invalidated by a third argument. As Bakker and Galsterer-Kröll have noted,[270] owner's marks (both names and signs) are much more likely to occur in a military camp with a large number of heterogeneous people living together than in the households in civilian settlements. This would explain why two decades of occupation in Vetera yielded 75 graffiti, whereas two centuries of occupation in the equally well-excavated Colonia Ulpia Traiana, which undoubtedly had quite a few literate inhabitants, resulted in only 18 graffiti in the Bonner museum collections. For this reason, the absence of graffiti cannot be interpreted as an absence of literacy.

The approximately 50 inscriptions on pottery from the cemetery near Hees testify to the literacy of at least part of the population of Ulpia Noviomagus. There are only a few graffiti from a civilian, or at least not directly military, context in the surrounding area. These are:

1	IV[on an amphora	Site 1
2	C[on a smooth-ware sherd (probably flagon)	Site 1
3	// on a coarse-ware rim Nb 89	Site 7
4	// on an amphora	Site 7
5	]RA[on a smooth-ware sherd (probably flagon)	Site 47
6	]VSA[on part of a flagon	Site 96
7	]NO ANAΓINωCK[on the wall plaster of temple II[271]	Site 105
8	M on a terra sigillata cup Drag 27	Site 154

263 In addition to the literature which has already been mentioned, see also, e.g., Spitzlberger 1968, 96 on Raetia and Alföldi 1967, 44 ff. on Dalmatia.

264 See p. 137. The total amount of material recovered from site 326 is too small.

265 See Haalebos/Thijssen 1977.

266 See Bakker/Galsterer-Kröll 1975, esp. chapters 3a, 4, and 5.

267 De Waele 1931, 65–72; Breuer 1931, 112–4 and Pl. XIV; Brunsting 1937, 191–7 and Pl. 8; Stuart 1963, 98–104 and Pl. 26–8; Stuart 1977, 69–71 and Pl. 58–60.

268 *Op. cit.*, 7–8 and 55–6.

269 *Op. cit.*, 9–10. See also paragraph 6.2.1.

270 *Op. cit.*, 56.

271 Bogaers 1955, 135–7.

9	L on a two ribbed flagon-handle	Site 212
10	TAGVZI on a smooth-ware sherd (probably flagon)	Site 214
11	V FEC on the wall plaster of building no. I[272]	Site 214
12	]PPX[on a smooth-ware sherd (probably flagon)	Site 232
13	VERVS(?) on a terra sigillata mortarium	Site 239
14	]X[on a smooth-ware sherd (probably flagon)	Site 270
15	XII on a coarse-ware sherd	Site 298
16	E (or N) on a terra sigillata cup Drag 27	Site 315
17	AMAE(?) on a terra sigillata dish Drag 18	Site 319
18	M on a terra sigillata dish Drag 18/31	Site 332
19	N.NOVVM on a terra sigillata dish Drag 18/31[273]	Site 368
20	A M on a terra sigillata dish Drag 18/31[273]	Site 368
21	IN(?) on a terra sigillata dish Lud Tq[273]	Site 368
22	VITALIS on a terra sigillata dish	Site 396
23	BE[on a terra sigillata dish Drag 31[274]	Site 432?
24	NITV on a bronze patera[275]	Site 440
25	IVL VICT on a terra sigillata cup[276]	Site 442
26	SIPERI (or CIPERI) on a flagon[277]	Site 455?
27	//// on a terra sigillata dish Lud Tq	Site 462
28	NEXAVVO (or NIRIAVVO) on a terra sigillata cup Drag 33[278]	Site 500

Simple owner's marks (or numbers) in the form of X, ⁜, or ⊗, which occur at a number of sites (a.o. 21, 120, 126, 214, 232, 234, 298, 315, and 368) have been omitted from this list. The 'military' graffiti are more numerous. They have been found at a number of sites, including 9, 38, 39, 69, 120, 125, 126, 129, 182, 433, and 499(?)[279] and generally consist of readable texts or names.

As is obvious from the above list, the civilian graffiti in the eastern river area also consist partly of readable text or at least a letter. Nos. 3, 4, 27, and possibly 14 and 16 are illiterate or numbers, no. 15 is a number. When the X marks are added to these, the ratio of 'literate' v. 'illiterate' graffiti would probably be c. 50:50, but such a relation means little. Not all graffiti were necessarily written by the native population,[280] although it is not unreasonable to suppose that they were generally inscribed by the people who lived or were buried on the sites where the graffiti were found. The illiterate X-signs and numbers that appear especially on common coarse ware or terra nigra-like pottery (e.g. the storage jars T I[281]) point to the fact that they are not owner's marks but designations concerning the contents or special use[282] of these pots. The literate graffiti are found especially on terra sigillata (twelve times) and flagons (eight times), and could be predominantly owner's marks. These differences cause difficulties with a straightforward interpretation of literary v. illiteracy, as proposed by, for example, Bloemers.[283] There was apparently little need to inscribe valuable pottery outside military camps and the chance of finding this pottery, let alone the inscribed part of it, is exceedingly small on sites where it is relatively infrequent. The more frequent kitchen wares with use- or content-marks are more likely to be found.

The above arguments lead to the conclusion that graffiti cannot, as yet, be used as hard evidence in comparing settlements and evaluating degrees of literacy, or its absence. They can, however, serve to establish the presence of literate people, and in this sense the eastern river area does not seem to do badly, given that the total number of graffiti from settlements exceeds the total number recovered by the excavations of the Colonia Ulpia Traiana until 1975. If this would imply that the total number of literate inhabitants also exceeded that of the CVT, the region would probably have quite a few of them.

6.6 OTHER FINDS

This paragraph discusses a number of diverse artifacts related to industrial or, more generally, to economic ac-

272 Peters/Swinkels/Moormann 1978, 170.
273 Bogaers/Morren 1954, 58–9.
274 Bakker/Galsterer-Kröll 1975, 171–2.
275 Den Boesterd 1956, 25 (no. 68) and Pl. XVIII, 68a.
276 *CIL* XIII, 10017 = *ER* II, 118.
277 Bakker/Galsterer-Kröll 1975, 172.
278 Koeling/Koolen 1978, 46 and *afb.* 18.
279 For details see the catalogue and the literature cited therein. The graffiti from site 126 will be discussed in chapter 9. Those of 39, 182, and 499 (?) are unpublished.
280 Which is certain for no. 7 from site 105, which was written by a Greek immigrant or visitor, according to Bogaers, *op. cit.*
281 See p. 163 and note 86.
282 As Bakker and Galsterer-Kröll (*op. cit.*, 56) rightly point out, the same pottery would not be used one day for fish and garlic and the other for a sweet food, so there was a need for distinguishing possibly identical pottery forms.
283 Bloemers 1980, 167.

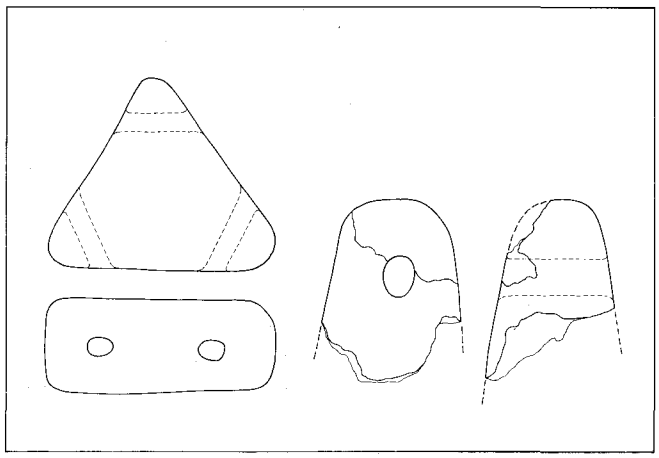


Fig. 51 Triangular and conical loomweights from Ewijk-De Ooigraaf (site 235).

tivities in the eastern river area. This subject, which is touched on several times elsewhere in this article, will be dealt with in a more integrated way in the second part of this study. But some of the material evidence – usually described in the categories (*l*), (*n*), (*p*), and (*q*) of the catalogue – is presented here for separate evaluation.

6.6.1 Loomweights and Spindle-Whorls

Loomweights and spindle-whorls come to light relatively frequently on settlement sites. Loomweights occur at least at the following sites: 1, 42, 59, 81, 93, 141, 142, 151, 189, 197, 214, 232, 235, 250, 261, 270, 280, 299, 300, 301, 326, 333, 479, 483, 500, and 524. During the inventarisation of the collections, only one type of loomweight was recorded in any quantity: fairly large triangular loomweights with three holes through the sides (fig. 51). These are found, together with an occasional ring-shaped loomweight, in a late-Iron Age as well as Roman Period context and are manufactured from clay that is generally, but not always, fired rather softly. Loomweights made from secondarily used Roman material, such as brick, do not exist (see paragraph 6.6.4).

The ring-shaped loomweight, which serves to hold a large number of threads in its one large hole, seems to be more or less timeless. It is also found in later settlements such as Wijster and Odoorn.[284] The triangular loomweights, however, seem to disappear during the Late-Roman Period when they could have been replaced by conical loomweights, such as Wijster type XII B, that have also been found in some quantity in Ede-Veldhuizen (1). This does not imply that all conical loomweights are relatively late. They have been found in pre-Roman and middle-Roman contexts[285] and are, therefore, a rather long-lived form. There are only a few loomweights datable to the Merovingian Period. The examples from a weaver's hut in Cuijk (500) are described as having a central hole and roughly biconical in section.[286] Spindle-whorls are more frequent than loomweights. They were found in larger numbers and at more sites: 1, 106, 111, 114, 140, 141, 144, 148, 149, 150, 151, 177, 185, 189, 214, 218, 226, 232, 234, 235, 239, 244, 248, 250, 273, 283, 300, 315, 329, 350, and 479. They are generally conical, sometimes more bulbous in section, always well finished and fairly hard-fired. Spindle-whorls manufactured from secondarily used native pottery sherds have been observed only sporadically.

There are a few points about loomweights and spindle-whorls which deserve further attention. First, it could be important that both are definitely 'native'. There are no examples anywhere that have been manufactured in, for example, smooth or coarse ware, which would point to their production by a potter working in the (Gallo-) Roman tradition. They were therefore probably not bought but locally made,[287] a possibility which may have interesting implications. It could indicate a continued and uninterrupted tradition of native ceramic technology. The fact that apparently loomweights and especially spindle-whorls were not made by (semi-) professional (Gallo-) Roman potters in any period provides support for the idea that in most settlements at least some native pottery continued to be manufactured as well.

It could also be an indication that the technology of textile production continued relatively unchanged during the entire Roman occupation,[288] although there is no

284 Van Es 1967, 285, type XIIA and Van Es 1979, 216, type IX.

285 Van Tent 1978, 243. Fig. 35,1 is pre-Roman. Bloemers 1978a, 373, type XIIIB, dates to the Middle-Roman Period.

286 Bogaers 1966, 71.

287 In contrast, many examples of later (Medieval) spindle-whorls found in the river area are clearly not locally made. They are manufactured in *Steingut* and other pottery, sometimes even glazed.

288 See e.g. Forbes 1964, 203 ff. on the persistence of the warp-weighted loom, even though it was partially replaced by the double-beamed vertical loom in the classical world.

way of verifying this because of lack of organic remains. On the other hand, the organization of textile production could have changed considerably. Applebaum[289] has argued, on the basis of different distribution patterns of loomweights and spindle-whorls in Roman Britain, that while 'spinning was the practice in every household, weaving was limited to established mills associated with industrial centres, located on large estates private or governmental'. There is no way of testing conclusively this assumption in the eastern river area on the basis of the available evidence. Loomweights are definitely less numerous than spindle-whorls and also occur at fewer sites. But this may in part be fortuitous, because the former are less well fired and easily break into unrecognizable pieces. Besides, the proportion of 26 against 31 sites is not particularly significant.

It could be that there actually is a difference between the late-Iron Age and the Roman Period distribution patterns, especially as far as loomweights are concerned. Unfortunately, we are dealing largely with surface collections and this means that the weights cannot be accurately dated and that a difference, if it indeed existed, is hard to detect. It should be noted that no significant concentrations of loomweights are found in strongly acculturated settlements, which, in the context of the river-area, could be interpreted as 'large estates'. On the other hand, loomweights certainly do occur in small native-Roman settlements (such as 93). As long as insufficient excavation material is available it seems best, therefore, not to apply Applebaum's idea but to assume that both spinning and weaving were activities at the household level in the river area.

6.6.2 *Slags and Crucibles*

Slags occur frequently on sites, sometimes even in large numbers. Almost all of them are iron slags, although a few have been related to pottery production. Unfortunately, iron slags are undatable, so some of them may be later than the period treated here; examples are the slags (and cindered clay) from site 197, which are probably medieval.[290] Iron slags which probably date to between 250 BC and AD 750 have been found at sites 79, 81, 93, 98, 123, 126, 141, 142, 168, 214, 227, 232, 235, 256, 261, 270, 289, 299, 300, 312, 350, 416, 417, 454, 460, 461, 466, 479, 485, and 508.

These are, of course, certain indicators of local iron production. Only the slags from sites 256 and 350 can be dated to the Late-Iron Age. Other Late-Iron Age slags may occur elsewhere,[291] but this cannot be demonstrated. The production of iron certainly increased during the Roman occupation, in view of the number of slags datable to that period.[292] Crucibles and cindered clay are also largely restricted to the Roman Period sites. They have been found at sites, 1, 93, 109, 270, 312, 416, 417, and 466.[293] The crucible fragment from site 1 is possibly of late-Roman date and the material from site 466 was found in a (late-) Merovingian smithy.[294]

According to Brongers and Woltering,[295] iron ore did not have to be imported but was available in the form of bog-iron ore and iron concretions (*Klapperstenen*) in Pleistocene sands. Bog-iron ore does not occur in the river area proper but it can be found close by, both south of the Meuse and north of the Rhine.[296] *Klapperstenen* are found on the Veluwe and also on the ice-pushed ridge Nijmegen-Kleve. Although both sources have probably been exploited, large amounts of iron (and all other metals) must have been imported into the eastern river area, especially during the Early- and Middle-Roman Period. Probably in the Late-Roman Period and certainly during the Merovingian Period, the earlier settlements themselves must have been an important third source of iron. The early-medieval smithy at site 466, in which almost all of the iron of the late-Roman burgus was probably reused, is an example.

6.6.3 *Glass*

During the Late-Iron Age, glass was commonly used in the river area in the form of bracelets and, occasionally, ring-shaped beads (*Ringperlen*). These have been studied and described by Peddemors,[297] who concluded that at least one type (3b) was probably produced locally. The bracelets cease to be manufactured and used during

289 Applebaum 1972, 216–8.
290 See NKNOB 1958, 166.
291 Possibly at site no. 232.
292 See also Brongers/Woltering 1973, 29.
293 Not all of these were used for iron. Some, e.g., from site 417 (Bogaers/Haalebos a.o. 1975, 156) are definitely for bronze.
294 Hinz/Hömberg 1968, 190–1.
295 *Op. cit.*, 28–9.
296 *Op. cit.*, 30, fig. 2.
297 Peddemors 1975; the catalogue contains several additions to his inventory.

the Roman Period, although they are occasionally found in an early-Roman context.[298]

Although the distribution of the La Tène bracelets shows a consistent association with obvious trade-routes to the south along the major rivers,[299] the clearly observable abundance of certain bracelets, such as type 3b, makes Peddemors' hypothesis of local production acceptable. The import of at least some of the raw materials (manganese and cobalt oxides) and maybe even crude glass does not contradict this probability. At the moment, the technological, typological, and to some extent the chronological data on La Tène glass are fairly well investigated. Unfortunately, the functional or social aspects of the bracelets have been almost completely neglected, apparently on the assumption that they must have been adornments.

They undoubtedly had this function, as is evident from numerous grave finds.[300] But the bracelets may have fulfilled other social and economic needs as well. In Switzerland and Rheinhessen, where they have been found mainly in graves, their role in the burial program may have been that of an indicator of, for example, social status. As has already been discussed elsewhere, [301] there are also a number of peculiarities about glass bracelets, which may indicate that they functioned as primitive money, in Dalton's sense of the concept.[302] These peculiarities include, among others, their frequent occurrence in commercial centres such as the Celtic oppida and especially along trade-routes, although many bracelet types were locally made and not imported. Therefore, they seem to be more a concomitant phenomenon of, rather than an object of trade itself. Furthermore, secondarily worked pieces of bracelets[303] could well indicate a phenomenon similar to the halving and quartering of coins. A third notable feature is that the end of their production is in some way directly related to the beginning of the Roman occupation.

The high quality of La Tène v. Roman glass could have encouraged the prolonged production of the former, or perhaps the adoption of its technique by the Romans. Neither happened and, even more significantly, these bracelets were not replaced by Roman equivalents. If the La Tène bracelets were just trinkets, one would expect to find Roman (glass or other) bracelets instead, but these are very rare indeed.

A more promising explanation, therefore, seems to be that they were indeed replaced, not as bracelets, but as objects of socio-economic importance. As a means of storing (and displaying) wealth and as a medium of exchange in certain transactions, the bracelets may have suffered from the introduction of Celtic coins in the river area, an event which has been related to the arrival of the Batavians.[304] As yet it is unknown to what extent these coins were actually used in the river area. But whatever happened then, they were definitely replaced when Roman coins started to serve as a standard of value.

For the moment, it is impossible to test this hypothesis regarding the function of La Tène glass bracelets in other areas because detailed regional distribution maps are lacking. The dating of different groups of bracelets and their frequencies in different regions, in relation to the introduction of coinage, needs to be studied in greater depth. Moreover, it cannot just be assumed, that 'glass bracelets' in general fulfilled exactly the same functions and/or had exactly the same symbolic meaning in all regions, although their total disappearance at about the same time indicates that these were probably not radically different.

As already mentioned, Roman glass completely replaces the rather limited range of Iron Age glass products and has been found on several sites in the river area, although its total distribution is still rather limited. Settlements with Roman glass are, curiously enough, much less numerous than those with Iron Age glass. The settlements where Roman glass has been found are listed in fig. 52.

It is evident from this list that the occurence of glass sherds is related to the other attributes. It is to some extent determined by the total number of sherds recovered, but it occurs only at 24[305] of all the Roman Period settlements, which is a very restricted distribution compared to, for example, terra sigillata. It seems to have been a relatively expensive commodity, especially because of the relation to factors as 'stone building' and 'a

298 For example, on site 417. The clearly antique bracelets in early-Medieval contexts (cf. Peddemors 1975, 97) are irrelevant in this respect.

299 See Willems 1983a, fig. 3.

300 Haevernick 1960.

301 Willems 1983a.

302 Dalton 1978.

303 Peddemors 1975, 99: fragments bended into small rings or having round melted fractures.

304 Roymans/Van der Sanden 1980.

305 Note that some (excavated, unpublished) sites may be absent from the list because of lack of information.

Fig. 52 The relation between different categories of finds from sites containing Roman glass. The Nijmegen sites have been omitted. Glass is present on all of them in relatively large quantities.
Column 1: x = excavated sites or sites where large amounts of pottery (at least 250 sherds) were collected.
Column 2: x = sites with (a) stone building(s).
Column 3: x = sites associated with military affairs, e.g., because the material contains military tile-stamps or other 'military' artifacts (without the implication that they are themselves military).
xx = sites strongly associated with military affairs (implying that they are (probably) military).
Column 4: x = sites with more than 10 sherds of terra sigillata.

No.	Name	1	2	3	4
43	Kesteren – De Hoge Woerd	x	x	x	
48	Opheusden – De Zeven Morgen	x		x	x
59	Hien – De Wuurdjes	x	x	x	x
105	Elst – dorp	x	x	x	x
117	Driel – Baarskamp/Lizesland	x	x	xx	x
126	Arnhem – Meinerswijk	x	x	xx	x
130	Huissen – Loostraat I[306]				
142	Huissen – Bergerden	x			
182	Herwen – De Bijland	x	x	xx	x
214	Druten – Klepperhei	x	x	x	x
232	Ewijk – Ewijksche Velden	x			x
234	Ewijk – De Hoge Woerd	x	x		x
239	Ewijk – De Grote Aalst	x	x		x
244	Beuningen – De Heuve	x		x	x
284	Leur – dorp				
298	Wijchen – De Pas, Passerot I	x	x	x	x
379	Katerbosch – Eindweg				
433	Berg en Dal – De Holdeurn	x	x	xx	x
441	Millingen – Eversberg	x	x	x	x
460	Schneppenbaum – Qualburg	x	?	xx	x
466	Asperden – Versunkenes Kloster	x	x	xx	x
499	Cuijk – centre I	x	x	xx	x
500	Cuijk – centre II	x	x	x	x
508	Middelaar – Witteweg		x		

lot of terra sigillata'. Also, the relation to military sites or finds is quite clear. Because glass also sporadically occurs in graves and, of course, in civilian settlements further south, it is rather too far-fetched to assume that is has a 'military' distribution. But at least the owner had to be relatively rich, a factor which, in the river area, seems to be firmly associated with 'army connections'.
As far as the nature of the glass is concerned, almost all the finds are fragments of vessels. Only two certain examples of window-glass (126, 441) and two doubtful ones (130,[306] 298) have been recorded.
Not all the glass in the river area was imported. The discovery of a pit filled with broken glass at site 416 suggests a local workshop where glass was recycled in the Flavian period.[307] The same may have happened at site 466 in the Late-Roman Period.[308] The numerous fragments of glass vessels and the presence of congealed drops of glass make this interpretation very probable. In view of the total distribution of glass, the military nature of both sites is hardly surprising. The production of glass not primarily intended for use by the army is not very likely in the eastern river area.

306 It is uncertain whether the fragments from site 130 are Roman.

6.6.4 *Net-Sinkers*

Weights which are probably net-sinkers have been found at a number of sites in the river area. The weights are generally made of secondarily used Roman brick or other building-material, but specimens that were primarily manufactured as weights also occur. A very large number of these weights were among the material from site 316 (Wijchens Meer). A selection of these is illustrated on fig. 53 and gives the range of variation.
The first distinction to be made is between weights made from secondarily used material (by cutting and drilling) and those primarily manufactured as weights (by modelling the clay and firing it). The second category may be especially informative on the function of these weights, because they can be assumed to conform closely to the purpose they were supposed to fulfil.
Most of the weights from secondarily used material are

307 Isings 1980.
308 Hinz/Hömberg 1968, 186–7.

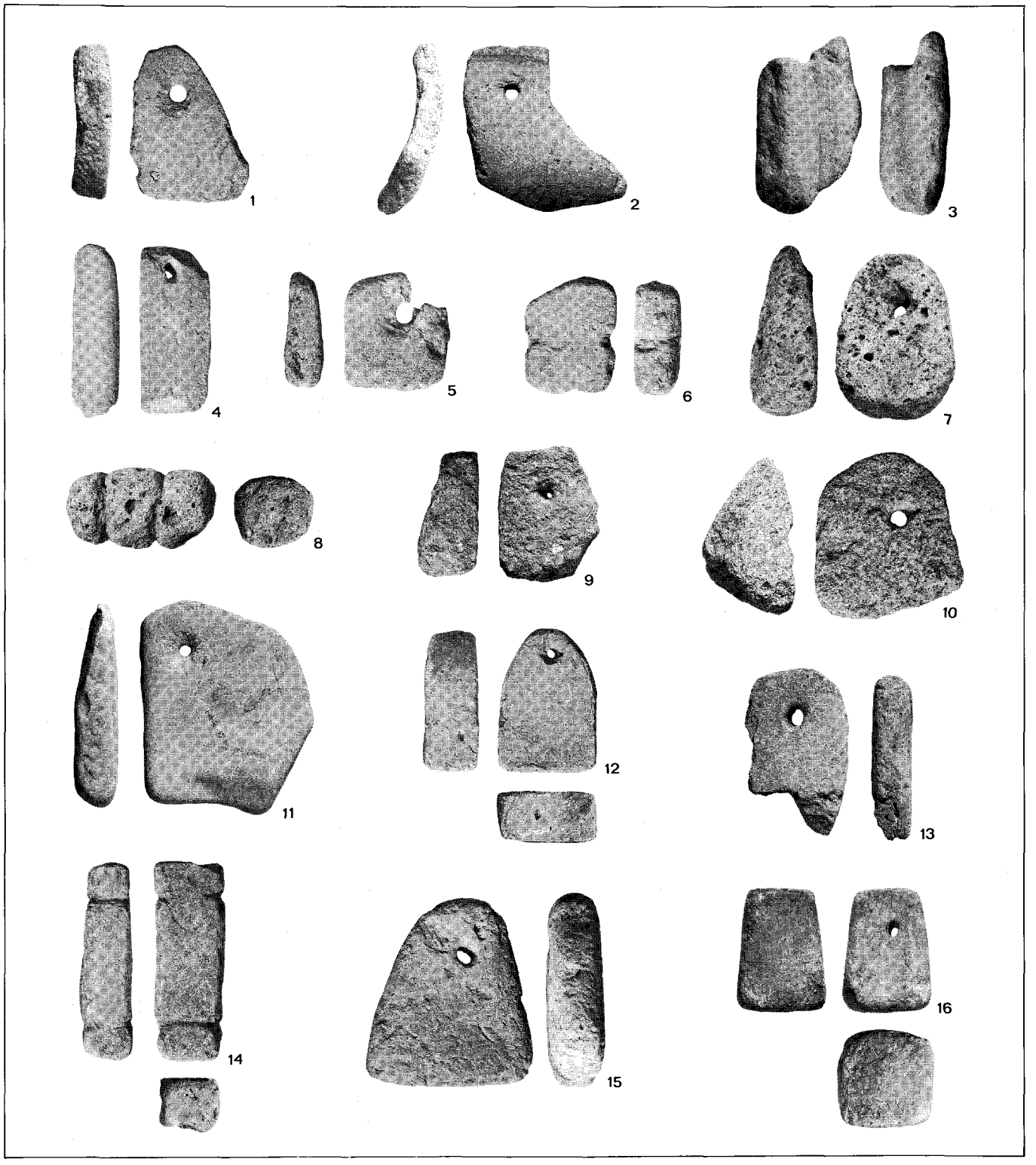

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Fig. 53 Typical forms of Roman Period net-sinkers found in Dutch rivers. The examples are all from Wijchen (site 316).

clearly made from Roman brick. Fig. 53, 1 and 2 are parts of *imbrices*, 4–6 of *tegulae*. All show provisions for attaching a rope. For the weights nos. 1–5 this is a hole which was drilled through the brick (diameters between 8 and 12 mm), while no. 6 is notched. In section, it appears that no. 1 is carefully modelled into a triangular shape, 3 and 4 are rectangular, and 5 and 6 more or less square. No. 2 is either damaged, unfinished, or clumsily made. Comparable weights are known from sites 23, 194, and 199. They also occur outside the area studied, for example in the settlement at Rijswijk, in the Koornwaard near 's-Hertogenbosch, the Ossermeer, and in the IJssel near Dieren.[309]

Apart from brick, weights were also made from tuff (fig. 53, 7–8), basalt lava (9–10), and quartzitic sandstone (11). Nos. 7 and 9–11 all have holes (5–12 mm), while no. 8 has two grooves. No. 7 is triangular in section, no. 8 is rectangular (although so weathered that it is almost round) and no. 9 is more or less square. No. 10 is barely modelled and no. 11 not at all. Weights made of tuff are also known from sites 75 and 194, from the Koornwaard, and from Rijswijk.[310]

Primary weights, which have so far been identified only at site 316, are made of fired clay, but here there are two varieties. The two wedge-shaped weights on fig. 53, 12–13 are manufactured from well-fired clay tempered with sand, which can only be described as brick. The sides indicate that they are moulded, and the holes were drilled before firing. On no. 12, the opening has been widened on both sides with a knife into a conical shape, probably to avoid a sharp edge which could cut into the rope. On no. 13, the openings are widened by a smooth groove at the top, the only place where the rope would actually rub against the brick. The weight on fig. 53, 14 is rectangular in section and can also be described as a primary, moulded brick product. The two grooves were hollowed out before firing.

The wedge-shaped weight on fig. 53, 15 and the conical weight no. 16 are less well fired. No. 15 is tempered with organic material in addition to sand grains. The paste is very much like that of the loomweights described earlier. No. 16 is more brick-like. Both have holes which were drilled before firing.

There seem to be three major types of weights: wedge- and block-shaped, both with holes, and block-shaped with grooves. The undamaged wedge-shaped ones weigh 40 (1), 340 (7), 394 (12), and 585 (15) grams; the blocks with holes weigh 343 (9) and 495 (16) grams; the grooved specimens weigh 223 (8) and 348 (14) grams. Nos. 10 and 11, which are also complete, are considerably heavier: 960 and 1022 grams. The variation in weight is representative for the other weights from Wijchen and elsewere, but it should be noted that very light, complete weights are also present which are not illustrated here. The extreme is 126 grams for a grooved specimen, also from Wijchen.

As far as the dating of these weights is concerned, there is little reason to doubt that they are Roman. All were found in association with either exclusively or predominantly Roman material. In addition, when secondarily used material was employed it was Roman, either definitely in the case of brick or very probably in the case of tuff and basalt lava. A Roman origin cannot be demonstrated for primary brick weights such as nos. 12–14, but the brick does not at all look un-Roman. It fits perfectly in the range of variation displayed by Roman brick, such as that from De Holdeurn and other places. Moreover, should these not be Roman, then there is no alternative but to date them to the Late-Middle Ages, which, in the context of the find-spots, is less likely.

In principle, however, it is always possible that at least some weights were actually made and used in a later period. Roughly comparable weights, including examples which are definitely made of secondarily used Roman material, are also known from Carolingian Dorestad.[311] There are, however, some positive 'typological' differences. The specimens from Dorestad include comparable wedge-shaped ones, but also rhombic weights with a hole. All rectangular or round, long specimens have one groove in the middle only, instead of two on the sides.[312] Finally, in the Dorestad material there is no or very little

309 Rijswijk: Bloemers 1978a, *Abb.* 180, 579/5620; Koornwaard: Zoetbrood 1983, 48; Ossermeer: Verwers/Beex 1978, 32 and *afb.* 47; Dieren: finds in the Gemeentemuseum Arnhem.

310 Bloemers 1978a, *Abb.* 146, 744/5785.

311 Kars 1982.

312 The same is also observed on Roman weights from Pommerœul in Belgium, see De Boe/Hubert 1977, 37 and fig. 50–1, but these could well be anchor stones of a gill net (cf. Cleland 1982, fig. 6).

secondarily used Roman brick, whereas it is in the overwhelming majority on the sites discussed here.

In conclusion, it can be said that the weight's material in itself is not decisive for dating purposes. The evidence presented above indicates that weights made of Roman tiles are very likely to be Roman, but that for weights made of tuff this is somewhat less certain. The material therefore, can only exclude some possibilities and provide a *terminus post quem* in a number of cases. Until even more evidence is available, the associated material is decisive in dating the weights. The third possible indicator, the shape and mass of the objects, is not likely to be very time-sensitive , because the basic characteristics of weights are rather universal: a certain mass and a provision for attaching a rope are all that is necessary.

The shape and mass of weights do, however, yield information concerning their function. This is a difficult matter, because weights are essentially multi-purpose objects which can be used to hold down many different things (in different periods), but, in combination with the find-circumstances, a convincing interpretation can be offered.

The interpretation that they are net-sinkers is based primarily on the findspots. It is very significant that, while isolated examples occur in settlements such as Rijswijk or site 75, all the sites where large quantities were found are associated with rivers. Site 316 is a findspot of material in a Roman branch of the Meuse, and the same is true for the material from the Koornwaard and the Ossermeer in the province of Brabant. The finds from sites 23 and 194 are clearly related to the bed of the Roman Rhine, those from Dorestad to the early-Medieval Rhine, and the finds from site 199 and from Dieren to the IJssel.[313]

Especially because weights are often associated with other material, presumably from nearby settlements, it would still be possible to argue that they are normal settlement debris. Apart from the vast differences in quantities found in water and on land, which is a convincing argument to the contrary in itself, the shape and mass of the weights are also significant.

Except for fishing, there seems to have been only one other sort of activity in which large numbers of weights were used, namely, weaving. The loomweights discussed in paragraph 6.6.1 are, however, very different from the weights treated her. First, there is a clear difference in mass. The loomweights are relatively heavy and the variation is limited: from c. 500 to c. 800 grams. This would seem to exclude an interpretation as loomweights for the very heavy kind (around 1000 grams and more) and the light weights of less than c. 400 grams, the latter being in the majority.[314] Second, there is a difference in shape, especially in the holes. Apart from the fact that no examples with grooves have been observed, the holes in loomweights are either large (c. 2 cm or more), or there are several somewhat smaller holes with a diameter of at least 1 cm. This is understandable, because a considerable number of threads had to be attached to each loomweight.[315]

The net-sinkers generally have much smaller holes, ranging from 0.5 to 1.2 cm, and never more than one. These are suitable for attaching one rope but not for bundles of threads. It is worth noting that not only the net-sinkers from secondarily used material but especially the primary (brick) specimens have holes with a diameter of less than 1 cm.

Third, loomweights have one essential characteristic that net-sinkers do not: all weights hanging from the same loom must have almost exactly the same mass. If they do not, the tension will vary from one bundle to the other and the cloth so woven will be irregular.[316] Because several classes of loomweights may have been used, a random selection of them could still vary widely, but it appears in practice that this range is not so large: loomweights seem to have a mass of 500–800 grams, whereas the masses of net-sinkers differ more widely.

Apart from the mass-distributions, the necessary uniformity of loomweights also has consequences for their manufacture. Equal masses are very easily attained when weights are manufactured from clay, but they are much more difficult to obtain when the weights are cut from stone or brick. This implies that weights from secondarily used material are very unlikely to be loomweights for that reason alone.

313 The possible significance of Roman net-sinkers at site 199 is discussed in chapter 3, p. 60. Other findspots associated with rivers are the Roman weights from Pommerœul (see note 312) and the early-Medieval specimens from Koudekerk, referred to by Kars 1982.

314 These figures compare very well with those of the Dorestad specimens discussed by Kars 1982.

315 The exact number is, of course, variable. It depends on the number of threads per cm^2 of cloth.

316 I thank Mrs T. IJzereef-Wald for drawing my attention to this fact.

On the other hand, the relative hardness which is necessary for weights used under water makes it practical, if not necessary, to cut weights from stone or brick or to manufacture them in a brick or brick-like quality. Loomweights are usually fairly soft-fired.
From the above discussion, it is clear that the weights discussed in this paragraph cannot be loomweights and must have a function related to water. Whether their only function was indeed to hold fishing-nets such as seines or gill-nets in a vertical position cannot be established here. In the Koornwaard, the weights were found together with a bronze fish-hook,[317] and various kinds of weights may have been used for various fishing techniques, which may be related to the effective mass in water of the weights[318] and to their shape.[319] Other objects may have had a similar function. At site 316 the weights are associated with a large number (37 pieces) of small, rolled up sheets of lead. These small leaden rolls have also been found on two sites along the Meuse, near Kessel/Lith (over 150 pieces) and Teeffelen, as well as in Maurik along the Rhine.[320] These are likely to be Roman, and their shape conforms exactly to the leaden rolls which are fastened around the lower horizontal rope of some modern fishing-nets.

317 See Zoetbrood 1983, 48, fig. 11.
318 See Kars 1982. The effective mass of tuff is low, compared to brick or sandstone.
319 Possibly, though not necessarily, there is a difference between weights with holes and those with groves. For illustrations of two sorts of nets, a seine and a gill-net, see Cleland 1982, figs. 5 and 6.
320 Van der Sanden 1983.

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7 INTRODUCTION

The present article is the second part of a study of the habitation in the eastern part of the Dutch river area (fig. 54) from the Late-Iron Age until the earliest Middle Ages, the Merovingian Period. It thus covers approximately the millennium between 250 BC and AD 750, which follows from its purpose. Very briefly stated, that purpose is to collect and interpret the available evidence on society in the river area during the Roman Period. Obviously, this necessitates inclusion of the immediately preceding and subsequent chronological phases.

An introduction to the general context of the present study in the Eastern River Area (ERA) Project and a discussion of aims and methods has been provided in chapter 1. The subsequent five chapters of Part I are mainly restricted to a presentation and interpretation of basic data of various kinds. In chapter 2 historical information is used to define five major chronological phases in the millennium studied. These are the Late-Iron Age (c. 250-12 BC), the Early- (12 BC-AD 47), Middle- (AD 47-c. 270), and Late-Roman (c. AD 270-425) Periods, and the Merovingian Period (c. AD 425-750).

Chapter 3 is devoted to a discussion of the landscape and its reconstruction during the Roman Period. Chapter 4 is concerned with the chronology of the different sites and the basic typology of different kinds of sites as used on Appendices 1-5 (see below, under technical remarks). The information about all individual sites has been assembled in chapter 5, which is a catalogue listing finds, literature, and other relevant information. Finally, in chapter 6, a number of find categories is presented in more detail from varying points of view. These range from a concern with social or economic significance to a limited discussion of chronology or provenance.

In this second part, the information from chapters 1-6 is further analysed. Chapter 7 mainly provides some data which are additions to or corrections of those in Part I on the basis of recent research and some new finds. It also intends to facilitate as much as possible the independent use of Part II.

Chapter 8 is devoted to the typology and interpretation of the sites in the five differentiated chronological phases. One of these sites, the Roman fort at Arnhem-Meinerswijk which was discovered as a result of the present study, is treated separately in chapter 9. The remaining three chapters are an attempt to draw together the information presented and analysed in the foregoing chapters and to integrate it with other relevant data into a coher-

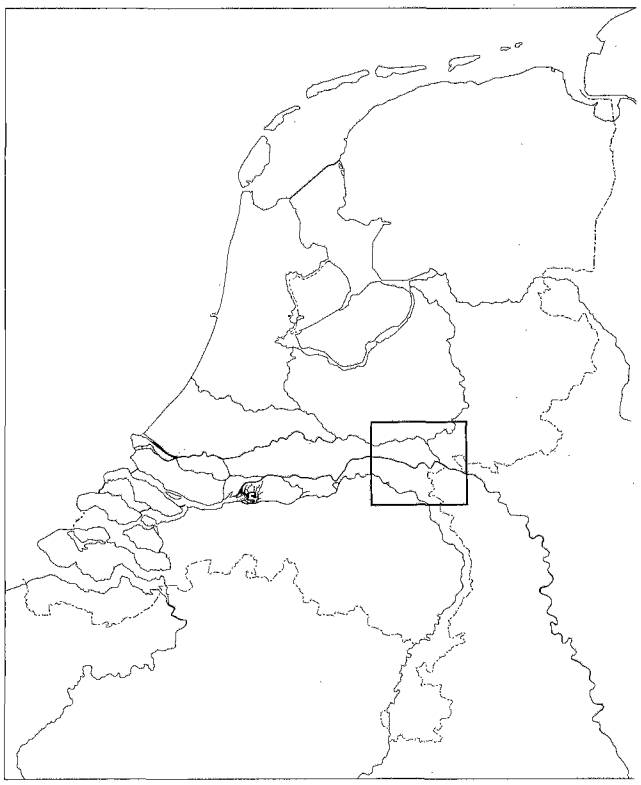


Fig. 54 Location of the eastern river area.

ent picture of developments in the river area between c 250 BC and c. AD 750. However, equally important as th synthesis of all information into a multi-dimensional bu still largely descriptive 'occupation history' of the regio is the attempt, in addition, to analyse the processes un derlying the observed developments or patterns, or a least some of them.

Within limits, archaeological, historical, and geologic data and other information can be used to formulate an sometimes test a number of hypotheses about the work ings of society in the river area. These can be illuminate by – and sometimes perhaps shed light on – socio-cu tural processes at higher levels of analysis. After all, pec ple in the river area were not an autarkic group but sub jected to influences of a far more encompassing natur But this is as far as we can go. The unavoidable and ac ditional self-imposed limitations outlined in chapter and later have defined the present study as research a the (micro-)regional level: to collect the available infor mation and to explore its potential and shortcomings. T thereby serves as a pilot-study for the ERA project and a

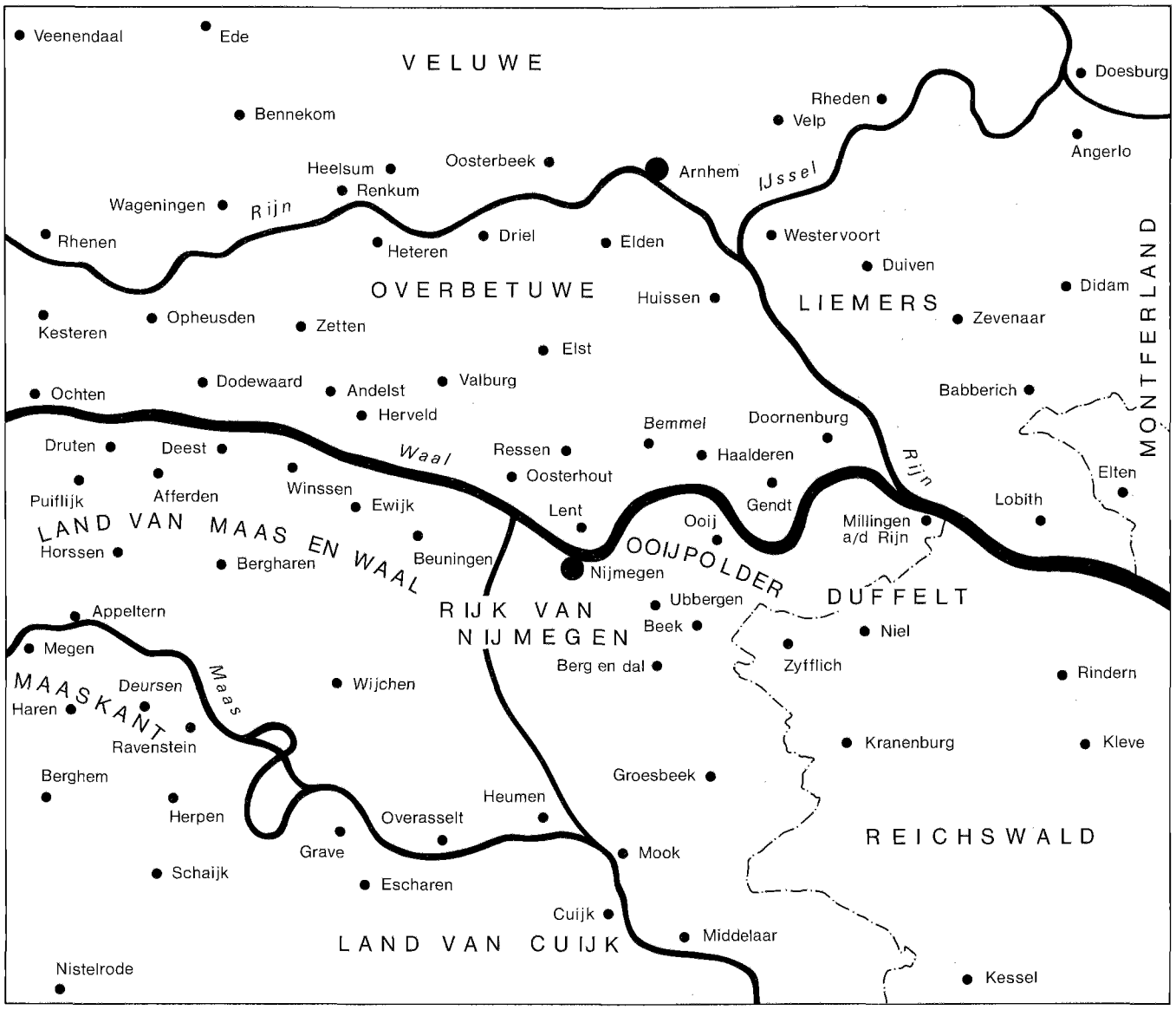


Fig. 55 Place names and the different regions in the eastern river area. Scale 1:250,000.

one of the elements for a comprehensive supra-regional approach which could be spatially defined as the basins of Scheldt, Meuse, Rhine, and Ems (see Part I, fig. 4).

Technical remarks

The first part of this study was published in the *Berichten ROB* 31, 1981, p. 7-217. In order to avoid cumbersome cross-references, that article is not cited but referred to directly by chapter, page, and figure numbers. For this reason, chapters and figures have been numbered consecutively throughout the entire study, which is also published as a separate book. For references to chapters 1-6 and figures 1-53 readers of BROB 34 therefore need to consult BROB 31; references to page numbers

of chapters 7-12 in BROB 34 always indicate the number between brackets.
To facilitate the use of the present article, the most important information about the modern topography of the river area has been indicated on fig. 55. More details can also be found on Appendices 1-5 of Part I. But these are maps primarily designed to present the distribution of the different types of sites during the five phases against a reconstructed geological background. They are at the same time sheets of the *Archaeological Map of the Netherlands 1:100,000* published by the ROB. Figs. 60-64 below are abstracts of these Appendices.

7.1 ADDITIONS TO THE CATALOGUE

After the publication of Part I several new findspots were discovered in the eastern river area and even more new finds were reported from known sites. That was to be expected, and in most cases the new information does not contribute important additions. Therefore, in this paragraph only those new finds or sites will be mentioned which are relevant to some of the issues discussed in the following chapters. They are an addition to chapter 5 and presented in the same way (for abbreviations see chapter 5, p. 95).

199 Lathum-Lathumse Waard
Finds (*l*) late-Roman bronze hairpin (see fig. 56)
Collection Museum Zevenaar
Other data isolated find

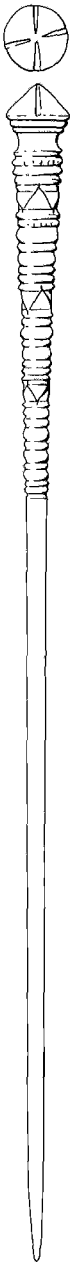


Fig. 56 Late-Roman bronze hairpin from th IJssel at Lathum (site 199). Scale 1:2.

544 Ooij-Stenen Kamer
Situation a.s.s. (not recorded by soil survey)
Finds (*a-e*) x, (*l*) glass bottle, (*m*) x
Collection GAS, private
Literature JROB 1983, 112-3; JAWNN 1984, 9-13
Other data settlement

545 Oosterhout-Verburgtskolk
Situation a.s.s. (not recorded by soil survey)
Finds (*b*) Br 5, (*c*) xx, (*d*) St 109A, St 109B, xx, (*h*) illegible round stamp probably VEXEXGERINF, (*j*) xx, (*o*) Bö B, Bö D 9-12
Collection Historische Kring Oosterhout
Other data settlement

546 Huissen-Zwanewater
Situation Finds in secondary location
Finds (*b*)xx, (*c*) xx, (*d*) Br 36, (*e*)Nb89, xx, (*h*) LX[G], ḶXG and LEG[on tegula, (*j*) xxx, (*k*) x, (*m*)Bö D 9-12
Collection private
Literature JAWNN 1984, 16-7
Other data isolated find

547 Driel-Marskamp II
Finds Drag 18/31, stamp CANAIM, (*e*) Gose 485
Collection private
Other data burial

548 Escharen-Esterveldlaan I
Excavation ROB 1981-3
Collection ROB, private
Literature Verwers/Beex 1978, 36-8; JROB 1983, 60-2
Other data settlement

549 Escharen-Esterveldlaan II
Excavation ROB 1982
Collection ROB
Literature JROB 1983, 61
Other data burials

7.2 THE LANDSCAPE AND ITS POTENTIAL

In chapter 3, the genesis and structure of the landscape in the eastern river area and various related issues were discussed in detail. It is not the purpose of this paragraph to summarize or otherwise repeat what has been said there but to discuss a number of necessary additions or corrections. Notably some additional evidence on the nature and extent of the fluvial system during the periods covered by the ERA project, which has only recently become available, allows improvements to the reconstruction of the landscape presented earlier (fig. 13).
A second major subject is a short overview of the potential of the landscape for the subsistence of its inhabitants. This is intimately related to the subject of chapter 3 but was deleted in the first part of this study because it has a direct bearing on such matters as carrying capacity, subsistence strategy, and economic change which will be discussed here. Data on the actual – as opposed to the potential – use of the landscape provided by archaeological and palynological analysis and the presently available archaeozoological and palaeobotanical information, are included in the following chapters.

7.2.1 Geology and Reconstruction

As was to be expected, new information that became available after the publication of the Eastern River Area sheet of the Archaeological Map of the Netherlands (Appendices 1-5), in 1982, has resulted in a number of corrections in the form of changes or additions, but also in confirmation of arguments. Furthermore, two important studies which were then still in preparation are now available. These are the publication of sheets 39W and 39O of the Geological Map of the Netherlands,[1] and of the geological development of part of the central Dutch river area.[2]
Both studies provide a wealth of information relevant to most or all of the subjects treated in chapter 3 and to the geology on the Appendices; this is based on manuscripts provided by both authors and thus needs no vital corrections. The other additional information or necessary changes concern two subjects. First, there are several issues related to the map which are an addition to paragraph 3.2. Second, there is the new information which necessitates further comments on the reconstruction of the fluvial system during the period investigated.

Additions and corrections
For the area north of the Rhine and IJssel, the discovery of site 542 has demonstrated that the area between Nude and the Rhinedike (c. square 171/440), left blank on the Appendices, should be included in the pre-Roman and Roman channel deposits.[3]
For the area between Rhine and Waal (the *Overbetuwe*), the presence of site 545 proves that south of Oosterhout (c. square 185/431) these same channel deposits are still uneroded beyond the dike under the present water-meadows. This phenomenon may, however, be of local significance and cannot be bordered or extended to other areas along the Waal. In addition, the discovery provides a context for the finds from the nearby site 155, interpreted as a probable cemetery.
For the area between Waal and Meuse (*Land van Maas en Waal*), the presence of sites 367 and 534 near Heumen (c. square 186/420) and a reconsideration of the geological evidence[4] have led to a different conclusion than that which is incorporated in the map[5]. The Pleistocene fluvial deposits between Nijmegen and Wijchen can indeed largely be considered a flood-basin, but an exception should have been made for a small area from Molenhoek to Heumen indicated by Pons as 'high reddish brown river terrace soils' (Sh 1-3 soil series). These should be counted among the (Pleistocene) 'fluvial deposits with a

1 Verbraeck 1984.
2 Havinga/Op 't Hof 1983.
3 Cf. chapter 3, 40, note 54.
4 See Pons 1957, 1966.
5 See chapter 3, 32 and 42.

relatively high-lying surface' on Appendices 1-5. The area concerned has been indicated in the new reconstruction of the landscape as presented in figs. 58 and 59. In the area south of the Rhine and Waal (*Ooijpolder* and *Duffelt*), two important new finds have confirmed the somewhat uncertain interpretation of the geological situation. The location of the clearly eroded settlement near Kekerdom (site 533, square 197/431) shows that the deposits there are indeed post-Roman. Even more important is the discovery of site 544 near Ooij (square 192/458). This undisturbed site was found as a result of clay extraction from deep pits. The site consists of two stratigraphically differentiated settlements, one from the middle-Neolithic Michelsberg culture and one Roman. The Neolithic site shows that, in contrast to the earlier interpretation,[6] the stream-ridge is originally very old and was probably partly rejuvenated during the Bronze or Iron Ages when bank deposits covered the Neolithic site. On those same deposits, the Roman settlement was established and, in fairly recent times,[7] covered by bank deposits from the present Waal. Further to the south, there are also flood-basin deposits. The actual situation is thus more complicated than expected, but the new discovery demonstrates that the geological situation during the Roman Period must have been as indicated. These findings also show that a survey, such as proposed for this area,[8] may indeed produce significant results.

Another addition to our information about the same region is provided by the results of new geobotanical research in the Kermisdahl near Kleve[9]. The stream-ridge from Kleve to Millingen is very deep and stratigraphical observations have shown that Roman levels are only 1 m below the surface. The deep stream-ridge must have been largely formed by redeposited material eroded by the Kermisdahl meander-curve from the Pleistocene soils south of Kleve in pre-Roman times. The geological situation as indicated on Appendices 1-5 is thus largely correct, with one important exception, namely, the pe filled channel there. It was indicated as such because was assumed, on the basis of the possible presence o Roman road on the inside of the meander-curve[10], t the channel must have been fossil in Roman times. Th however, is not true. A C14 dating of clay-gyt (1810 ± 80 BP; GrN 12603) is a first indication for th but even the date obtained is too old.[11] Palynologi analysis of the channel fill has conclusively demo strated that the channel only started to fill up in t Late-Middle Ages, presumably in the 12th century AD These findings are, by the way, in agreement with t fact that the 9th-century border between the dioceses Utrecht and Köln is the same channel which was th still a river-course.[13]

An important second conclusion from the now availat information must be that the road, designated by G rissen as Roman[14], cannot have been the *limes* road b cause it was located on the eastern bank of the Rhi during the Roman Period. This does not entirely excl de the possibility that the road, known as *Steynpad* (St nepath) in AD 1394, is Roman,[15] but it is more probat that the undated (!) gravel road was in fact construct much later as a connection along the successor to the R man Rhine, the so-called Alter Rhein. This interpret tion also removes the difficulty that the *limes* road wou have passed by Qualburg (site 460-461) at a distance 2 km.[16] Moreover, the site can now be situated on t bank of the Rhine, which is also more in accordance wi expectations because Quadriburgium, with which it c probably be identified, is reported to have been refor fied in AD 359 to protect transports of British grain the Rhine.[17]

Another Roman road should also be mentioned aga here, namely, that from Cuijk to Nijmegen. As di cussed, the road presumably crossed the Meuse Cuijk,[18] but no reference was made to the positive e

6 Chapter 3, 51.

7 The site has also yielded early- and late-medieval finds.

8 Chapter 3, 75.

9 Cf. chapter 3, 44, and note 85. New information provided by Dr D. Teunissen, Nijmegen.

10 Gorissen 1952, 49 and fig. 37.

11 It is another example of the unreliable results of gyttja-datings mentioned earlier (chapter 3, 48-9, and notes 107 and 109). See also below, 207–8.

12 Research included in a doctoral paper by Mrs J. van der Sman, University of Nijmegen, Dept. for Biogeology.

13 Gorissen 1935, 4. The medieval boat with querns from the Rindern-Spyck harbour (site 452), connected to the sa channel, is another indication.

14 Mentioned in chapter 3, 44 and 64.

15 E.g., a secondary road from the Rindern fort across t river to some specific important site beyond the *limes*. Th such sites did exist is shown by the example of the *Tegula Transrhenana*.

16 On Qualburg, a late-Roman fort and quite possibly a *st tio* of some sort before that time, see below, 260 and 308.

17 Ammianus Marcellinus XVIII 2, 3-6.

18 Chapter 3, 64.

dence for this in the form of wooden posts in the present-day river which may have belonged to a (late-?) Roman bridge.[19] A radiocarbon date of 1715 ± 35 BP (GrN-6006) confirms this interpretation.[20]
A last addition to chapter 3 which should be mentioned is the presence of Roman (and later) settlement sites near Ravenstein and Neerloon, in the Maaskant area south of the Meuse (squares 172/422 and 174/422). These illustrate what was already indicated in the discussion of the maps,[21] namely, that the inadequate depth of the borings for the soil maps which had to be used as a basis for the map here prevents a satisfactory reconstruction of the still uneroded (pre-)Roman deposits. Most of the area south of Ravenstein, left blank on the maps, should evidently be included among the 'pre-Roman and Roman channel deposits', but a reconstruction cannot be attempted until a geological survey is carried out here.

The Roman Period fluvial system.
The major Roman river-courses as reconstructed in chapter 3 do not need to be reconsidered here. The system of functioning rivers and their courses is sufficiently known in outline, although ongoing research, such as described above, has added new detail to the general picture and will do so in the future. Apart from the major rivers there is, however, the question of the fluvial system in general, encompassing various other streams, and especially the degenerating channels of pre-Roman rivers or the new courses reaching maturity only in later times.
In chapter 3,[22] attention was paid to the fact that more channels must have been to some degree still functioning than could be indicated and that some of the available C14 datings were not reliable. A reconsideration of these data, and notably the implications from palynological research, had to be awaited in order to reach further conclusions. A highly illuminating study by Teunissen now allows some more concrete interpretations.[23]
On the basis of pollen diagrams Teunissen, following De Jong,[24] concludes that C14 dates of especially gyttja-like sediments tend to be too old. Both authors agree that the assimilation of C14-free CO_2 from geologically old calcareous rock by the organisms in the gyttja is the most likely explanation. As a result, some channels can now be shown to have remained open much longer than could hitherto be assumed on account of the radiocarbon dates. This is true for nos. 3 (Oude Zeeg), 7 (Oosterhout), 8 (Ressen), 9 (Baal), 10 (Homoet) in fig. 15, for the above-mentioned date from the Kermisdahl deposit,[25] and probably also for a few others.
All this does not mean that during the period studied one should reckon with many more functioning rivers – in the sense of major channels – than assumed previously. On the other hand, several fossil channels which were, except for the channel between Driel and Elst, considered to have been little more than small brooks or drainage gullies and thus deleted from the reconstruction (fig. 13) must now be assumed to have been still open and navigable streams. As was to be expected, the careful analysis by Teunissen has also shown that there are chronological differences between the various channel-fill deposits. These are relevant not only as indications for differentiation between various branches but, in combination with geological data and the behaviour of river systems, also lead to differentiation between different trajectories of the same system.
The degeneration of a river-branch normally starts with the silting-up of its upper reaches, slowly choking the channel, while this same channel remains open much longer downstream.[26] This process is amply documented for the Lower Rhine and also the IJssel, which were only preserved as functioning rivers due to human interference.[27] With reference to Teunissen's more elaborate evaluation it can be concluded that the principal pre-Roman Lower Rhine system developed into such a state of degeneration. The major Roman Period branch was undoubtedly that along which the forts of Herwen-De Bijland (182), Loowaard (194), and Meinerswijk (126) were situated but its geological age is quite young. It can now be demonstrated that its predecessor(s), with branches from Haalderen and from Ressen to Elst and from there to Driel as well as over Homoet towards

19 Cf. Bogaers 1966, 70.
20 Lanting/Mook 1977, 169. The date is rather early, but this is presumably due to the fact that the sample came from the core of a post (pers. comm. from Dr D. Teunissen, who submitted the sample).
21 Chapter 3, 44-5.
22 See esp. 45-52 and note 107.
23 Teunissen 1984.
24 De Jong 1983a; 1983b.
25 See note 11.
26 See e.g. the remains of the Oude Rijn north of Lobith on the Appendices (topography).
27 The developments are extensively documented, with many illustrations, by Van de Ven 1976.

Randwijk, were still open and navigable channels until the Late-Roman Period. Also, as presupposed in chapter 3, channels in the yet older stream-ridge Herveld-Zetten-Randwijk were as a rule presumably still carrying water.

As demonstrated by their characteristic plant communities, most of these channels remained open until the Late-Middle Ages but their upper reaches had silted-up much earlier, between AD 300 and 700. This implies that the process started in approximately the beginning of the Late-Roman Period, and was contemporary with the Duinkerke II marine transgression phase (AD 250-600). It may well have been a sequence of events with entirely natural causes, although this is not necessarily true. As Berendsen has shown,[28] the formation and degeneration of river branches is a highly complex process. At least in the marine and perimarine areas there is a relation with sea-level changes and presumably climatic factors, but this relation is such that the formation of new branches is closely connected to transgression phases while their degeneration and silting-up accompanies regression phases.[29]

This evidence cannot be extended to the eastern river area without additional research, but it does indicate that it is not necessary to accept natural causes unconditionally and look no further. There are at least two good reasons why the upper reaches of the older channels would have been kept open artificially, if that were necessary, thus preventing those branches from degenerating much earlier than from AD 300 onwards. These are the fact that they surely provided useful waterways but also the fact that any additional water entering the Lower Rhine this way must have been most welcome, not only in Drusus' day but also later on.[30] That the organization behind such work could no longer function in the Late-Roman Period or that the work was no longer relevant or necessary (because of climatic change), or all of these circumstances together may then have allowed the start of the process of silting-up.

Of course, there is no decisive evidence to prefer either the 'natural causes' or the 'cessation of human interference' hypothesis, although the latter will need further evidence. In any case it is quite certain that up to the 3rd century the older channels were still a functioning part of the Rhine system and thus provided a direct and navigable route from Nijmegen on the Waal to the Lower Rhine, over Elst to Driel and Randwijk.[31] This fact calls attention to another aspect discussed in chapter 3, namely, the presence of the Drusian mole at the fork of the Waal and the Roman Period (easternmost) branch of the Lower Rhine. It is now necessary to evaluate this choice of location, because in view of the new information the construction could also have been built more to the west, close to Nijmegen. After all, in Drusus' time its purpose was merely to divert more water to the Lower Rhine.[32]

The answer must be that its actual position was technically the best location to achieve optimal results. However, in chapter 3 evidence was presented in support of the upper course of the IJssel as the Drusian fosse. If this evidence is accepted,[33] the location of the mole is in fact the only possible one to achieve the desired effect. We know too little about the precise condition and water transport capacity of the different channels to use this as additional evidence for the IJssel as the Drusian fosse, but it is nevertheless worth mentioning. The more so because, in the absence of an IJssel route, the probable early camp in Driel (site 117) becomes an even more acceptable interpretation, but the location of the certain early camp in Meinerswijk (see chapter 9) is then no longer readily understandable.

As regards the IJssel problem, there are two other new publications which should be mentioned here, namely, those by Verkerk and Heidinga concerning its function in post-Roman times.[34] Heidinga quite correctly takes the position that in principle archaeological and historical information should be employed to make deductions about the way in which the river functioned, independent of the geological arguments. In this way, and nota-

28 Berendsen 1982, chapter 4.

29 Berendsen, *op. cit.*, 83-4.

30 See chapter 3, 52-5.

31 Which is, by the way, another argument in support of the proposed early-Roman camp (site 117) in Driel and also of the as yet purely hypothetical fort of Randwijk.

32 There is a strategic advantage of the easternmost branch over the others in the *limes* concept, and thus from Claudian times onwards, but not in the context of an offensive campaign which only needed a reliable waterway to the north.

33 There is, at the moment, new unpublished material which again provides support for the proposed interpretation. A new boring in the Giesbeekse Broek again provided strong evidence against the 'dry period hiatus' devised by Poelman (cf. chapter 3, 60) and the pollen diagram, with four C14 dates in the critical trajectory, clearly shows an early-Roman date for the beginning of the IJssel with rapid sedimentation until the 4th century, much less from then until c. AD 1000, and an increase again afterwards. Pers. comm. Dr D. Teunissen.

34 Verkerk 1983, esp. 19; Heidinga 1984, esp. 238-40.

bly using the – in his view – late-9th century *portus* of Deventer on the IJssel as evidence, he concludes that the IJssel cannot have provided a suitable waterway during the 7th and the early-8th centuries. The conclusion incorporates a dangerous element of reasoning *ex nihilo*[35], but it is nevertheless possible. As noted by Heidinga, it implies nothing about the condition of the river in earlier periods.

Support for Heidinga's view is perhaps supplied by Verkerk, on the basis of an older itinerary in the mid-13th century recorded by Albert von Stade, and describing the route from Rome to Denmark. This route explicitly takes a course from Xanten over Arnhem, Utrecht, and Staveren while Deventer or Kampen are not mentioned. If the itinerary was indeed very old when it was recorded, and datable to the 8th century at the latest, it could be further confirmation for a period in which the IJssel was not an important waterway. On the other hand, the indicated route may well represent a positive decision and not a negative one, because there is every reason to expect pilgrims to travel by way of Utrecht if they had a choice!

Neither approach to the problem thus provides reliable evidence, and a source stating explicitly that the IJssel was or was not navigable is lacking. All we have are inferences which suggest that in the earliest Middle Ages the IJssel was not an important route. Although uncertain, this viewpoint is plausible, but it can have no bearing on the discussion of an earlier IJssel route.

That it does indeed not imply anything about the situation more than seven centuries previously is also shown by the evidence for the 17th- and 18th-century situation at the Lower Rhine/IJssel fork. At that time the upper course of the IJssel was virtually silted-up (fig. 57), much to the distress of the Province of Overijssel and the towns of Zutphen, Deventer, and Kampen.[36] The situation was remedied by additional waterworks, but it is symptomatic for the troublesome situation there caused by a rather unfavourable angle of bifurcation of the IJssel. Of course this evidence cannot be projected unconditionally into earlier periods, but at least the general information about the fluvial system and the relation between Lower Rhine and Waal is remarkably similar in

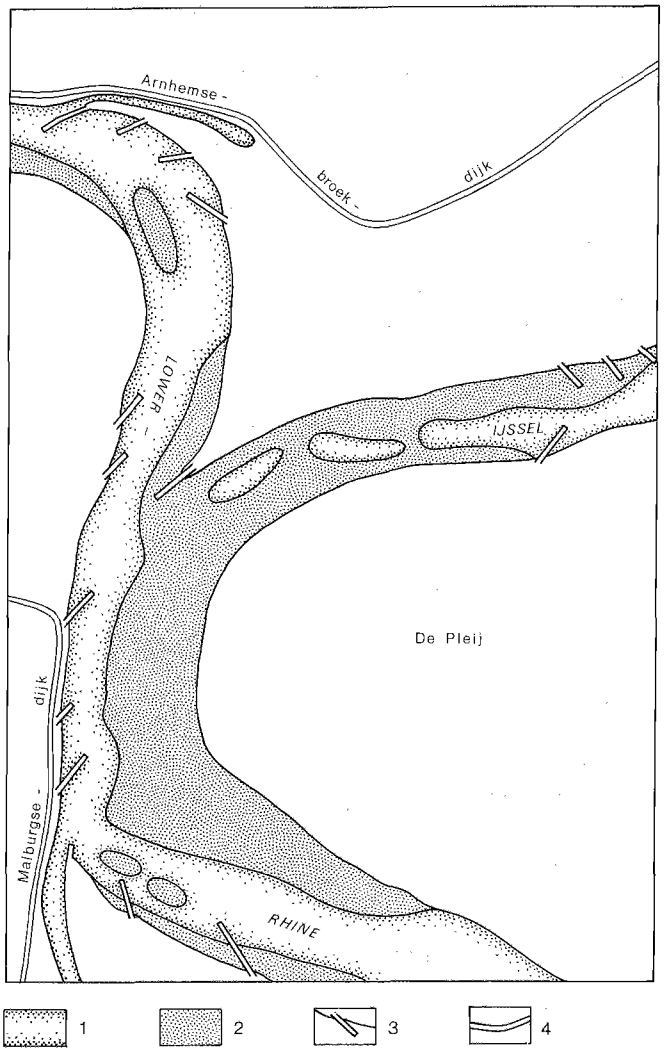


Fig. 57 The bifurcation of Lower Rhine and IJssel in August, 1705: 1 water, 2 dry sand-bank, 3 groyne, 4 dike. After Van de Ven 1976, fig. 25.

Roman sources and those of the 15th century and later mentioned by Van de Ven.

Thus the digging of a fosse or the amelioration of an existing insignificant overflow or basin drainage gulley by Drusus' troops, for which there are various kinds of evidence, should have resulted in a new Rhine branch. Perhaps this branch functioned only briefly, but it may have been of some importance for several centuries, depending on the extra water provided by the waterworks at the bifurcation of Lower Rhine and Waal. Because there is

35 This is illustrated by the fact that Heidinga's very recent study is already out of date where the early-medieval occupation of Deventer is concerned: see Van Es/Verwers 1985, esp. 28-9, on Merovingian occupation there.

36 See Van de Ven 1976, esp. chapter 11.

evidence that the river was of no real significance in Merovingian times it must be assumed that the branch had silted-up before or was in that process during this period, functioning only intermittently at times of high water. The sedimentation data mentioned above (note 33) support such a view. There may well be contemporaneity, if not common causes, with the degeneration of the other branches discussed previously. In addition, the branch of the Meuse in the Land van Maas en Waal (the so-called Wijchense Maasje) also silted-up in approximately the same period.[37]

With the new information it is now possible to refine the reconstruction of the landscape as presented in fig. 13 and along the lines already set out at the end of paragraph 3.3, and to include some minor corrections. As a general overview fig. 13 remains valid, but the chronological evidence is such that an overall picture of the entire period studied is even more difficult than it already was. Therefore, two new reconstructions are presented in figs. 58 and 59.[38] The first shows the situation from the Late-Iron Age to the end of the Middle-Roman Period (250 BC-AD 270); the second that at the end of the entire period covered by this study, in the last phase of the Merovingian Period (c. AD 700). For the Late-Roman Period the situation is somewhat uncertain, but the currently available data suggest that fig. 58 should be more relevant than fig. 59.[39]

7.2.2 Ecology and Land-Use

In his important treatise on archaeological cartography, Hallewas also devoted a section to the relation between characteristics of the geological landscape and its exploitation potential for specific forms of land-use.[40] His discussion drew attention to a relatively new and for archaeology potentially useful product of the geo-sciences, namely, the so-called land(-use) capability classification (*bodemgeschikheidsclassificatie, Bodenschätzung*). There are some obvious difficulties in employing present-day soil valuations for archaeological purposes but such data can nevertheless become a useful tool and the method b which they are obtained is also important. It gives in sight into the various valuation factors which, sometime to very different degrees, are relevant to forms of (pre) historic land-use as well.[41]

In principle, such information should be an importar factor in a discussion of the human ecosystem of a re gion.[42] In practice, however, difficulties arising fror lack of information about groundwater-levels, tempera tures, and the like only allow a still rather general an undifferentiated valuation of soils. When the suitabilit of the geological deposits in the river area is estimate with regard to arable farming and stock-breeding, th most important valuation factors are 'drainage' an 'moisture supply and nutritional condition of the soil for arable the factors 'relief' and 'crumbling capacity c the soil' should be added. Based on the data compiled i the soil survey studies,[43] the following generalizatior can be made about the deposits indicated in the legend c Appendices 1-5 (on a scale very-moderate-limited-not)

Deposit	Suitability for arable	Suitability fo pasture
Channel deposits	moderate-not	very-modera
Bank deposits	very	very
Flood-basin deposits	not	moderate
Brook deposits	moderate-limited	moderate
Low-lying Pleistocene fluvial deposits	limited-not	moderate
High-lying Pleistocene fluvial deposits	moderate-limited	very-moderate
Low-lying coversands	limited	very
High-lying coversands and fluvio-periglacial deposits	moderate-limited	moderate-limited
Loess	limited	limited
Fluvioglacial deposits and ice-pushed ridge	limited-not	not

37 Cf. chapter 3, 62.

38 Note that the legend symbols 3 and 4 now have a slightly different meaning. This was necessary because most river branches were originally indicated as channel zone deposits while others can now only be drawn as channels or channel deposits. In order to arrive at a balanced picture the only solution proved to be the use of channel deposits, the extent of which is often known or can otherwise frequently be estimated on good grounds. The two alternatives are unacceptable because the precise location of major channels can only rarely be indicated and to indicate the entire channel zones of the largely pre-R man stream-ridges would severely distort the reconstructic by overemphasizing already less important branches.

39 It is not impossible that future research will show that tl change from fig. 58 to 59 can be related to the end of the Dui kerke II transgression phase or, perhaps better formulated, the beginning of a new marine regression phase (and its gener causes) around AD 600. But this is no more than speculation.

40 Hallewas 1981, esp. 226-9.

It appears from this table that, as an overall estimate, channel zone deposits are in all respects the best soils while the high ice-pushed ridges and related deposits are virtually good for nothing, with all others in between. It should be realised that the indications given here may not be transferred directly to other areas because they are partly based on the recurrent and frequent combinations of deposits and particular groundwater-levels in this area only. Elsewhere, the same deposits may normally be associated with other levels, which could significantly alter the valuation.[44] At the same time, this warning indicates the limited absolute value for the situation during the period studied: we do not know what the levels were then and, moreover, we can be quite certain that they changed to a smaller or larger degree during that time. Therefore, the valuations are only applicable in a relative sense, to indicate suitability of one type of deposit versus another, and even then they are not without problems.

Also directly related to the question of soils and what humans could do with them is the matter of the natural vegetation associated with different deposits. The situation in the past and developments therein, for the period under discussion mainly those under human influence, can be evaluated by palynological research. The results of such research, as far as available, will be discussed wherever appropriate in the following chapters. But, apart from that, it is also useful to have some idea about the potential natural vegetation in addition to what can be said about the agricultural potential. It is, of course, almost impossible to do this by way of reconstruction, but it can be done by way of the 'potential natural vegetation' concept in ecosystem research.[45] This is based on the knowledge of different standard-types of plant communities and their requirements, and proceeds by assuming complete cessation of all human activity and immediate succession, that is, the development of the vegetation to its natural final stage (climax), on the basis of present-day soil, climatic, and other conditions.

Obviously there are many problems with such an operationalization of the concept,[46] but it still does produce a general idea of what the potential vegetation should have been like on the various deposits. From the Vegetation Map of the Netherlands scale 1:200,000,[47] which gives an overview of the potential natural vegetation, the following conclusions can be derived for the eastern river area.

The Holocene or fluvial deposits are dominated by the main vegetational series of the *Alno-Padion*,[48] the alder-bird's cherry association (*Elzen-Vogelkersverbond, ooibossen, Auenwälder*). A finer distinction is possible for the Holocene deposits, which are dominated by three different associations of a sub-series, that of the oak-ash forest abounding in elm (*Ulmion carpinifoliae, iepenrijke eiken-essenbossen*). The channel and bank deposits are characterized by the *Fraxino-Ulmetum* (ash-elm forest, *essen-iepenbos*) or the *Anthrisco-Fraxinetum* (ash forest, *fluitekruidrijk essenbos*), or a mixture of both, primarily depending on the calcium-content of the soil. Forests like this have a very species-rich undergrowth of shrubs and herbs. On the driest parts of these deposits, notably the highest-lying stream-ridges, oaks may have grown in relatively large numbers.

The flood-basin deposits, peat, and brook deposits are typically dominated by a second major sub-series, the *Circaeo-Alnion* (moist alder-ash forests, *vochtige elzen-essenbossen*), with alder, ash, and willow as the main trees and a dense undergrowth of herbs and grasses.

41 Hallewas 1981, table 1.

42 Cf., for example, the studies by Welinder (for a summary, see Welinder 1984).

43 STIBOKA 1973, 1975, and 1976 which each contain a chapter on the agricultural possibilities and an appendix with valuations of different soil types combined with groundwater-levels. For the terminology on deposits used in the text below, see chapter 3, p. 29-39.

44 Other factors may, of course, also be important. Probably the best example is the loess (or *Sandlöss*) area which is not very suitable for agricultural activities in the region studied for various reasons, including relief. There are, however, a few places with this deposit where the valuation is 'very', and in other regions, such as the loess-belt from Belgium over the southern Netherlands into the German Rhineland, the overall valuation of loess is bound to be much higher.

45 See Kalkhoven a.o. 1976. The same issues are also treated (in German) by Tüxen 1956 and Trautmann 1972.

46 These are extensively treated in the literature (cf. note 45) and include the problems of sometimes very drastic human interference in the landscape or the unerasable presence of exotic vegetation.

47 Kalkhoven a.o. 1976, Appendix 1 (two sheets).

48 Vegetational series and syntaxonomical terms after Westhoff/Den Held 1969; for a short overview of characteristics of the different sub-series, see Kalkhoven a.o. 1976, paragraph 2.2.3 and Appendix 3.

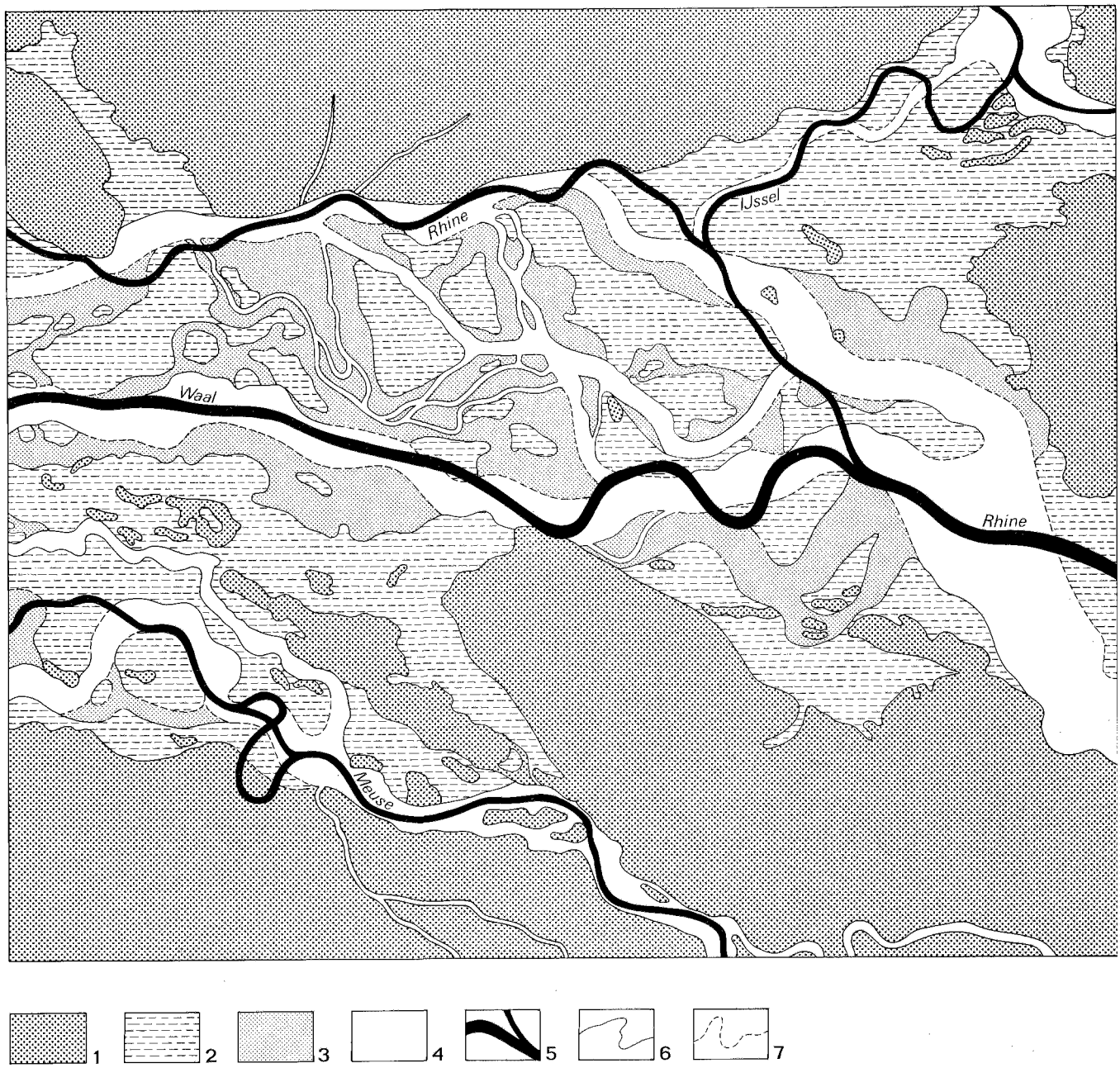


Fig. 58 A reconstruction of river-courses during the Late-Iron Age and Roman Period: 1 Pleistocene deposits, 2 flood-basin deposits and peat, 3 bank deposits and pre-Roman channel zone deposits, 4 estimated channel deposits (meander-belts) related to active river branches, major brooks, and some of the small and in part still active fossil channels,[38] 5 present day river-channels, 6 boundaries of deposits, 7 reconstructed boundaries of deposits. The reconstruction in this way is much more tentative than the general picture presented in fig. 13. Scale 1:250,000.

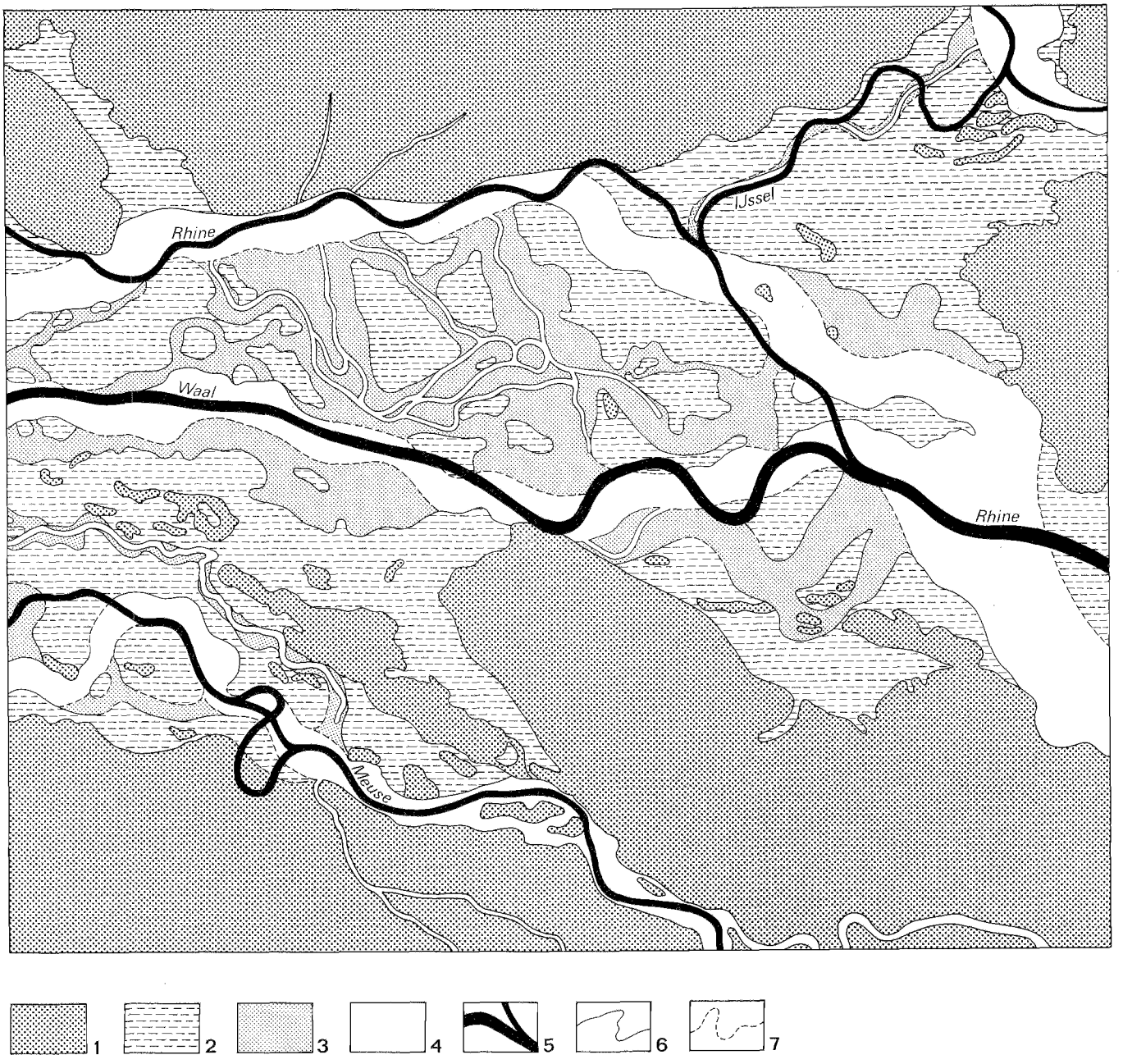


Fig. 59 A reconstruction of river-courses during the Merovingian Period: 1-7 see fig. 58. Scale 1:250,000.

On the Pleistocene deposits the *Quercion robori-petraeae* (oak association) is dominant. These are little stratified and species-poor climax-vegetations. The most general sub-series is the *Fago-Quercetum* (beech-oak forest, *beuken-eikenbos*), which occurs on the loess and mineralogically richer coversands and fluvio-periglacial deposits. In some areas, notably on the ice-pushed ridges, a variety of the same kind of forest, but with less species, may be found, while the mineralogically poorest areas (especially the fluvioglacial deposits) have an oak-birch forest (*Querco roboris-Betuletum, eiken-berkenbos*). This may, however, not be a true climax-vegetation but the result of human exploitation and poorness of the soil due to the removal of biomass in subrecent times.

On the basis of the foregoing, it is possible to make some general statements about the theoretical potential of the eastern river area for various ways of land-use. This potential has no direct relation to a situation in the past, because that will be determined by additional factors such as previous human interference, the nature of society and its economy, or technological constraints and capabilities. But there is at least the general framework of what is possible and what not, and where there are restrictions or limited options. This can be contrasted with the available ecological and archaeological data in the following chapters.

As an overall conclusion, it can be said that the actual present-day river area is entirely and eminently suitable as a stock-breeding area. All deposits are at least moderately but often even very suitable for pasture, but the more natural vegetation associated in the *Alno-Padion* is, in principle, also quite suitable, especially for cattle.[49]

For arable farming, there are fewer options because this cannot be practised in the flood-basins nor even on some more shallow bank and channel deposits. The overall suitability for arable of these, and the brook deposits, is highly variable. Only the high bank deposits are, as a rule, quite well suited for arable.

Other exploitation possibilities should also be mentioned here. One of these is wood which can be used a fuel but some species, notably oak (from the highes parts of the stream-ridges), ash, and elm also for con structional purposes.[50] The same is true for resource such as reed and rush. Another and conceivably quit important contribution are the fish resources which, in delta area with three major rivers and numerous sma streams, are very considerable indeed.[51] In additior birds,[52] small game such as beaver and especially hare and large game such as roe deer, elk, and perhaps wil boar and red deer should also be counted among the re sources of the river area.

The Pleistocene deposits offer a different picture. Cov ersands with a low-lying surface and fluvial deposits ar often quite suitable for stock-breeding and this can als be done on the higher coversands and loess areas, but considerable degree of human interference with the nat ural vegetation of the *Quercion robori-petraeae* is needec which should result in a development towards *Nardc Callunetea* (matgrass and heather communities). Stock breeding on the ice-pushed ridges and fluvioglacial de posits is not very likely but not entirely impossibl especially when it is considered that on the Pleistocen deposits in general sheep may be kept more profitabl than in the riverine area. The *Quercion robori-petraeae* also eminently suitable for feeding pigs.[53]

Most Pleistocene deposits can to some extent be used fc arable, but the better areas are restricted to fluvial de posits and coversands with a relatively high-lying sur face, the rest is rather or even very marginal in this re spect. Manuring or long periods of fallow are necessary Apart from being an obvious source for fuel and timbe and some small game, the deciduous woods will hav housed large game such as red deer en roe deer, elk, wil boar, and perhaps aurochs, but only boars may reall have thrived there unless the woods were fairly open a a result of human exploitation.[54]

For the river area as a whole, a few mineral resourc should be added to this picture of the resource potentia

49 The damp environment, with much open fresh water, may be less favourable for sheep because of certain parasites (cf. Prummel 1979).

50 See e.g. Casparie/Swarts in Van Es/Verwers 1980, chapter 23. Trees such as alder and willow were not preferred, at least not in the Early-Middle Ages.

51 For an overview, with further references, see Prummel 1983a, 235-6. For archaeological evidence, see paragraph 6.6.4 (p. 197-201).

52 See Prummel 1983a, chapter 5.4. An overview of birc and other wild animals in the eastern river area during the Rc man Period will be provided by R. Lauwerier (see chapter note 8).

53 Cf. Prummel 1979, esp. 96.

54 See Jochim 1976, chapter 8, for overviews of preferre habitats and possible densities; also Clason 1977. The scull an aurochs was found in the Roman frontier fort at Woerde (Bogaers/Haalebos 1983, 309).

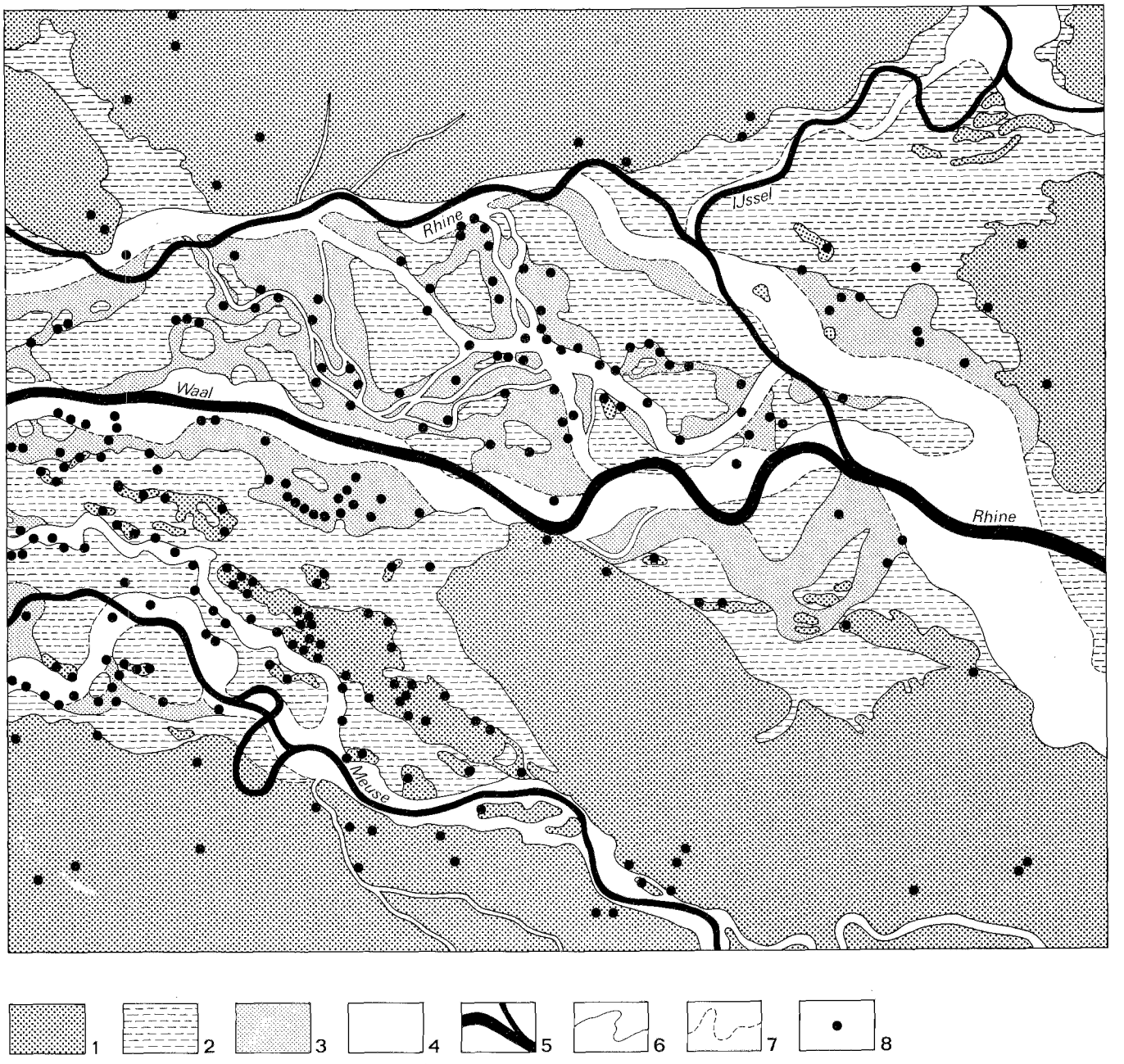


Fig. 60 The distribution of late-Iron Age settlements (c. 250–12 BC) in the eastern river area. After Appendix 1, with additions: 1–7 see fig. 58, 8 settlement. Scale 1:250,000.

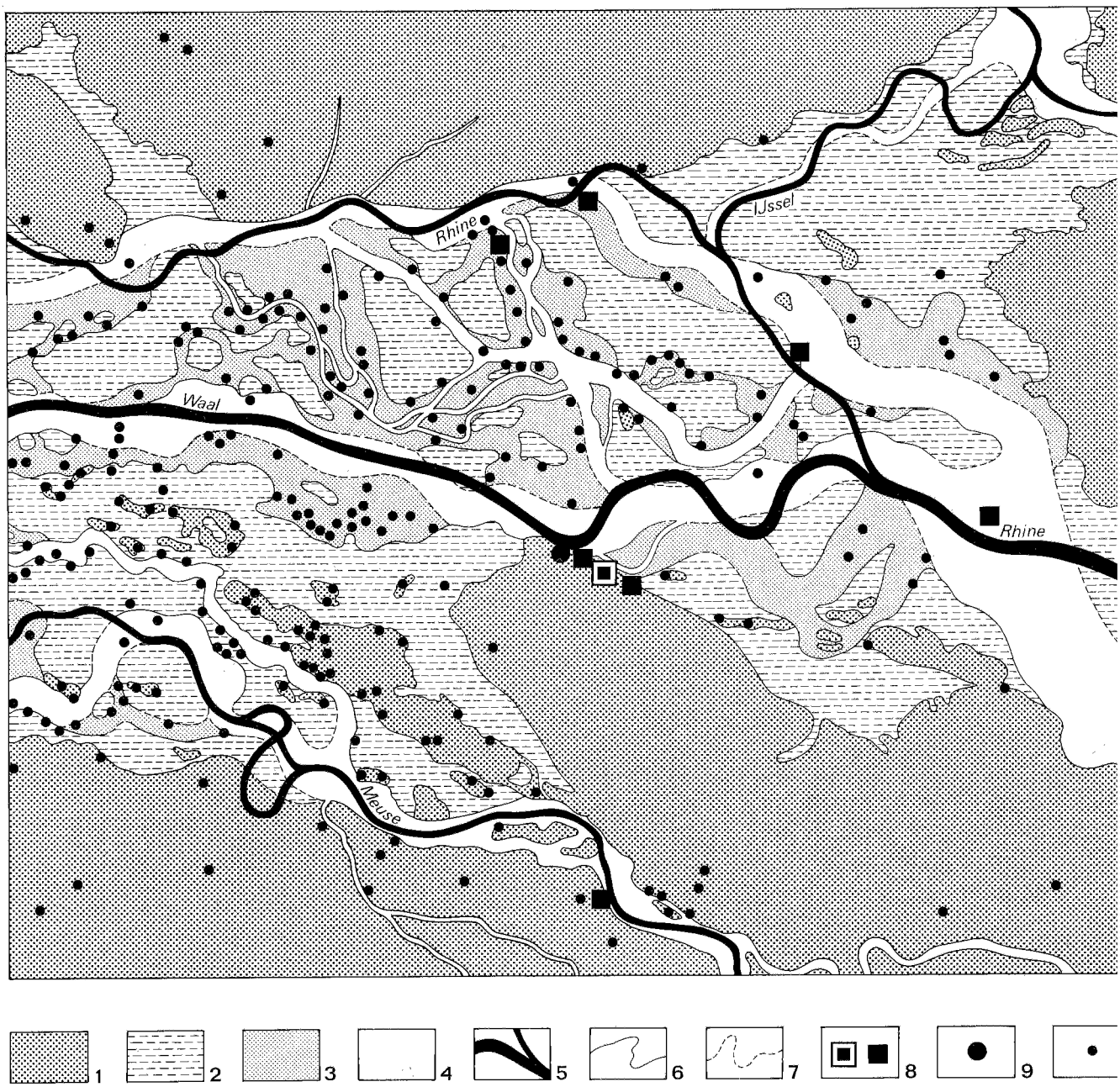


Fig. 61 The distribution of early-Roman settlements (12 BC–AD 50) in the eastern river area. After Appendix 2, with additions: 1–7 see fig. 58, 8 legionary camp and other (possible) camps, 9 large settlement, 10 settlement. Scale 1:250,000.

The Holocene and Pleistocene clays are the most obvious of these, but not the only ones worth mentioning. There is also the possibility for exploiting bog-iron ore, beds of which occur in the area studied and in very large numbers to its immediate south and northeast.[55] A third resource, exploited increasingly in recent years but also in the past,[56] is the gravel which is available in enormous quantities in the Holocene as well as Pleistocene areas.

A final topic to be discussed in the present context are the surface areas of the different deposits with their varying resource potential. Unfortunately, these data cannot be derived directly from the geology on Appendices 1-5, because it is relevant to know the extent of these areas during the period studied. Therefore, the eroded areas have to be considered as well, on the basis of a reconstruction such as presented in figs. 13 or 58-59.

The total surface of the study area is 1,650 km², of which 764 km² are Pleistocene and 886 km² Holocene deposits. In Roman times the Pleistocene area must have been larger but this concerns mostly low-lying fluvial deposits later covered by Holocene basin clay and is thus less relevant. Only a small part of the Pleistocene area was eroded in later times, but the surface (mostly coversands) concerned cannot have been large. The total eroded area ('Post-Roman channel deposits and present river courses' on the legend of Appendices 1-5) is 306 km². On the basis of the reconstruction of the Roman Period landscape, it can be concluded that 70% of that area consisted of channel zone deposits,[57] 25% was a flood-basin, and not more than 5% a coversand area. For the entire region, this results in the following estimated total surface areas:

1	Channel zone deposits	281 + 214 =	495 km²
2	Flood-basins	257 + 77 =	334 km²
3	Peat and brook deposits		42 km²
4	Low-lying Pleistocene fluvial deposits		25 km²
5	High-lying Pleistocene fluvial deposits		50 km²
6	Low-lying coversands		45 km²
7	Coversands, loess, and fluvio-periglacial deposits	457 + 15 =	472 km²
8	Fluvioglacial and glacial deposits		187 km²
		total	1650 km²

The major problem in the above overview are, of course, the channel zone deposits. These comprise the pre-Roman channel and bank deposits but also the bank deposits and the levees belonging to functioning rivers in Roman times and, not to be forgotten, the entire water surface. The present-day water surface of Rhine, Waal, Meuse, IJssel, smaller streams such as the Niers and Graafsche Raam, and of the various brooks, residual channels (*strangen*), and the like, can be estimated at some 50 km². The major rivers, however, have artificially straightened, constrained, and rather deep channels. In prehistoric and historic times before dike-building and other measures were effective, the rivers were meandering rivers which were not so deep, with many oxbow lakes and also alternative channels (cf. fig. 58). This implies that the total water surface must have been considerably larger although this conclusion is mitigated again by the fact that the average discharge at the major inlets in the region, the Rhine and Meuse (currently estimated at 2200 and 250 m³/s respectively), should have been less. This is mainly because in the catchment area upstream the drainage efficiency today is higher and the vegetation cover less.[58] The extent of the entire river-system, however, is also related to the maximum discharge. As a conclusion, we may therefore assume that the total maximum water surface in Roman times must have been larger than today, perhaps between 60 and 70 km², while the minimum may have been very much less, around 30 km² or even smaller. The typical situation is a small summer-bed in the wider winter-bed of the main channel with other channels dry or partly filled with stagnant water (see fig. 57!).

With the exception of most of the actual levees and part of the bank deposits, a very substantial part of the Roman channel zones can be expected to have had a rather low-lying surface and clayey structure. Several parts of the river-system were still relatively young or even (the IJssel) at the very start of their development),[59] and frequent flooding must have restricted the land-use potential. An exposed site such as the fort in Meinerswijk (no. 126) illustrates this because it could only be maintained by artificial raising of the soil, and evidence for repeated flooding is quite clear (cf. chapter 9).

55 Cf. Brongers/Woltering 1973, figs. 2-3.
56 See chapter 6, 184 and note 235.
57 See chapter 3, 29.
58 There are more factors involved, and all are interrelated in a complex way (for a summary discussion see Doeglas 1973), but a full discussion would serve no purpose in the present context.
59 See chapter 3, esp. paragraph 3.

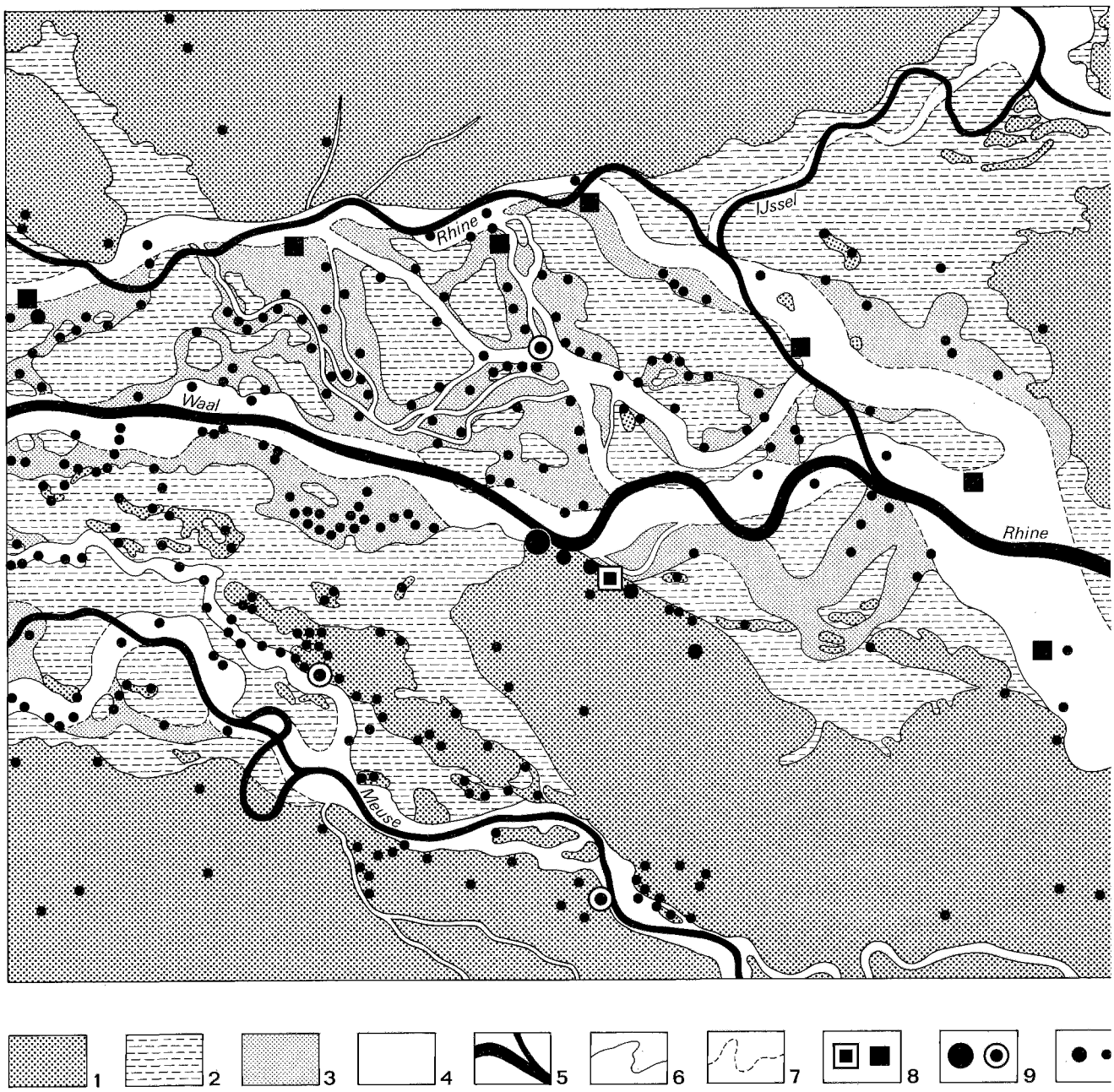


Fig. 62 The distribution of middle-Roman settlements (AD 50–270) in the eastern river area. After Appendix 3, with additions: 1–7 see fig. 58, 8 legionary fortress and (possible) frontier fort, 9 regional centre (*civitas* capital) and secondary centre (*vicus, pagus* capital), 10 military *vicus* and settlement. Scale 1:250,000.

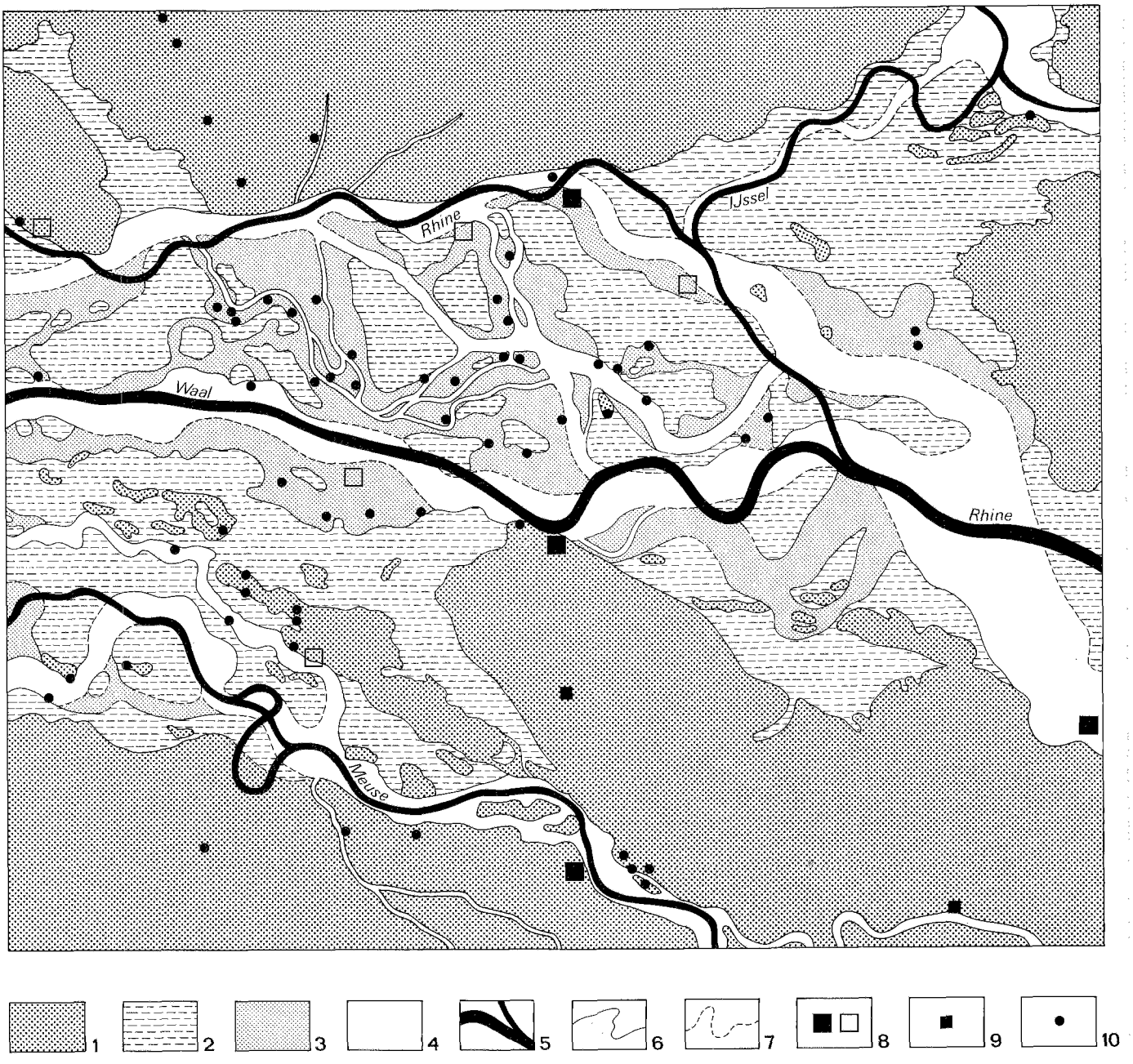


Fig. 63 The distribution of late-Roman settlements (AD 270–c. 425) in the eastern river area. After Appendix 4, with additions: 1–7 see fig. 58, 8 fortified (military) settlement and possible stronghold of unknown status, 9 small fort (*burgus*), 10 settlement. Scale 1:250,000.

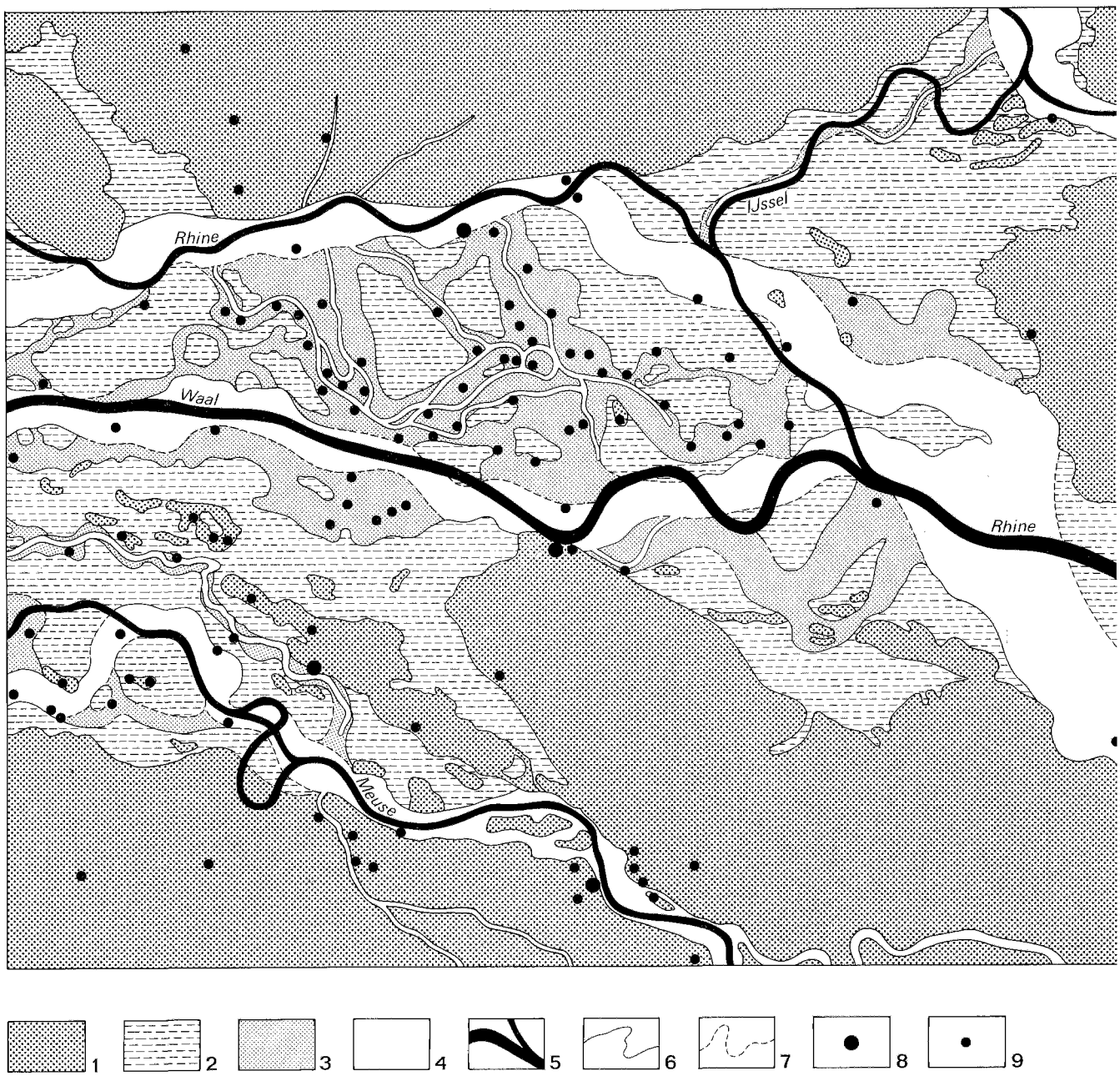


Fig. 64 The distribution of Merovingian settlements (c. AD 425–750) in the eastern river area. After Appendix 5, with additions: 1–7 see fig. 58/59, 8 (possible) large settlement, 9 settlement. Scale 1:250,000.

Although, as mentioned in the previous paragraph, the pre-Roman channel zone deposits had more still functioning channels than was hitherto assumed, these cannot have imposed comparable restrictions. Only the lowest parts of these stream-ridges may have been quite wet during the annual flooding of the flood-basins, but then such flooding, which leaves a layer of fertile mud, has its advantages.[60]

From the above considerations it may be concluded that of the total channel zone area of nearly 500 km² some 60 km² consisted of channels sometimes partly and sometimes entirely filled with water. Of the various channel and bank deposits by far the largest part, at least 300 km² (namely, an estimated 90% of the stream-ridges and at least 25% of the deposits along functioning rivers), may be considered to have been composed of relatively sandy clay with a relatively high-lying surface. The remainder, approximately 140 km², were sometimes much more clayey and had a lower-lying surface.

These data are, of course, only relevant for some forms of land-use and hardly or not at all for others. They influence the natural vegetation potential but they do not have a great effect on an activity such as cattle-breeding. They are, on the other hand, very important for settlement location and could be anything from very to hardly relevant for arable farming, depending on the kind of crop, technical capabilities, as well as the size of the population and the nature of its economy.

Indicative for this are even the land-use data from the last century or so. On the first topographical maps (mid-19th century), so much land suitable for arable was indeed used as such that the map gives a quite reliable picture of the geology: stream-ridges emerge when arable is mapped. A similar procedure is entirely impossible with recent topographical maps and this agrees with current land-use figures of 83% pasture v. 17% arable,[61] which gives 735 v. 151 km², and in fact even less,[62] for the study area. This change may be attributed to economic developments, because better drainage conditions and increased technological means could easily have increased the arable surface. Without going into any details it can thus be illustrated that the land-use potential of the river area is only a framework within which actual land-use may vary considerably. Especially the degree to which the region is tied into larger socio-economic networks and the demands or impulses coming from there determine what actually takes place.

7.3 THE DISTRIBUTION OF SETTLEMENTS

An overview of the distribution of settlements during the five differentiated chronological phases is presented in figures 60–64. These figures are in fact abstracts of Appendices 1-5 and designed to give a general overview of the habitation in the eastern river area. Other types of sites such as cemeteries and isolated finds have been deleted as well as most of the differentiations indicating the quality of the evidence for individual settlement sites. The different types of settlements, as far as indicated, are based on the discussion in chapter 8 and are therefore in some cases slightly different from the legend on the Appendices. However, not all settlement types discussed in chapter 8 are indicated by separate symbols.

On the other hand, the number of settlements indicated is larger than on the Appendices. It includes the settlement sites from site no. 530 onwards listed at the end of chapter 5 and in paragraph 7.1, and also sites not included in the catalogue of which descriptions are stored in the CAA.[63] Therefore, the settlements indicated on figures 60-64 represent the total numbers known in December 1984, when the inventory of new sites was terminated.

The geological background is also an abstract from the Appendices, but in this case the simplified and to some degree speculative reconstructions from figures 58 and 59 have been used.

60 Cf. Pons 1957, 135.

61 Haans 1979, 104-5.

62 The built-up areas and water surfaces should be deducted first.

63 The Central Archaeological Archives. See chapter 1, note 27.

8 TYPOLOGY OF THE SITES

8.1 INTRODUCTION

It is the purpose of this chapter to examine more closely the different sorts of sites referred to in general terms in the preceding chapters. This is, of course, a necessary step in the analysis of archaeological data from the region. It is also a critical step, because the conclusions reached thereby determine to a large extent the outcome of the entire study. The classifications made here are important building blocks for any reconstruction or model of (aspects of) the occupation of the region during each of the periods treated and for any hypothesis on diachronic developments. Also, the degree of differentiation which can be reached is not only informative in itself. It points at the same time to shortcomings in the present study and the available data, and may therefore direct further research. It definitely determines the interpretational limits to which the present evidence may be pushed.

For these reasons, and also because a description of several particular (and some types of) sites should be provided anyway because they are new interpretations, a detailed discussion is presented in this chapter.

For the period between c. 250 BC and c. AD 750, 543 sites have been recorded in the eastern river area.[1] In relation to the legends in Appendices 1–5, the basic assumptions and interpretations leading to a site typology were discussed in paragraph 4.2. These include two fundamental levels of interpretation, namely:

a establishing the contemporaneity of sites, resulting in a division over five periods, and

b establishing the general type of activity, resulting in a division in settlement sites, burial sites, and isolated finds.

When an attempt is made to achieve further levels of interpretation, there are several issues which have to be dealt with for each period and general type of site. The first concerns measurement. Attributes of sites may be measured both qualitatively and quantitatively. Qualitative measures are essentially deterministic and result in statements such as: when the settlement has artefacts, structures, or other attributes, of type A, then it is a settlement of type B.[2] Quantitative measures are probabilistic and lead to statements like: when a settlement has a high number (or preferably > x%) of type C artefacts, is larger than y ha, or something else, then it is a settlement of type D.

A second and related issue is whether the measured attributes indicate differences in degree or in kind. There is no problem with qualitative measures, for these are only used because they (are taken to) indicate differences in kind. But quantitative measurements have to result in more or less arbitrary decisions when no clear-cut divisons are apparent. Although there are statistical techniques to help in this procedure, this is a universal problem when the staircase model is used instead of the slope model.[3] There may, for example, well be an approximate continuum of settlement sizes, but when the size-criterion is used, it is necessary to divide this slope into different steps, each as a threshold for a new settlement type.

The third issue, namely the availability of historical data (and how to deal with them) could provide at least a partial solution to the slope-step problem. This would be the case when such data provided 'external' evidence which could be used to indicate thresholds. In practice, however, the historical information does not contain unambiguous data of this sort. On the contrary, it can be used primarily to obtain qualitative instead of quantitative information, if it can be used at all. The latter restriction is important, because Roman concepts should

1 To this number should be added another 19 sites, located south of the Meuse in the Province of Brabant. These were known during the first compilation of the catalogue (chapter 5) in 1980, but could not be incorporated (see chapter 1, 14). Some are, however, indicated on figs. 60–64. Furthermore, nearly 100 finds from unknown findspots, with uncertain find-circumstances, and the like, have been left out. A number of these must have been found on sites recorded in the catalogue, the association no longer being reliable or even lost. Additions and corrections to chapter 5 are listed in chapter 7, which brings the total at 549 sites.

2 An example is discussed in paragraph 6.5.1, namely, the statement that the occurrence of brick with military stamps (in a primary context) on a settlement site inevitably implies that the site is military. This example indicates at the same time that qualitative measures are not completely deterministic (which is almost necessarily true for all social sciences; see, e.g., Galtung 1967, 321–3) because the fact that the measure is not (completely) effective in the river area does not imply that it should therefore be rejected altogether. This normally has to be done with a completely deterministic statement, where one case that does not confirm is enough to falsify the entire statement (hypothesis).

3 See Claessen 1980.

not be employed casually to designate types of sites. They are not always straightforward and their use may introduce more difficulties than are solved.

All these matters need, at least in principle, a different approach for the successive periods, because it is known beforehand that there will be more to differentiate during the heyday of the Roman occupation than in other periods. What is also known is that, certainly during part of the Roman Period, many of the sites in the river area can only be properly evaluated against the background of a much wider superstructure. This is not only true for military settlements, but also for a settlement type like the villa. As Rivet has shown in his admirable analysis of what exactly constitutes a Roman villa,[4] it is a type of settlement which is only understandable in a very wide context. These are special problems, which need to be incorporated in the discussion of what sorts of settlement can be discerned in the river area.

The regional network itself, however, will be discussed in chapters 10-12, devoted to the necessary integration of the settlement types into regional and supra-regional patterns and systems[5] and their development in the course of time.

The fact that sites are treated here more or less in isolation does not cause any problems because, after all, they are differentiated by mutual comparisons. It does, however, impose restrictions on the sorts of sites that can be used. At the very least, the sites that do not definitely belong to the period under discussion cannot be used at all, which rules out a lot of the 'possible' and 'probable' sites indicated on Appendices 1–5.

8.2 THE LATE-IRON AGE

8.2.1 Settlements

There are 155 settlements which have been dated with certainty to the Late-Iron Age. These are the settlements to be discussed here, but they do not represent the total number of inhabited sites. There are another 87 settlement sites which were probably or possibly occupied during this period, so the maximum number is 242. The true number will probably be closer to the latter figure, because it often turns out that when more material is available from a site, it contains finds datable to the Late-Iron Age. Also, the distribution is very uneven because virtually all of the sites are located in the river area proper. The distribution on the Holocene deposits may give a relatively complete picture, but this is definitely not true for the distribution on most of the Pleistocene deposits. Especially south of the Meuse and also north and east of the Rhine, the evidence is known to be deficient. In view of the fact that not all of the 87 uncertain settlements will actually date to the Late-Iron Age and that, on the other hand, quite a few sites still remain to be discovered and some have been eroded, an estimate of 250–300 for the total number of late-Iron Age settlements would seem to be in the correct order of magnitude for the area studied.[6]

The period in question is rather long, between 200 and 250 years, and except for the 44 or 45 sites which were already inhabited before the Late-Iron Age and continued to be occupied afterwards (one site, 228, yielded early- but no middle-Iron Age material), there is no reason to believe that they all existed during the entire period. This is indeed rather improbable, but unfortunately there are no means by which a finer chronological subdivision can be made. One could assume that the sites with only Iron Age material ended well before the beginning of the Roman Period, but this would have to remain unconfirmed. The same is true for the hypothesis that settlements with the latest late–Iron Age (La Tène D) material, combined with native and imported Roman Period finds, may have started only towards the end of the Late-Iron Age. Although in this case the presence of finds such as the glass bracelets Haevernick type 3a and 3b, which date to the last century BC,[7] can be interpreted as an indication, it does not constitute proof.

With all this in mind, the use of chronology for further differentiation seems to be very restricted. The only hypothesis which can actually be tested is that there may be a difference between the settlements with a long and continuous occupation and those without. There is, of course, other information which may be useful in this respect, as is discussed below. But it is important to realize that, apart from reasons why there could be differences

4 Rivet 1969.

5 For a poignant and very instructive indication of the difference, see Flannery 1976, 161–2. A system is defined as the set of rules that generates a pattern.

6 The true number is probably even higher as might be deduced from the figures for the 1st century AD discussed below, 232–3 and 394–7.

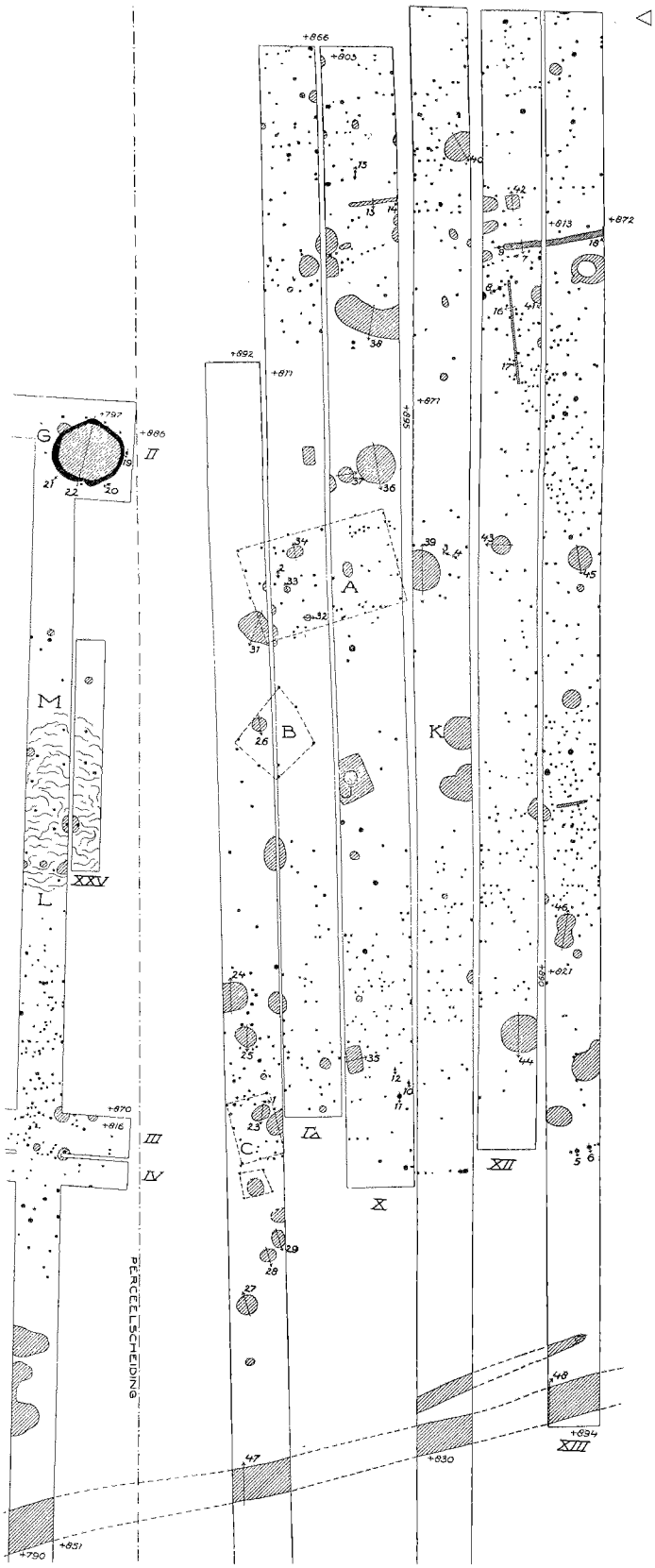
7 See Haevernick 1960; Peddemors 1975, 106–8; below, fig. 120 (distribution map).

between settlements, there are also reasons why these could be or could seem to be absent.
There may well be a high degree of uniformity, caused by actual uniformity in the past as well as by discriminatory factors which are not archaeologically traceable. Also, the landscape itself may prevent both the genesis and the discovery of significant differences. As for the first aspect, it is conceivable that the rivers, by altering their courses or by inundating certain areas, cause fairly regular changes in the localities which are most optimally situated in relation to traffic routes, agricultural potential, and the like. Regarding the second aspect, it is also conceivable that the rivers themselves were the prime location, and the situation is such that if the most important settlements were normally located on the banks of, or very near, the major rivers, it is very likely that they have been eroded in later periods. All these factors will make it difficult to interpret the results of an analysis of the settlement types of the Late-Iron Age.

The structure of settlements

Although restricted in usefulness, the chronological position, location, and sorts of finds are the only attributes of settlements which can be used in an analysis of individual sites. Data on relative size of settlements, on the type of buildings, their disposition, and the like, are very restricted. As was explained in chapter 3, the size of an ancient settlement soil cannot be used as a measure of settlement size. The only data on settlement sizes are those provided by records of the extent of surface scatters of pottery in the literature or in the CAA, and these are seldom present.
The same is true for data on the kind and number of the buildings in settlements, and on their internal structure. Intensive ploughing makes it virtually impossible to estimate, for example, the number of houses in a settlement from surface scatters, which means that excavation or remote sensing methods[8] are the only potential sources of information.
There are a number of (partially) excavated settlements with recognizable late-Iron Age features. These are:[9] Ede–Maanderbuurtweg (4), Zetten–De Hoge Hof (75), Raayen–De Woerd (109), Bemmel–De Heuvel (143), Ressen–De Kerkenhof (150), Ressen–De Woerdt (151), Lent–Het Laauwik I (158), Ewijk–Ewijksche Velden (232), Wijchen–De Pas (302), Wijchen–De Berendonk (333), Alverna–Heumenseweg (351), Nijmegen–Kops Plateau (417), Berghem–De Lallenberg (473), Haren–De Spaanse Steeg (475), Deursen–Onze Lieve Vrouwenberg (479), Oss–IJsselstraat (487) and Cuijk–Padbroek (515). Notwithstanding this rather long list of 17 excavated sites, these can contribute information only in a general way because they had limited results or have not, or inadequately, been published.
The only investigated settlement north of the Rhine is Ede-Maanderbuurtweg. During the small rescue excavation at least one house-plan was discovered which could be dated in the Middle- to Late-Iron Age. The house measured 25 × 5.5 m and was partly three-, partly two-aisled.
Few such definite data are available from the river area, although the published excavation-plans and scattered remarks in the literature are very clear about one phenomenon, namely, the absence of recognizable house-plans in a number of cases. The excavations of sites 109, 333, 473, 479, and 515 are essentially unpublished, site 158 yielded Iron Age material but hardly any features, and the data on site 475 are mainly of a stratigraphical nature. These sites cannot be considered here. The excavations at sites 143, 150, and 302 were very limited, but they seem to indicate exactly the same as the larger excavations on sites 75, 151 (see fig. 65), and 232, namely, one or several virtually inextricable 'swarms' of post-holes, combined with a number of ditches, refuse pits, wells, or other features. As the excavator of site 232 put it in his annual report: 'The numerous features do form several groups, as is evident from the distribution of granaries belonging to these groups, but still the other elements within these groups remain beyond the reach of our perceptive powers'.[10]
Because as yet no 'post-hole swarm' settlement has been excavated entirely, it is not completely impossible that clear house-plans are actually present at the same sites. But observations such as those made in Ewijk (232) and similar phenomena in Zetten (75) or Heteren (93) indicate that this is rather unlikely. The excavation plans show a number of nuclei in the distribution of post-holes and the related features and finds indicate that these

8 These have so far met with only very limited succes on the Holocene river deposits.
9 For the literature, see chapter 5.
10 *JROB* 1974, 9. Comparable citations are provided by Van Es 1981, 167.



◁ Fig. 65 Ressen-De Woerdt (site 151), part of the excavation plan, after Braat 1949, *Kaart* 1. Scale 1:500.

must represent dwellings of some sort, which are extremely resistant to archaeological analysis.

A convincing interpretation of this striking phenomon is not readily available. Although found elsewhere, such clusters of post-holes occur so frequently in the river area that it can be considered a regional characteristic. Chronologically, it seems to be especially common in the Middle- and Late-Iron Age. Earlier settlements normally have recognizable house-plans,[11] but the post-hole swarms do continue into the Roman Period, as demonstrated by sites such as Heteren-Het Lage Land (93) and Heteren-Uilenburg (95).[12] On the other hand, they were never the only kind of settlement. Middle- to late-Iron Age houses have been excavated at site 351 near Wijchen[13] and a continuous native tradition of house-building is also demonstrated by the plans excavated at sites 487 (Oss-IJsselstraat), 214 (Druten-Klepperhei), and other settlements outside our study-area.[14]

In order to evaluate the significance of the post-hole swarm type of settlement, two-factors of primary relevance have to be considered. First, that they are so characteristic for the river area. Second, the available evidence clearly indicates that these sites are not different from others in the sense that they represent the permanent residence of one or more families. Both statements require further discussion.

The fact that the post-hole swarms are characteristic for the river area should not be interpreted as a geographical restriction. They do occur elsewhere and in addition, as has inevitably happened in the past, one runs the risk of ending up with an unproductive culture-type 'explanation'. In this way, 'characteristic for the river area' has been interpreted as 'typically Batavian', the sites in question remaining an otherwise inexplicable 'cultural trait.'[15] Instead, it is more useful to consider that 'river area' in its geological sense means the area of Holocene river deposits and that it is on those soils that post-hole

11 See e.g. Beex/Hulst 1968, Hulst 1973a, Louwe Kooijmans 1974.

12 For a short report on site 95, see *JROB* 1983, 31–2.

13 Compare Verwers 1972, 87 and *Abb.* 57. The two-aisled houses in the settlement Wijchen-Holenbergseweg (Haalebos/Willems/Giebels 1976) near site 305 can be dated to the Middle-Iron Age only.

14 See Hulst 1978. Other examples are mentioned there (p. 145, note 13).

15 A good example is Braat's (1949, 34) disheartened conclusion that 'in the final analysis, *Batavian settlements* are never more than a tangle of post-holes'.

swarms occur so frequently.[16] The habitable parts of these deposits, the stream-ridges, are also very fertile.
This last circumstance has led to suggestions that the post-hole swarms could be not regular but special kinds of settlements: satellite settlements used for the exploitation of different natural resources during part of the year or central depots for storage of agricultural produce.[17] This interpretation is supported by the apparently numerous four- or more-post granaries visible on the excavation plans and the absence of regular house-plans. The latter is certain, because it is difficult to see how 'normal' house-plans can become completely unrecognizable even after a number of houses have been built on the same spot.
As long as detailed ecological studies providing evidence to the contrary are lacking, the 'storage-site' interpretation remains a serious possibility. It contains, however, two elements. The first of these is the idea that the unusual structure of the sites should not be explained as a typical cultural phenomenon but as the result of social and economic factors leading to a particular subsistence strategy. This is a very attractive hypothesis which, as will be discussed below, is supported by the available information. The other element, however, is the suggestion that the structure of the sites implies that they were not settelements, that is, that no people lived there or at least not permanently.
This suggestion is contradicted by the second primary consideration mentioned above, that they do represent permanent habitation. There are a number of reasons for this assumption. First, the increase in the number of sites and also the pollen diagrams[18] clearly indicate population growth during the Iron Age and large parts of the Holocene channel and bank deposits can be shown to have been permanently habitable from the Bronze Age or at least the Early-Iron Age onwards. Permanent habitability does not imply permanent habitation, but the large scale deforestation and increase of pastures and arable land cannot be understood when the post-hole swarms are not considered to be permanent settlements. As far as we know today, their relative number, although of course difficult to quantify, is too high and we would be left with too few 'real' settlements to account for the observed changes.

16 There are possible exceptions, such as sites 302 and 417 (for the latter site see Holwerda 1943 and especially Bloemers 1974 and 1978b; see also chapter 6, p. 181, note 212), but the excavated parts of these settlements are too small for definite conclusions.

A second reason is the fact that it is difficult to unc stand why special-purpose sites should have been cr ed unless most of the population was in fact concent ed in only a few large and centrally located settleme There is no evidence for these on the Holocene depo they were not necessary for environmental reasons a as will again be seen below, there is no evidence of a erarchically organised social system which could be pected in such a situation. In addition, there is the po bility suggested by the excavations in Ewijk (site 2 that at least sometimes a late-Iron Age post-hole sw was succeeded by a settlement with normal houses c ing the Roman Period.
An important third reason to consider them permar settlements is provided by the finds and features of post-hole swarms themselves. As already observed Van Es[19], the mere reconstruction of granaries leaves many post-holes unaccounted for. Moreover, as is dent from the citation on p. 224 above, the swarms as s are structured: they are composed of spatially diffe tiated clusters of post-holes, each with a number of ic tifiable granaries but also with other features such as fuse-pits, wells, ditches, and the like, identifying ther household-units. The quantity and the kind of fi from these sites is also not different from others. T have the same, large amount of pottery, sometimes (151) even evidence for pottery production, bo querns, and loomweights. In short, they have everyth one would expect on a permanently inhabited se ment-site, and we have to face the fact that they a regular form of permanent habitation and, thus, th must have been houses.
The houses are difficult to analyze, but that may be n a result of preconceptions regarding prehistoric ho which has led to a strong emphasis on recognizing v able but still highly formalized types of house-plan Dutch archaeology. To be sure, there are good reas for this approach and it has proved to be very effect nevertheless, because of this it creates difficulties w very simple, unusual, or irregular house-plans are c cerned. We are in trouble when there are too few tra such as in some middle-Neolithic settlements, when re are too many, such as the post-hole swarms, or w

17 This hypothesis was proposed by Van Es 1981, 169.
18 See Teunissen 1982. An overview of several diagran presented in his fig. 11. See also fig. 137, below.
19 Van Es 1981, 169.

we are not used to them, such as the possible circular huts on Bronze Age sites.[20]

The most likely interpretation of the post-hole clusters when they are an apparently normal form of permanent habitation is to assume that they represent a tradition of house-building which is more simple and either less formalized or formalized in a way which is archaeologically difficult to detect. After all, the designation as post-hole swarm or cluster is somewhat misleading, because there are still regularities present, such as straight or curved rows of post-holes, different rows at right angles, etc.

Viewed in this way, the post-hole cluster settlements from the river area are probably identical to very similar sites excavated in the adjacent German Rhineland. Recent excavations such as those in Weeze-Baal or Eschweiler-Laurenzberg[21] have provided quite similar plans from which not only four-post granaries and six-post granaries or barns, but also nine- (or more) post houses have been deduced. In his overview of late-Iron Age settlements in the Rhineland, Joachim[22] indicates that regular and formalized house-types such as those known from the Netherlands and northern Germany or the Haps-type houses are absent. Houses tend to be small, often 9-post, structures with a surface of only c. 15 m^2, but other and irregular configurations as well as larger one- to two-aisled houses (e.g., Grevenbroich-Gustorf[23]) also occur. They represent a tradition of house-building which, according to Joachim, is undoubtedly derived from southern Germany and which goes back to at least the late-Bronze – Early-Iron Age.

It thus appears that this tradition coexisted with others in the Dutch river area because there is not much doubt that structures such as described by Joachim can be found in our post-hole swarms.[24] In addition, it is not improbable that during the Late-Iron Age first-quality timber was no longer readily at hand due to the large-scale deforestation.[25] The use of lesser quality beams may have caused additional irregularities in plans which are difficult to recognize as it is, and which overlap as well.

Finally, as Joachim has also observed,[26] settlements with Eschweiler-type houses may testify to a different subsistence strategy, based on arable rather than mixed farming. At least for Eschweiler, this idea is also backed by ecological analysis.[27] It is further supported by the large number of granaries on these sites and it confirms the economic interpretation discussed earlier, at the same time reconciling it with the permanent habitation argument.

It is justified to propose, therefore, that the structure of settlements during the Late-Iron Age shows two different kinds of sites, due to a difference in house-building traditions which existed at least from the Middle-Iron Age onwards. The coexisting traditions are related to different subsistence strategies. The normal strategy is that of mixed farming, which is archaeologically visible in structures with a house and a byre under one roof. These occur in the river area,[28] but they are almost universal on the generally less fertile Pleistocene soils. They are highly formalized and only slowly evolving house-types which, for the period under discussion, have typical two- or partly two-, partly three-aisled plans in the river area.[29]

The very fertile Holocene river-clays, just as the equally fertile loess areas, allow a more specialized subsistence strategy of arable farming. Although animals are surely present and even necessary in that context, the relatively sophisticated house-and-byre structure is apparently replaced by a more simple house with spatially segregated structures around it. In this respect, it should also be remembered that the river area is not only suitable for specialization in arable farming. The large flood-basins are, in principle, eminently suitable for the opposite strategy, cattle-breeding. That situation can be con-

20 Beex/Hulst 1968.

21 Müller-Wille 1966; Joachim 1980. Location on fig. 80, p. 283

22 Joachim 1982.

23 Reichmann 1979a. For location, see p. 283, fig. 80.

24 The analysis of Eschweiler-Laurenzberg was greatly facilitated – and is thus more reliable – by the fact that it had only one occupation phase of relatively short duration. There were very few overlapping features.

25 This is shown by the pollen diagrams (cf. note 18) and undoubtedly caused primarily by increased agricultural activities, but additional factors, such as the need for fuel in the smelting of iron ore and other industrial activities may have been significant. On the other hand, it is certain that first-quality timber could still be obtained in the region, as is evident from the presence of little disturbed oak and birch-woods near site 417 in the second decade BC (Teunissen/Teunissen-van Oorschot 1980).

26 Joachim 1980, 336–7; 1982, 158.

27 Knörzer 1980, esp. 446–7 and 456.

28 For example, in the recently excavated settlement in Wijk bij Duurstede – De Horden (Van Es 1981, 169–70). Cf. note 14.

29 Van Es 1982, 144–5.

nected to the post-hole swarms as well, because large herds of cattle cannot be stalled in a normal byre. They must normally have been left in the open in all seasons, and their owners could suffice with building much simpler houses for themselves only. In this stituation, a large number of 'granaries' can also be expected, as storage facilities for fodder in the winter season.
Some further implications and evidence for the interpretation offered here are discussed in chapter 10.[30] It offers at least a potentially fruitful insight into the causes of diversity in settlement and house types in the eastern river area. This diversity cannot, however, be easily if at all related to artefacts and certainly not to those from surface collections. It can only be established after excavation and is thus impossible to employ in a detailed differentiation of all known settlements.

Chronology, location, and finds
As far as chronological differentiation is concerned, but also in view of what has been said above, it is important to check whether the settlements already occupied before the Late-Iron Age have recognizable house-plans. From the few excavated sites there appears to be no relation between a long pre-Roman occupation and the presence of house-plans. Sites 4 and 351 have them but sites 151 and 232 (and possibly 302 and 417) are of the post-hole cluster type.
Similarly, there is no relation between a long pre-Roman occupation and the location of settlements on Pleistocene v. Holocene deposits. Of the 44 (45) settlements which existed before, during, and after the Late-Iron Age (see chapter 4, fig. 23), 14 are located on Pleistocene and 30 (31) on Holocene deposits. The 14 sites should be augmented with another 10 examples which existed before and during the Late-Iron Age, which results in a ratio of 24:30 (31). It should be noted that all of the settlements which did not continue into the Roman Period (4, 6, 24, 184, 185, 288, 302, 350, 351, and 523) are located on Pleistocene deposits. But this fact does not necessarily imply anything about late-Iron Age settlements. It could just as well be relevant in the context of the Roman occupation or not at all.

As far as the finds are concerned, there is also very li evidence of differentiation available. There are a f sites which have yielded somewhat extraordinary ma rial such as bronze or iron La Tène fibulae (43, 209, 2 or pins (209, 270), bronze bracelets (209, 244), a bro buckle (296), and iron knives (360). Relatively late bronze Nauheimer fibulae (296, 349) and copper ra bow-cups (239, 244).
The glass bracelets discussed in paragraph 6.6.3 can be considered extraordinary in a qualitative sense, cause they have a very wide distribution (see fig. 120). a quantitative sense, however, the sites in the reg around Wijchen are remarkable for the large number bracelets found on them, a fact which was already served by Peddemors.[31]
The material evidence, thus, does not point to a sign cant association with long, continuous occupation with different geological deposits. In fact, most of evidence points, if at anything, to the near absence differentiation between settlements, except econo specialization. The few special objects, accidentally c covered on a few sites, may not be used to set these si apart from the others. The numbers of glass brace are more useful in this respect, because, whatever th interpretation may be, it is a reasonable assumption associate them with some sort of wealth. This does s not permit differentiation between individual set ments, because the data on different sites have been c lected in a way that is anything but uniform.[32] Bu does provide a clue in the sense that the region aro Wijchen and along the Meuse at this point (in gene the middle of the south-western quadrant of the a covered by the map) is wealthy, albeit in a very relat sense.
At this (micro-regional) level it is also evident that few exceptional metal finds are concentrated in the sa area and that most of the sites involved are situated Pleistocene or relatively old Holocene deposits.[33] Th sites may also be larger than others, because Appendi clearly shows clusters of them which may, in real have constituted only one settlement. After all, set ments are only interpretations of findspots. On

30 See p. 374–6.
31 Peddemors 1975, 105. Several sites can be added to his list, such as for example 275, with at least 37 bracelets.
32 It would be possible to name a few 'rich', but only in very rare cases to indicate definitely 'poor' settlements. The distribution pattern of glass bracelets is also distorted somewhat by a veritable collecting mania which arose especially in the L van Maas en Waal in the aftermath of Peddemors' (1975) s dy.
33 Especially the branch of the Waal around Ewijk and pr ably also the Wijchense Maasje. See chapter 3, 42.

Holocene deposits, indications for such large sites are lacking.

Conclusions

When the evidence discussed in this paragraph is evaluated, it is clear that it still leaves much to be desired. Nevertheless, what we do have is enough to reach at least some conclusions about what late-Iron Age settlements in the river area were like. The most important conclusion must be that in general they were all more or less the same and that differentiation is very hard to find.

The only very clear difference is that between the structure of two kinds of settlements on the Holocene deposits, one having technologically fairly sophisticated houses with a byre, the other with simpler and smaller houses almost indiscernible from subordinate structures around them. A plausible interpretation for this phenomenon is a difference in subsistence strategy and economic specialization on the latter sites. But except for the fact that these are especially characteristic for the Holocene river deposits, they are like the others in all aspects that can be considered; as far as we know they are of approximately the same size and yield the same finds, without material diversity.

Such material may be absent because it has not been found and the fuzziness of the evidence can partly be attributed to a lack of archaeological investigation. On the other hand, the large quantities of finds from a considerable number of sites over the entire region, in addition to the data from excavations, should have provided firmer arguments for differentiation than are available. It is, therefore, not at all improper to conclude that at least material (and thus archaeologically traceable) differences must have been rather marginal.

Although there is economic or functional diversity, there is thus no apparent inequality and higher-order settlements are not identifiable. Only of the area around Wijchen can it be said in general that sites there are somewhat 'richer' and perhaps larger. The available evidence thus does not offer any arguments for a settlement hierarchy but suggests that if there is differentiation it is in degree and not in kind. The relative importance of the settlements at the top of the (short) scale, those around Wijchen, increases – or becomes archaeologically better visible! – in the last century BC. But the overall picture remains that of equality, which would agree well with the historical evidence,[34] which does not suggest a highly stratified society.

There is only one factor which could change this picture significantly, namely, the destruction of important settlements by selective natural processes, especially the erosion of sites by rivers. The observed but fuzzy concentrations of wealth and trade contacts are located near major functioning rivers in the Late-Iron Age, and most of them situated at relatively high points. It is possible that the hypothetical most important settlements were located on – at that time also very high – points on the river banks. A site like 275 could then be viewed as an accidental survival, because the branch of the Meuse it is located on was cut off and did not develop further in later periods, while other settlements may have been completely eroded. In fact, there is even evidence for this further to the west, near Rossum and Lith. As Roymans and Van der Sanden showed, the finds collected during dredging at this strategic spot, where Meuse and Waal are very close together, point towards the existence of important settlements there,[35] notably to a late-La Tène centre which is quite probably a result of the Batavian immigration. It does, therefore, not prove the existence of important settlements on the river banks *during* the Late-Iron Age. But it does indicate that the process of erosion of settlements did happen and that at least one very important settlement was actually located there at the end of the Iron Age.

There is one other important and possibly late-Iron Age settlement which should be discussed here, namely, the famous settlement at the Kops Plateau (417) in Nijmegen. Between 1915 and 1921 J.H. Holwerda directed excavations there which were, for those days, rather extensive. He concluded that he had found the oppidum Batavorum mentioned by Tacitus.[36] Later excavations, which yielded an excellent stratigraphy, have shown that this interpretation is very unlikely.[37] In a recent review of all the available evidence[38] it has been concluded that the initial settlement, which starts around 10 BC, can be attributed to military occupation which lasted to c. AD 10. The oppidum Batavorum, which is tentatively identified as the equivalent of Batavodurum, is (also tentatively) located at the large early-Roman settlement on and around the Valkhof (site 403).

34 See chapter 10.
35 Roymans/Van der Sanden 1980; see also chapter 11.
36 Holwerda 1920; 1921.
37 Bogaers/Haalebos 1975; see also chapter 11.
38 Bogaers/Haalebos 1979a and Bogaers 1979a.

Whether these identifications are correct, remains to be proved. For the present discussion it is significant that both sites 403 and 417 are early-Roman. The available and very considerable amount of material from both sites includes virtually nothing that points to the latest phase of the Iron Age, in contrast to the eroded settlement near Rossum/Lith mentioned earlier. Both these sites can therefore safely be ruled out as possible major late-Iron Age settlements, although attention should be called to pre-Roman occupation near both of them. Late-Iron Age pottery which probably points to a settlement has been found at site 402. The southern part of site 417 is definitely a settlement; in the literature it is usually referred to as the 'Batavian village'.[39] The excavations in 1975 showed that this site was already occupied in the Iron Age. In neither case, however, are there indications that the settlements were in any way especially important, nor indeed of positive connections with the early-Roman settlements.

8.2.2 Cemeteries

Archaeological evidence of burials in the eastern river area during the Late-Iron Age is almost completely lacking. Because the area was definitely inhabited and people inevitably die, there is a problem of archaeologically untraceable burials during the entire period. There are several observations which are relevant to this problem.

First of all, it is not unique. At the beginning of the Iron Age, both in the northern and southern parts of the Netherlands, most if not all people were buried in large cemeteries, the so-called urnfields. Recent discussions of the evidence in both regions are provided by Kooi and Verwers.[40] There are several differences between these two regions, most notably the near absence in the north of wealthy burials[41] which occur in the south. These are also present in and near the river area (Oss, Wijchen, and possibly Ede), where large urnfields are also found.[42] The material evidence clearly indicates that the urnfields in the river area are directly related to the southern group, but this is of little importance for the present discussion.

It has been established that both in the south and in the north several urnfields continued to be used throughou the Middle-Iron Age,[43] although not in exactly the same way. There were several changes in the burial practices some of which may go a long way to explain why virtually no burials dating to the last two centuries BC have been traced so far.

In both areas, as well as elsewhere in adjacent Germany and northern Belgium, interments within square ditches were a new phenomenon in the Middle-Iron Age. Plans of excavated cemeteries show very peculiar combinations of round and square ditches, as well as burials without peripheral structures. It is significant that when native cemeteries become archaeologically visible again which is almost from the very start of the Roman Period the excavation plans show exactly the same phenomena (examples from our area are sites 396 and 491). It would thus seem logical to assume continuity of burial practices in the centuries in between,[44] although it remains to be shown why the burials have not been found.

An indication for this may be other changes during the Middle-Iron Age, notably the clear tendency to omit the use of urns and, even more significantly, of the grave-pit. Although the latter development has been demonstrated most clearly for the northern part of the Netherlands changes like this make it easy to understand why late Iron Age burials are not normally archaeologically traceable. Burials without urns or grave-goods are not noticed easily, those without a grave-pit are even less likely to be discovered.

Extrapolation from the Middle- to the Late-Iron Age leads to at least four different sorts of internment which are to be expected.

These are:

1 cremation in a pit surrounded by a peripheral structure
2 cremation in a pit
3 cremation and pyre remains within a peripheral structure
4 cremation and pyre remains

The presence of a barrow may be assumed for all possibilities but this need not be so (especially in cases 2 and 4) which introduces another source of variation. There wil

39 Holwerda 1943; Bloemers 1974 and 1978b.

40 Kooi 1979; Verwers 1972.

41 Kooi 1979, 181–2 and Verwers 1976. Two more recent discoveries in the southern Netherlands, in Horst and Venlo, in *NKNOB* 1980, 130–2 and 1981, 167.

42 See Modderman 1951, 30–45.

43 Verwers 1972, 34–7, 42–8 and 51–3; Kooi 1979, 133–4 and 174. A concise overview of the dating evidence for both areas is presented in Lanting/Mook 1977, 159–65.

44 This continuity has been demonstrated elsewhere, see e.g. Decker/Scollar 1962 and Wightman 1970.

undoubtedly be even more alternatives, but the important point is that none of them is likely to be discovered. Even if burials of this sort occur in older cemeteries, they cannot be identified as such after excavation, unless radiocarbon dates place them in the Late-Iron Age. In this respect, it is also noteworthy that, with the start of the Roman Period, cemeteries immediately or very soon regain visibility in the south, while their northern counterparts remain the same and can only gradually be identified more clearly.[45]

The few possibly or certainly late-Iron Age burials mentioned by Lanting and Mook are already sufficient explanation for the near absence (= invisibility) of such burials in the eastern river area. An additional case was recently reported from its immediate surroundings: a small group of cremation burials from Oss-Ussen, which was accidentally discovered during the large-scale excavations there, yielded a radiocarbon date definitely placing it in the Late-Iron Age.[46] There are, however, also at least a few possible burials from the eastern river area which should be mentioned here.

There are a number of sites (314, 324, 520, 521, and 522 in the catalogue) which refer to the large urnfields around Wijchen. These have never been archaeologically investigated, but large amounts of urns found on them were systematically collected by the local factory-owner F.J.G.H. Bloemen and partly published by Modderman. While most of the pottery can be dated to the Late-Bronze Age and Early-Iron-Age, there is also material from the Middle-Iron Age and a few Roman Period urns.[47] In the recently published catalogue of sites in the Land van Maas en Waal, the sites mentioned above are, however, also indicated as having late-Iron Age pottery.[48] It is conceivable, therefore, that some of the cemeteries do not have to be supposed but can actually be proved to have been continuously[49] used until, or occasionally even into, the Roman Period. The evidence presented in a subsequent study in preparation by Peddemors has to be awaited on this matter; the sites involved have been indicated as isolated finds on Appendix 1.

There are also three cemeteries, two of which are indicated as such on the map, which could conceivably contain late-Iron Age burials. The first is site 411, which was accidentally discovered during excavations in Nijmegen in 1974. Apart from a middle-Iron Age grave with the remains of spearheads, a horse-harness, and a waggon,[50] the cemetery consisted of simple pits with cremations. It is located immediately to the northwest of the early-Roman cemetery 409, which is probably a burial-ground related to the large settlement around the Valkhof (403). It is tempting to propose that the entire complex was used continuously from the Middle-Iron Age onwards, but for lack of datable objects or C14-dates this has to remain a (rather remote) possibility.

The second cemetery is site 490 near Uden in Brabant, partially excavated by the RMO in 1923.[51] The earliest pottery from this cemetery dates to the Early-Iron Age, the latest is early-Roman, Gallo-Belgic ware,[52] found together with two spoon-bow fibulae and other items. In the excavation report 38 burials are mentioned, of which 22 contain pottery. None of the burials was placed in a pit but all were surrounded by a ringditch. Again, it is quite conceivable that the cemetery was continuously used from the Early-Iron Age until the middle of the first century AD, but definite proof is lacking.

The third cemetery, if it can be called such, is a collection of 'insignificant cremation burials', discovered before and during the 1974 excavations at Ewijk (site 232).[53] They were generally located at some distance from the settlement. The undatable burials could well be connected to the central settlement, which dates to the Middle- and Late-Iron Age.

Conclusions

It is only occasionally possible to indicate with certainty a late-Iron Age burial. Their near absence can, however, be explained by their archaeological invisibility, probably caused by changes in burial practices which had already started in the Middle-Iron Age. These were reversed again at or after the end of the Iron Age, and are in some way related to the Roman occupation.

Late-Iron Age burials are unique in that they are *all* so difficult to detect. A few may have survived under the rather unfertile (drift?-) sands near Wijchen and Alverna

45 See Lanting/Mook 1977, 183–5.

46 GrN 10731: 2085 ± 30 BP. See Verwers 1983, 29.

47 See Modderman 1951, 30–45 (esp. 44 and fig. 6, 10); also, Remouchamps 1928 and chapter 5, site 324.

48 Peddemors 1978, catalogue nos. 216, 234, 235, 237, 239–40, and 281.

49 Peddemors does not indicate any middle-Iron Age pottery from them.

50 See Bloemers, in press.

51 Remouchamps 1924.

52 See also chapter 6, 161–2.

53 *JROB* 1974, 9.

and others, which are deep enough under the present-day surface, have only been found as the accidental by-product of large-scale settlement excavations such as those in Ewijk and Oss-Ussen. Even then it is not a simple affair to date such simple cremation burials.
That they are all so plain and difficult to find implies that they must have been quite similar in their material aspects. Little expenditure of energy is required to construct burial monuments and normally no grave-goods are present. This situation is different from the earlier urnfields which show more variation and occasionally exceptional burials such as those of Wijchen and Nijmegen, and also from the Roman Period cemeteries. It would be a bit too far-fetched to assume that for a period of roughly two centuries there was, therefore, no social and or economic inequality among the inhabitants of the river area. After all, the recognition of the status of any individual at his death may also be expressed in other ways. But the absence of any material expression of status strongly suggests that social differences in the population must have been rather limited. They should probably be comparable to those of the earlier Iron Age or, instead of being a further development in socio-economic stratification, represent even a simpler form of social organization.

8.3 THE EARLY-ROMAN PERIOD

The Early-Roman Period is rather short; it was defined in chapter 2 (p. 24-5) as the period between 12 BC and c. AD 50. Nevertheless, there is evidence for the occupation of at least 142 settlement sites. As mentioned in chapter 4, this evidence may be composed of both positive and negative arguments. Positive is the presence of early-Roman imported material, in most cases pottery. Although it is possible that in the course of six decades at least some of this material landed in every existing settlement, it is highly unlikely that it can be discovered on every one of them. The evidence presented in chapter 6 clearly indicates that large amounts of Roman pottery only became available from the end of the first century onwards. Early-Roman pottery will therefore only be found on sites which actually had quite a lot of it and, in some additional cases, by sheer luck. A simple calculation even shows that it is highly unlikely that any material is found at all.
If a site is assumed to have been occupied from the Late-Iron Age to the end of the Middle-Roman Period (which is frequently true) and that the amount of pottery used at any one time remains constant (which may not be accurate, but in the present context an acceptable proposition[54]), then the percentage of pottery from the Early-Roman Period will be c. 1/9th (11%) of the total quantity. If, further, a proportional rate of recovery is assumed for material from all three periods (in general not unlikely because most sites are hardly, if at all, stratified), then the percentage of early-Roman pottery in the sample will also be 11%. If the imported (Gallo-) Roman wares constituted 10% of the total amount of pottery used during the Early-Roman Period (which is probably a very optimistic estimate), the fraction of these wares will be only 1.1% of the sample. Finally, it is also necessary that the sherds are recognizable as early-Roman. This means that only rims (and not even all of them) and some other sherds are suitable, which is certainly not more than 25% of the total. An optimistic estimate of the fraction of identifiable early-Roman imported wares in a sample would therefore amount to only .275%, which is 1 out of 364 sherds!
These figures imply two things. First, that those sites which actually have a reasonable percentage of early-Roman sherds are probably special in some way; and second, that the absence of such sherds means nothing as far as the dating of unexcavated sites is concerned (except, of course, when a very large and representative collection of material is available). For this reason, definite occupation of sites during the Late-Iron Age and also during the Middle-Roman Period has been taken as (negative) evidence for occupation during the short Early-Roman Period.
Apart from the 142 settlements for which occupation is acceptable, there are no less than 118 possible or probable early-Roman settlements. They are generally sites where only relatively small amounts of material (< 100 sherds and often only a few) have been collected. Most

54 The amount of pottery used is, of course, determined by such factors as the number of people living at a site and their social and economic behaviour, as well as, for example, the durability of different sorts of pottery. Native ware may break more easily than Roman pottery and thus cause a larger amount of pots discarded during the Late-Iron Age and Early-Roman Period. On the other hand, population numbers and variability of pottery presumably increased during the Middle-Roman Period on most sites.

of these are late-Iron Age sites which could still, or late 1st- or 2nd-century sites which could already be inhabited during the Early-Roman Period. The maximum number of early-Roman settlements is therefore 260, but an estimate of the original number is very difficult to attain by this figure. It is close to the maximum of c. 300 calculated for the late-Iron Age, but an evaluation of early-Roman population-size shows that the true number is more likely to be in the order of 400 settlements.[55]

It is altogether possible that the actual number of settlements is roughly the same as during the late-Iron Age. The general tendency towards continuity of occupation visible in fig. 23 (chapter 4) would be in accordance with such a thought, but it does not constitute proof. It is very unfortunate that the presence or absence of conceivable negative effects of the arrival of Roman armies on the number of settlements cannot be evaluated more precisely for lack of reliable archaeological indicators. There are, however, a few arguments which make a considerable negative impact unlikely. These are the continuity in native pottery traditions, the large number of essentially native middle-Roman settlements[56] which cannot have come out of thin air, the positive effects of the occupation which led to new settlements (at the very least the military camps and related civil occupation), and also not only the absence of large-scale hostilities but, on the contrary, the presence of unusually friendly relations between Romans and natives which have been deduced from the classical sources.[57] All in all, the total number of early-Roman settlements may be estimated as at least equal to that of the Late-Iron Age, due to Batavian immigrants presumably larger.

As far as the typology of settlements is concerned, it is evident from Appendix 2 and fig. 61 that there is obvious and clearly identifiable differentiation. The cause of this differentiation is, of course, the arrival of Roman armies. The known camps have usually been to some extent excavated, but an insight into the properties of excavated camps may lead to the identification of possible other military establishments and also to conclusions about civil settlements. Any differentiation of the latter will necessarily have to be based on measurements reflecting primarily differential interaction with the new superstructure.

8.3.1 Military Settlements

As far as military camps are concerned, a consideration of the Nijmegen sites 403, 406, 412, and 417 (see fig. 66) is of fundamental importance. A concise discussion of all the available evidence on these sites has been published recently.[58] Unfortunately, only sites 406 and 412 are known well enough to permit a definite interpretation as military camps. Site 417 has also been defined as an, at least initially, military settlement, while site 403 is considered to be predominantly civilian. It is important to see which criteria are, or could be, employed to reach those conclusions. These criteria relate to four aspects of the sites: their location, their structure, the finds, and the available historical information.

All four sites are located on Pleistocene deposits. Sites 412 and 417 on the highest points of the ice-pushed ridge, 403 and 406 somewhat lower on fluvioglacial deposits. All sites are located on the brink of the ridge, which is very steep in the east (417, 412) and gradually declines to the west. The altitude over the (current) mean water-level of the Waal is 50 m for site 417, 35 m for 412, 25 m for 406, and 20 m for site 403. As shown by Appendix 2 and in part also visible on fig. 66, all sites are bordered to the south by dry valleys.

They are evidently located at high, easily defendable positions. This observation, combined with the fact that, notwithstanding very extensive excavations in the entire area, virtually to traces of pre-Roman occupation have been found,[59] indicates that the location of all four sites, and especially 417 and 412, is determined by military strategic demands. On a clear day, it is possible to look out from these two sites over the entire Overbetuwe up to the Rhine and the ice-pushed ridges at Arnhem.

Sites 406 and 412 are clearly defined as military camps by their peripheral structures. Site 406 has a V-shaped ditch and a wall, site 412 has two V-shaped ditches, large wooden gates, and towers. Inside these structures, no definitely contemporaneous traces of buildings have been discovered, so it is assumed that both fortifications were largely tent-camps.[60] Sites 403 and 417 are not as

55 See below, p. 396–7.

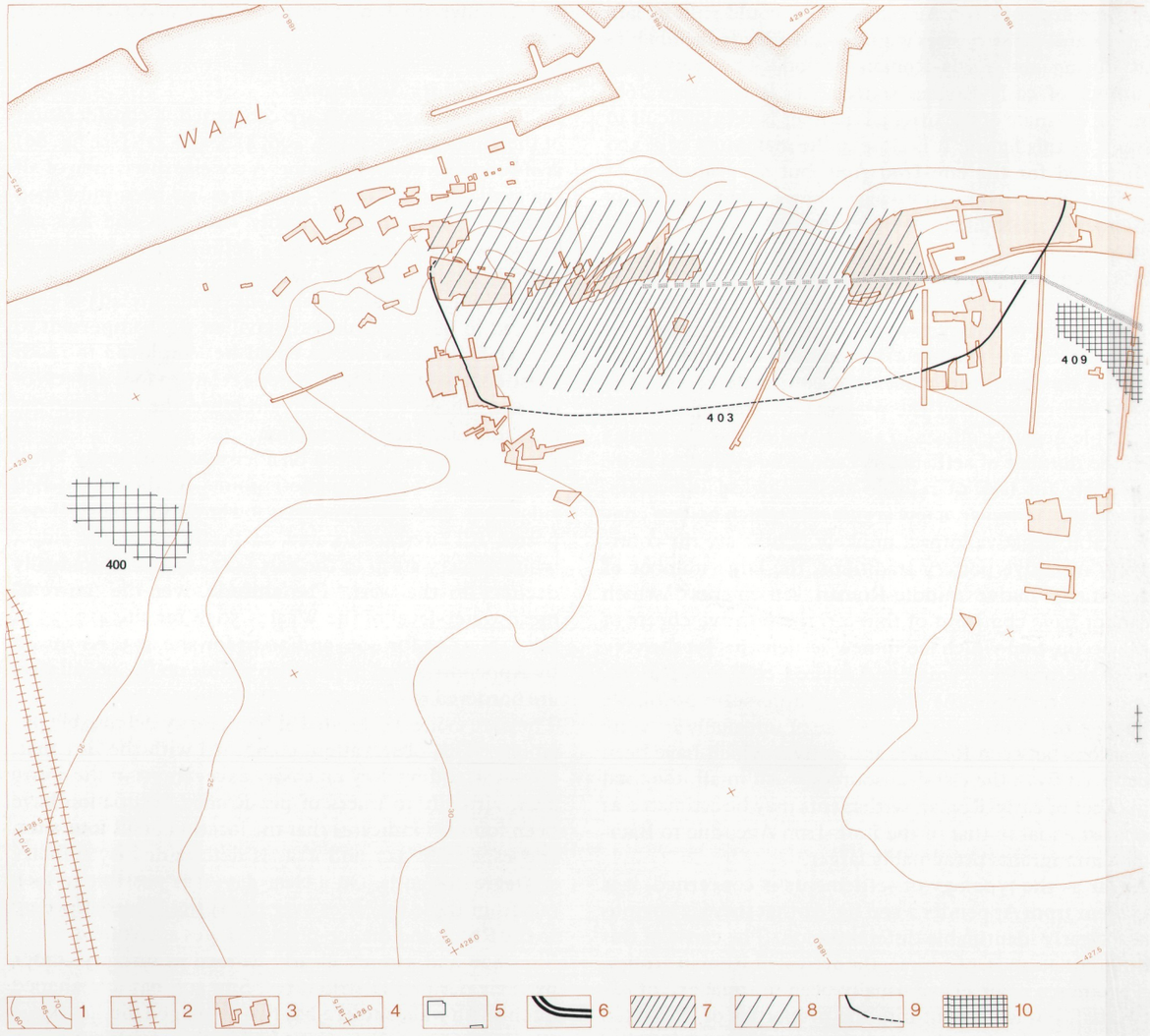
56 See below, p. 246.

57 See Bogaers 1960–61, esp. 263–72, for a summary of the arguments. See also paragraphs 10.3–4 and 11.3.

58 Bloemers/Bogaers a.o. 1979.

59 This observation is confirmed by a pollen diagram (see Teunissen/Teunissen-van Oorschot 1980) indicating a nearly undisturbed oak forest on the hills. For an additional interpretation, see also below, ch. 11.3.2.

60 For site 412 evidence for timber buildings is, however, not lacking: see Bogaers/Haalebos 1977, 97–9 and 1980b, 56–9.



extensively excavated, but both may have been surrounded by ditches: a V-shaped ditch around site 417 and an asymmetrical V-shaped ditch (a so-called *fossa Punica*) around site 403.[61] The restricted excavations since 1980 have produced plans of wooden buildings on

61 417: Bogaers/Haalebos 1979a, 19, *Abb.* 6, 3; 403: Bogaers 1979a, 30, *Abb.* 23, 5. The ditch is probably pre-Flavian.

Fig. 66 Nijmegen: sites from AD 30–70. For site nos., see chapter 5 (catalogue): 1 contour lines, 2 railway, 3 excavated area, 4 topographical coordinates, 5 outlines of buildings (not on this map), 6 double ditch around the Augustan legionary camp, an area unoccupied in this period, 7 inhabited area; investigated and/or many finds, 8 inhabited area; not investigated and/or few finds, 9 ditch, certain and hypothetical trajectory, 10 cemetery; investigated and/or many finds, 11 cemetery; not investigated and/or few finds, 12 road. Scale 1:10,000.



site 403 and there is enough evidence to assume that these are present on site 417 (cf. note 60).
As far as the finds are concerned, no comparative quantitative analysis is possible because of lack of sufficient published data. In general, it seems that a fair amount of native pottery is present, but the quantities are rather insignificant in relation to (Gallo-)Roman wares.[62] It is also worth noting that the early-Roman wheel-turned pottery from the four Nijmegen sites together probably accounts for 99% or more of all such pottery found in the eastern river area so far. The only well-published excavation to provide interpretable data is the investiga-

62 See, e.g., Daniëls 1955, 62–101 (403), Bogaers/Haalebos 1976, 180; 1977, 124; 1980b, 77–81 (412) and Bogaers/Haalebos 1975, 154 (417).

tion of a chronologically significant sequence of layers from site 417. The data included in the excavation report permit a calculation of the percentage of native pottery, which is c. 5%.[63] The conclusion of the excavators that native pottery increases strongly from the oldest to the later layers is definitely not correct. Layers 1-2 do indeed contain little native pottery, namely 4% (99:4), but the same is true in later periods: layers 3-5 have 6% (230:15) and layers 6-7 have again 4% (442:20).

In qualitative terms there are two properties which can be considered, apart from 'preponderance of Roman pottery'. All settlements have yielded specifically military metal objects. Intelligible graffiti have also been found on all of them, and an early inscription was found in secondary position on site 403 (fig. 67).

From a review of the available material evidence, it is clear that the differences between the four sites are primarily related to differences in the level of investigation. There are no compelling reasons why sites 403 and 417 should not be interpreted in exactly the same way as the camps 406 and 412, namely as military settlements. There is, however, historical information about an oppidum Batavorum and about a place called Batavodurum. As Bogaers has convincingly demonstrated, Batavodurum should certainly be localized in Nijmegen.[64] It is also likely that both names refer to the same place, a town (fortified capital, stronghold) of the Batavians. This should imply at least a large civil component for such a town. If 'civil' is in some way related to 'native', neither sites 403 nor 417 qualify as a civilian settlement on the available evidence. Site 417 has been shown to have started as a military camp on the basis of the finds and there is no convincing evidence that it did not remain a camp until the end of its habitation around AD 70. Its location is also hardly suitable for permanent civilian habitation.

Therefore, site 403 is the only plausible location for a civil Batavodurum. Although the identification rests mainly on historical arguments, the relatively low location of site 403 and its proximity to the river make it acceptable. The traces of buildings and indications for industrial activities discovered during recent excavations,[65] as well as the absence of really significant quantities of native artefacts, imply, however, that the Batavian capital was as 'Roman' as a settlement or even a camp could be. The material evidence indicates that, if the identification is correct, the Batavian capital was surely not 'native' and not even definitely 'civilian'. In fact, on the present evidence it could more accurately be described as a large military *vicus*.

Such an interpretation does not conflict at all with a status as Batavian capital, especially where no such centre had existed in Nijmegen before the Roman Period. The combination is also the only way to explain why the capital should look like a military site or – at least partially – be one.[66] It is evident that in the Early-Roman Period any site with V-shaped ditches and virtually nothing but Roman pottery and other indicators such as intelligible graffiti and military metalware is itself, or is directly related to, a military camp.[67] The archaeological data thus suggest that Batavodurum/oppidum Batavorum, instead of (fortified) capital or stronghold *of* the *Batavians*, is more properly understood as either '*in the land of* the Batavians', or as '*of* the *civitas Batavorum*'.

In the area outside Nijmegen, there are only two other sites which can be positively identified by one or more of these criteria. The first is site 126 in Meinerswijk, where the lowest levels of a small trial trench contained early-Roman pottery with a few native sherds. They also contained at least one military metal object and several sherds with parts of graffiti, one of them possibly indicating a legion (fig. 98).

The second is site 499 in Cuijk, where traces of V-shaped and other ditches and foundation trenches of

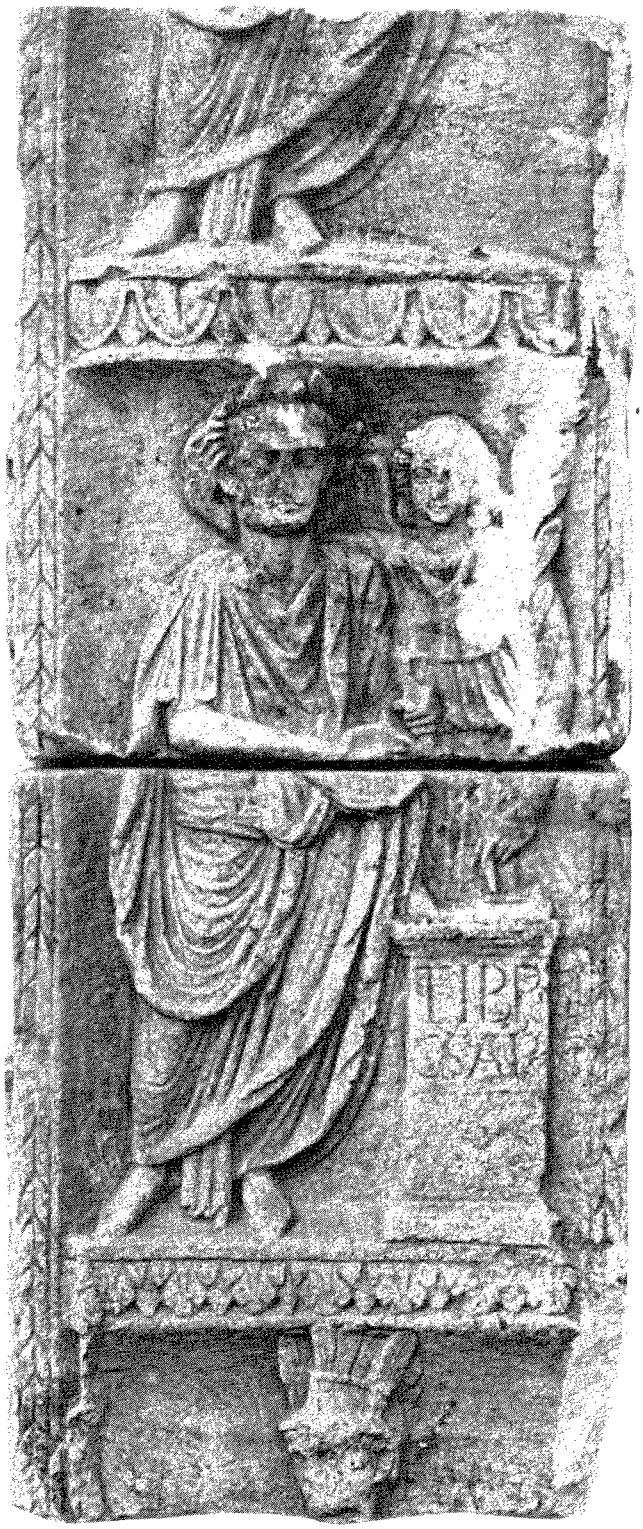
63 Bogaers/Haalebos 1975, 136, 148, 152–4. Calculation based on rim-sherds. The number of terra sigillata rims is not indicated completely, but estimated here at 1/3 of the determinable fragments. The (scarce) Belgic ware is not included.

64 Bogaers 1960–61, 274.

65 See *JROB* 1981, 32–3 and 1982, 34–5. Older discoveries are mentioned by Daniëls 1921 and 1955, 47–101 and Holwerda 1946.

66 Recent excavations around the Kelfkensbos in Nijmegen have revealed the presence of several presumably early ditches at site 403, which makes it even more easy to accept at the very least an initial and/or partial military occupation. Van Es (1981, 132) considers this also likely, on account of the distribution of early coins. On the other hand, there are early military defence works known from other towns (e.g., Frere 1978, 283 f.), so the possibility of a *fossa Punica* around site 403 does not necessarily point to a purely military occupation.

67 For the Middle-Roman Period, such criteria are difficult to employ when large civil settlements are concerned, because attributes of towns often look very much like military characteristics. See, e.g., Bogaers 1972a, 318 f. on the interpretation of Forum Hadriani and Webster 1966 (esp. 41–2) on the origins of some of the British towns.



◁ Fig. 67 Part of a limestone column from Nijmegen, site 403. The two blocks measure 90 × 80 × 80 cm and were part of a column at least 6 m high. The altar with a Victoria above it is dedicated to Tib(e)r(ius) C(ae)sar and the monument may have been erected in AD 17 on the occasion of Germanicus' victory, or somewhat later.

buildings (barracks?) were discovered. The finds of the excavation have never been published in detail.

Apart from the six settlements discussed so far, there are another three sites where military camps could have been located: 117/8, 183, and 194. Of these sites, only 117/8 has yielded material which allows such a tentative identification. It is located in Driel, where a very large (c. 8.5 ha) ancient settlement soil occupies three parcels of land: the Baarskamp and Lizesland, which have been interpreted as one site (117), and the Marskamp (118), which is somewhat further to the northwest but probably forms part of the same settlement.

Site 117/8 is somewhat exceptional, because it must have been a settlement for several centuries before the Roman Period. This is, however, in no way an impediment to identification as a military camp. Site 117/8 is located on the channel deposits of a pre-Roman branch of the Rhine. This branch was still functioning during the Roman Period but as discussed in chapters 3 and 7.2 it was no longer the main channel. Driel is situated at the point where it joined the new main channel, which was formed during the Iron Age. Site 117/8 is thus located at a high point with waterways in three directions: a rather favourable situation if it was a military camp which functioned in the context of an area (the *Insula Batavorum*) used as an assembly area for troops and a base for offensive campaigns across the water.

Apart from the locational and rather general historical arguments, all conclusions on the nature of the early-Roman settlement depend on the finds because no excavation has yet taken place. Since the last war, a rather large amount of material has been collected at the surface of sites 117 and 118. Although one occasional observation has shown that at least part of site 117 is rather deeply stratified (c. 1.5 m), the collection of sherds contained about a dozen identifiable fragments of early-Roman pottery. For this reason the fort, if there is one, is most likely to be located at site 117 while site 118 represents the surrounding occupation, but this is nothing more than an educated guess. Together with one (countermarked) Augustan coin, these dozen sherds are all de-

finitely early-Roman material. But in view of the exceedingly small chance of finding such material at the surface and the near absence of it elsewhere in the region, these finds can be regarded as the 'tip of the iceberg' and an indication of early-Roman military occupation.

In addition to these arguments, the probability that site 117/8 was (converted into) a frontier-fort in the Middle-Roman Period may also be taken as support for this interpretation. As is evident from excavations in many forts,[68] there is a strong continuity in the sites that were occupied by the army. Camps from the earlier period which were located in such a way that they could be incorporated as forts in the *limes*, were indeed used as such, for example, Vechten, Meinerswijk, Altkalkar, and Xanten).

Because of this last consideration, site 194 may also be regarded as an early-Roman camp. The finds (brought up by dredging) include clear indications for a middle-Roman fort, as well as several identifiable early-Roman sherds. The context of the discovery does, however, not permit more definite conclusions. It is quite conceivable that the dredging should have yielded a higher percentage of early pottery if site 194 actually was a military camp during one or more of the offensive campaigns under Augustus or Tiberius. In addition, the available sherds do not necessarily point to such an early date. Arretine terra sigillata is lacking, and mortaria Haltern 59, cork-urns Haltern 91A, and even flagons Haltern 48 still occur a few decades later. If, however, site 194 is interpreted as a middle-Roman fort, these finds indicate that it must have been established at a relatively early date either at the end of Tiberius' reign, under Caius, or at the beginning of the Claudian period.

The last site where an early-Roman camp must have been located is no. 183. This is based almost entirely on historical evidence and as a site (a physical phenomenon) it is hypothetical. Its exact location is unknown and its position on Appendix 2 is, though not completely arbitrary, little more than an indication. The arguments for its existence and location are outlined in paragraphs 3.3.3 and 11.3.2. Its presence somewhat to the south of site 182 is assumed in relation to Drusus' dam at the fork of Rhine and Waal. The only archaeological find is a tombstone from the associated cemetery (fig. 17), dated to before AD 50, on which the dam (*moles*) is mentioned.

Unfortunately, confirmation of the existence of site 18 will be nearly impossible to obtain. The meander-belt extremely wide around Lobith, and the constantly shif ing channels have probably eroded all older deposits. addition, the camp would not have had stone building perhaps not even tile-roofs, the remains of which wou have facilitated discovery.

8.3.2 Civil Settlements

In general, the information about the structure of earl Roman settlements in the river area is just as limited about those of the Late-Iron Age. There are few excav tions in addition to the list on p. 224 of sites which we occupied from the Late-Iron Age onwards, namel Ede-Manen (3), Kesteren-Hoge Woerd (43), Heteren Het Lage Land (93), Heteren-Uilenburg (95), El (105), and Overasselt-De Bullenkamp (356). But exce for site 105 they have produced no additional inform tion compared to what has been said about late-Iron A settlements. The investigations at sites 3, 43, and 3 were extremely limited, and sites 93 and 95 are of t post-hole swarm type already discussed.

Although the information from excavations is limited, seems that the same types of settlements which exist during the Iron Age still constituted about all there w in early-Roman times: there are settlements with one more houses of clearly identifiable types and there a the post-hole swarms representing a different form house-construction. If there were any changes in the i ternal spatial organization of settlements, these have n (yet) been detected. The recognizable 'standard' hous plans from the Roman period are a further developme from those of the Iron Age, as is demonstrated, f example, by the two- or partly two-, partly three-aisl houses from Oss, Druten, Ewijk, and Wijk bij Duurst de.[69] These are characteristic house-types of the easte river area and the Pleistocene soils of Brabant,[70] a seem to have evolved from the Iron Age houses of t 'Haps-type'. Traces of 'Roman' or 'vicus type' on aisled houses have not been found in early-Roman nati settlements. The only examples known so far were di covered on site 403 in 1981.

Apart from the gradually changing house-plans, a pr cess which was probably going on during the Early-R man Period, the settlements seem to have stayed more

68 See Bogaers/Rüger 1974 and Gechter 1979 for an overview.

69 Oss: see the preliminary notes in G.J. Verwers 1978, 1981, and 1983; Druten: Hulst 1978; Ewijk: *JROB* 1974, 9–10; W bij Duurstede: Van Es 1981, 177; 1982, 145–6.

70 See also Van Es 1982, 144–7 for an overview.

less the same. There is, at the moment, no real evidence for the introduction of new settlement-types or types of buildings, with the exception of site 403 which is not a native settlement. This does not imply that such a development did not occur before c. AD 50, for example in a settlement like Elst (105). The first temple there was built around AD 50, but a settlement (probably also a religious centre!) existed before that time, as is demonstrated by a layer with (secondarily deposited?) settlement debris underneath the building level of temple I.[71] Site 105 is located at a central spot in the Betuwe, and the fact that the temple was built there is hardly coincidental. An early-Roman local centre of some importance was probably situated in the immediate vicinity of the spot where the temple was built, in an area which is at present almost completely built over. This does, of course, not mean that this important settlement was a native affair. It may have been because, after all, the site was already inhabited in the Late-Iron Age, but other excavations, for example at Grobbendonk (Belgium), have shown that the construction of temples may be directly related to a previous function of the settlement in a Roman context.[72]

Another new type of settlement which must have appeared in the Early-Roman Period is the military *vicus*: the settlement in the immediate vicinity of a fort inhabited by civilians, in particular artisans and traders. Structurally, these *vici* should not be difficult to identify as such, because their layout and the types of buildings can be expected to differ significantly from native settlements. In fact, most of the features and finds discovered at site 403 in Nijmegen are typical for this type of settlement.

Although similar sites have not been found elsewhere in the eastern river area, they are known to have existed in adjacent regions during the Early-Roman Period, for example, in Moers-Asberg–Asciburgium and, perhaps, in Vechten-Fectio.[73] There are, however, a number of sites where military *vici* could have existed, such as part of site 126 (possibly site 23) and site 194. It is not known if the *vicus* in Cuijk (500) already existed next to the early fort (499), but archaeological investigations outside the area of the fort have been very limited. For site 117/8, if it was indeed a fort, the presence of an early *vicus* (118?) will be difficult to establish without extensive excavations because of the long pre-Roman occupation of the site.

Chronology, location, and finds

Apart from the observed, expected, or possible structural differences between early-Roman settlements, which are rather meagre, the chronology and location and also the finds are other important clues for a possible differentiation. On the Holocene deposits, there is no significant difference between late-Iron Age and early-Roman settlements, with the exception of the Rhine-branch between a point south of Groessen (c. 198/436) to a point west of Meinerswijk (c. 187/442), which only became properly settled from the beginning of the Roman Period onwards, and thus presumably under the impetus of military demands. As discussed in chapter 3, this branch was just barely or not at all habitable during the Iron Age. Therefore, the few new and non-military settlements there could be different from the others, either as 'pioneer' settlements or because they started as a spontaneous or induced response to the establishment of military camps.

It is, however, questionable whether these settlements were any different from other native sites except, perhaps, as far as their legal status was concerned. There is only one settlement (23) which may have existed in the Early-Roman Period and which probably started as a direct result of the Roman military presence.

Some of the finds are definitely pre-Flavian, but not necessarily datable to IA. They consist largely of wheel-turned pottery and the settlement is located very close to the military establishment at site 126. Although the two sites are located on opposite sides of the present-day Rhine (see fig. 61 and especially fig. 93), both were probably situated south of the Roman Rhine channel. If site 23 did exist in the Early-Roman Period, it could be the *vicus* of the early-Roman fort of Meinerswijk, or perhaps a wharf such as Valkenburg – De Woerd in its early stages.[74]

It is conceivable that spontaneous settlement in this area was not encouraged or even allowed by the military authorities. After all, the banks of the Rhine may have been military territory. In this respect site 132, on which only

71 Bogaers 1955, 42–3 and 174–5. Recent analysis of the bone material by R. Lauwerier has confirmed Bogaers' preliminary opinion on this subject. See *JROB* 1983, 33.

72 See De Boe 1977. Location indicated on fig. 80.

73 Fectio: Bogaers/Rüger 1974, 62–5, with further references; Van Tent 1976, 61–4; Kalee 1980; Asciburgium: Bechert 1974, 183–9, esp. 188.

74 Bloemers/Sarfatij 1976. See also below, p. 269.

native-Roman hand-made pottery was found, is interesting. It could well be that a native settlement which developed there 'in an unguarded moment', was not allowed to continue. The absence of early wheel-turned pottery is, however, perfectly understandable as long as a collection of sherds from a site is not very large indeed. Although the collection of site 132 is large enough to conclude that the ceramics are native-Roman and not late-Iron Age pottery, the material was recovered from a very restricted area and it is not possible to conclude that wheel-turned pottery is definitely absent.

As far as Pleistocene deposits are concerned, it is evident that the military establishments on the ice-pushed ridge in Nijmegen represent a completely new choice of location. The area is not very suitable for normal settlements, which are indeed lacking. The occupation of site 403 (and 417) can hardly be called normal, or indeed entirely (if at all) civilian. With regard to the coversands and loess, the real or seemingly near absence of occupation applies to all periods.

The choice of location thus offers only a few clues for the differentiation between settlements, and the same is true for the chronology. In the case of the Nijmegen sites and also for Elst, the chronological data are related to different and new types of settlements. But in general such a connection to settlement type is impossible to establish. If there were at all Iron Age settlement types which did not continue throughout the Early-Roman Period, which is hardly likely because the period is too short,[75] the available material evidence can only be used for differentation in one direction, namely, to identify new settlement types which are not native.

As already mentioned earlier, the available non-native finds from settlements in the river area are very scarce, which agrees with the theoretical expectations (cf. p. 232). The only clear exception, site 117/8, has been interpreted as a probable early military camp. When the other sites are considered, a chronological differentiation has to be made first, because not all material is equally precisely datable. As can be deduced from chapters 4 and 5, there are 7 settlements on which definitely Augustan material was found. These are 3, 142, 234, 244, 300, 310, and 515. The recently discovered site 544 (see chapter 7.1) should also be included in this list. In addition, there are another 13 settlements with material which could, in most cases, be Augustan, but which is at least datable to IA, namely nos. 43, 48, 93, 95, 96, 100, 105, 123, 150, 151, 299, 356, and 362. Finally, there are 15 settlements with certainly pre-Flavian finds which could be datable to IA. These are sites 23, 62, 75, 135, 140, 214, 232, 239, 250, 298, 315, 341, 459, 487, and 500.

The nature of the finds in question is not really relevant, because there are almost always only one, two, or three artefacts involved. These are always potsherds, in a few cases supported by a fibula, or one or a few coins. The presence of these rare items, when considered for each site separately, can be easily attributed to chance. It is quite well possible that some early-Roman pottery landed in every settlement which existed at that time and an identifiable fragment may be accidentally discovered on every one of them. Whether or not such a fragment has any special significance can only be evaluated at the regional level where the distribution pattern and increase of early finds through time can be observed.

The latter phenomenon is easily demonstrated by adding up the figures given above, which results in a total figure of 8 settlements for the Augustan period, 21 for IA and 36 for the pre-Flavian era. Although it is logical that the number of sites with material from a period should increase when the period in question is longer, the growth curve indicated by the figures 8-21-36 is, in a relative sense, still valid. This is because only for site 515 and possibly site 3 do the early-Roman sherds presumably mark the end of the habitation. For all the others they are either fragments of the first imported items in a pre-existing settlement or they date the beginning of a settlement, and in both cases the sites in question existed for a long time afterwards.

Therefore, the curve can be taken to reflect the phenomenon of the increasing availability of (Gallo-) Roman material to people living in the region. But this is still a general and intuitively even obvious conclusion. When the total quantity of (Gallo-) Roman artefacts in the area is compared to that of Nijmegen, it is completely insignificant. It is not possible to estimate the real distribution of imports in the area from the curve, but it is evident that the observed increase in the pre-Flavian period is from extremely to very limited only. There is thus a fundamental difference between settlements with virtually only imported material (126, 194, 403, 406, 412, 417, and 499) and with virtually none, the surface collection of site 117/118 being the sole intermediate case.

It is justified to conclude that Roman material trickled

75 The only real possibility could have been central places, whose existence in the eastern river area could not be demonstrated in the first place.

through only very sparingly from the military establishments into the region. The important question in the present context is to establish whether or not any conclusions can be drawn for the sites in the region: is there any chance of differentiating between the sites which did, and those which did not get such material.

In order to assemble a complete picture, all settlements and cemeteries with early imports are presented on a separate distribution map (fig. 68). It is quite clear from the distribution that in general the sites cluster in the middle of the map: around the axis Cuijk-Nijmegen-Meinerswijk. But when the structure of the landscape is taken into consideration, some differences emerge. The eastern part of the Overbetuwe is the immediate hinterland of one or more early camps on the Rhine, and precisely between those camps and the Nijmegen sites, with Elst exactly in the centre. The presence of some early imports in this area is, therefore, not likely to imply that the settlements involved are a special type of site. The same is true for the sites along the Rhine in general. The early military activities took place along two axes: one land route (Cuijk-Nijmegen-Driel/Meinerswijk) and one water route, along the Rhine. From the Overbetuwe, there were at least two routes going further north: one over water along the IJssel and one over land along the line Wageningen-Ede.

Although the Waal and the Meuse, and possible routes over land (see fig. 22 or 75), should not be neglected in this respect, these are the two routes which seem to be the most relevant for military operations and along which the archaeological visibility of some early refuse is not surprising. The same is not true, however, for the eastern part of the Land van Maas en Waal. The concentration of early material on the Pleistocene soils around Wijchen and the older Holocene deposits south of Ewijk is not so easily explained. It is west of the land route from Cuijk to Nijmegen and, although it is attached to the Waal and the Meuse, the concentration is between rather than at these rivers. As far as military operations are concerned, this area has no direct importance. The clear cluster of early and even very early imports is therefore more likely to arise from some intentional and directional process, rather than from an accidental and arbitrary one, even though the proximity to Nijmegen is undoubtedly very important, especially for the sites around Ewijk.

There are at least two processes which could be involved here: either a military one, involving the presence of military personnel in order to control the area, or a civil (or not directly military) one, involving reasons why the inhabitants of some of the settlements could obtain goods from the army. Whatever the real reason may be, if the interpretation of the distribution is correct it implies that certain settlements in the eastern part of the Land van Maas en Waal were of special importance. This observation is in accordance with the conclusions reached for the Late-Iron Age. Although no clear differentiation of settlements for that period was possible, it was concluded (at the regional level) that sites in the area around Wijchen were relatively more wealthy than elsewhere. Their importance may even have grown in the last century BC.

As mentioned at the outset, individual sites cannot be declared special just because an early-Roman sherd was found there. From the foregoing discussion, it appears that for most sites their location goes a long way to explain why such sherds were found on them. For the settlements around Wijchen such an explanation may not be enough, and they could indeed be something special. The apparent continuity of 'relative importance' in the same area supports such a conclusion (if soldiers were actually stationed here, it could be for this reason!), and it also enables the more precise location of the sites involved.

A cross-check with the 'richer' sites from the Late-Iron Age, shows that sites 239, 244, the closely related group 296-302, and possibly site 356 could qualify as exceptional settlements in both periods. The only site in the Betuwe which shows the same combination is no. 43. With the exception of sites 296-302 (especially 302 which, however, yielded no special items), none of these sites has been excavated. The investigations at site 43 were no more than very limited trial (1960) or rescue (1961) operations. All in all, it seems that the early-Roman native settlements can be divided hierarchically into two groups on the basis of their location and finds.

Conclusions

In contrast to the preceding period, the early-Roman settlements in the area show a clear diversification. The evidence is, in fact, such that instead of diversification it is more useful to say that there are two sorts of settlements in the area, which are virtually unrelated from several points of view, such as structure, location and material residue. The one sort can be properly called Roman, while the others are clearly native settlements. The gap between both is large, complete, and unbridgeable.

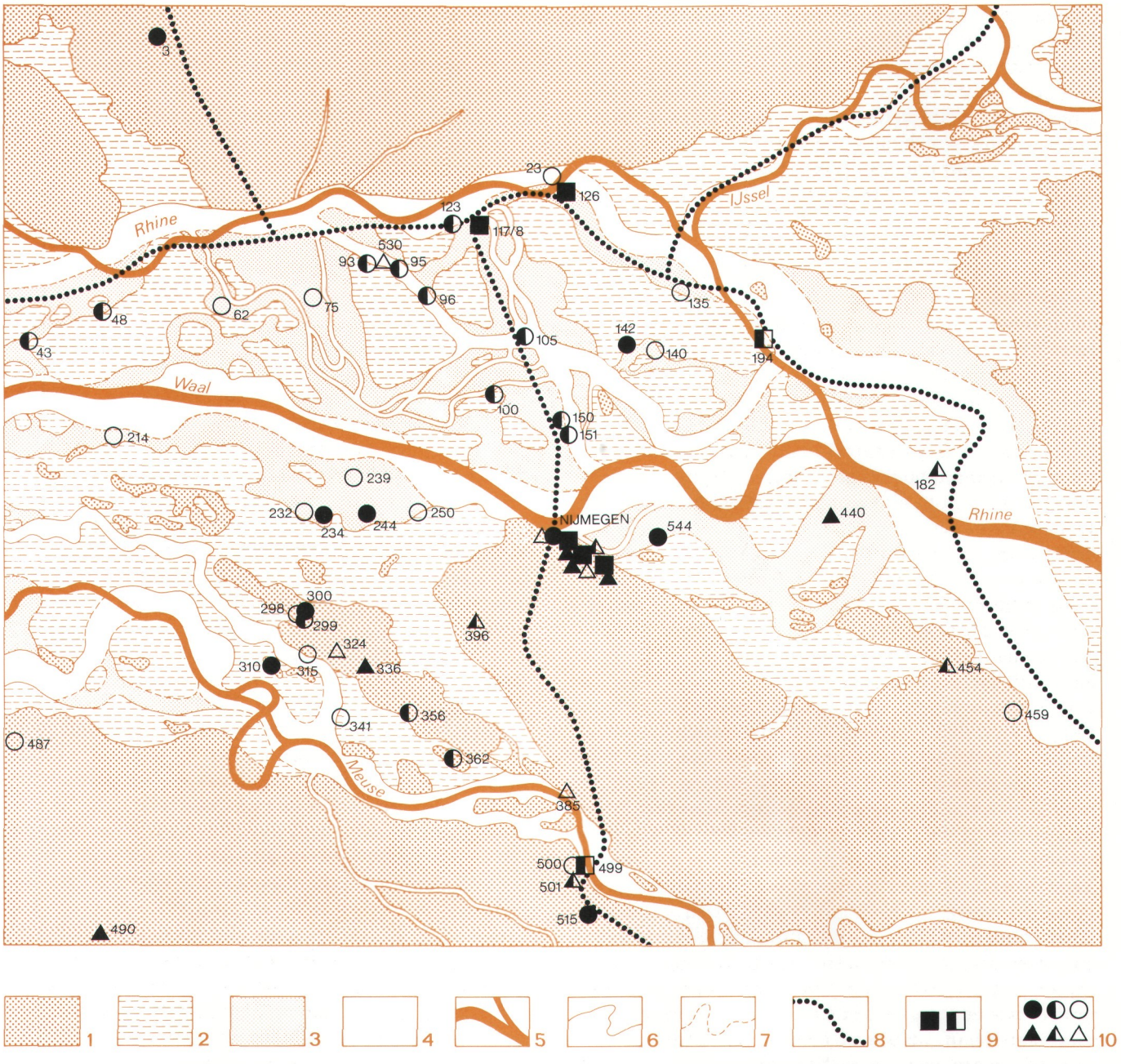


Fig. 68 Sites with early imports and primary routes (early axes of military activity) in the eastern river area. For site nos., see chapter 5 (catalogue): 1–7 see fig. 58, 8 routes, 9 military settlements, Augustan and IA, 10 native settlements (circles) and cemeteries (triangles), Augustan, IA, and pre-Flavian.

All this is, in principle, hardly surprising because we are dealing with an almost perfect difference in kind: on the one hand military camps, on the other native villages or farmsteads. The Roman settlements 126, 194, 406, 412, 417, and 499 are military establishments and site 117/118, with a long occupation history and nothing but surface finds, is likely to be one also. The nature of early-

Roman Elst (105) is unknown, but it is clear that the construction of a temple there around AD 50 was also a military affair, possibly related to the construction of the *limes* after AD 47.

What is remarkable and indeed very surprising is the fact that Batavodurum, the Batavian centre which can be identified as site 403 (or, very unlikely, as site 417), is also completely and utterly different from native settlements. Even though future research may well reveal some non-Roman components at both sites, it is impossible to call them native. The assumed existence of a Batavian capital depends entirely on information from classical sources, and it is doubtful whether anything short of a complete excavation of the entire area (no longer a possibility) or explicit inscriptions could have led to the same interpretation. The conclusion has to be that the Batavian capital was a Roman settlement and an implantation in the area. It did not exist earlier, nor did it emerge from a native central place. It grew out of a military establishment or at least in direct connection with such a site.

Apart from the capital and Elst, the *vici* around early forts represent a third form of settlement which is essentially military but in which the local population may have taken part. The existence of early-Roman *vici* in the area has not been demonstrated, but proof is impossible without excavations. If they did exist, there are at least three sites which could qualify, namely, 23 (next to 126), 118 (next to 117), and 500 (next to 499).

Under this entirely new, entirely Roman and to all intents and purposes military superstructure, the native settlement structure apparently continued relatively unchanged. The inhabitants of a few settlements seem to have had more access to rare, important or precious objects. Especially because of apparent continuity of these 'richer' settlements from the Late-Iron Age into the Early-Roman Period and their location in the region, this observation does not seem to be fortuitous. Sites like 43, 239, 244, 296-302 and 356 (?) can therefore be regarded as examples of settlements at the top of a very short settlement 'hierarchy', which are not likely to be very different from those at the bottom.

As the excavations in Heteren (sites 93 and 95) have shown, the post-hole swarm type of settlement also continued into the Roman Period. The proposed difference in subsistence strategy between such settlements and those with more easily recognizable house-plans thus also continued to exist during the Early-Roman Period.

To sum up, it seems that the profound changes which the arrival of Roman armies must have brought about in the river area are, as far as settlement typology is concerned, only visible in the creation of new, 'Roman' settlement types, not in the disappearance or change of existing ones.

8.3.3 Cemeteries

In contrast to the Late-Iron Age, cemeteries from the Early-Roman Period are not absent. As is evident from Appendix 2, the total number is still restricted (a maximum of 17 sites), but that is not surprising in view of the short duration of the period and the fact that cemeteries are not as easy to discover as settlements.

What is surprising, however, is that some of them are clearly not Roman, which means that at least some of the burials of the native population regained the archaeological visibility which was lacking for the preceding centuries. The difference between native and Roman – which in view of the conclusions of the preceding paragraph would seem to be almost the same as the difference between civil and military in the Early-Roman Period – can only be properly evaluated after excavation. With regard to the Nijmegen cemeteries, only part of the large graveyard under and around the present-day Museum Kamstraat (site 409) has been systematically investigated.[76] It consists of a very large number of various cremation-burials, the common practice of disposal of the dead in the Early- and Middle-Roman Periods. In view of its location, site 409 should be seen as the burial-place belonging primarily to site 403 which, although considered to be the Batavian capital, is definitely not a native settlement. The same is true for the cemetery, which, apart from a limited number of pots, yielded nothing specifically native. As long as an encompassing analysis of all Roman Period burial practices is lacking, it is not possible to make definite statements about single burials, but it is justified to conclude that site 409 with its burial pits with Roman pottery and without peripheral structures is a Roman cemetery. Whether or not civilians (immigrants and/or members of the native population) formed the majority of the deceased cannot be decided, but it is certain that they were buried in a way comparable to (Gallo-)Roman cemeteries elsewhere, with Roman grave-goods (often with intelligible graffiti), and very likely on military territory.

The other Nijmegen cemeteries (400, 413, 414, 415, 418, and 419) are probably all smaller than site 409, but there

76 Bloemers 1979c.



Fig. 69 Nijmegen-Hulzen (site 396), excavation plan of the native cemetery, after Bogaers/Haalebos 1980a, fig. 1: 1 ditch, 2 cremation burial, 3 post-holes.

is no reason to assume that they are any different. They have never been properly investigated.

In contrast to the data from Nijmegen, a few excavated cemeteries in the region offer a different picture. These are sites 396 and 490, to which can be added a recently excavated cemetery in Oss-Ussen.[77] The data on si 490, which was partially excavated in 1923, are limite Its size and the period in which it was used as a cemete are not known exactly. It is certain that site 490 is a pr historic urnfield with burials from the Early-Iron Ag As mentioned in paragraph 8.2.2, it may have been us continuously throughout the Iron Age, but this cann be demonstrated with certainty. It is a fact, howeve that the latest burial contains early-Roman materi which means that either the archaeological visibility lc after the Early-Iron Age was regained or, also plausibl that an older urnfield was used again. It should be not that the burials themselves are all more or less alike: n all have pottery, but in no case have the cremations be deposited in a pit and they are all surrounded by ringditch. There is no doubt that site 490 is a complete native cemetery.

The same is true for the recently excavated site 396. T nearly completely excavated cemetery (see fig. 69) co sists of a large number of cremation burials, usually i side round or square ditches which are very close t gether. The cemetery in Hatert probably started abo AD 25, and was continuously used to the end of the 2i century. The cemetery in Oss-Ussen shows very simil structures and also started in the Early-Roman Period. A detailed analysis of these types of burials may w produce more insight into the social positions of the d ceased and the social structure of the native populatio In any case, they fall within the tradition of native bur practices. The only really new element is the (re-)intr duction of grave-goods, especially pottery in some inte ments. Most of this pottery is (Gallo-)Roman, althou native pots occur as well. In this respect, there is no d ference from Roman cemeteries such as that on site 4c Graffiti, however, seem to be largely restricted to t Nijmegen cemeteries. Another important difference b tween the two sorts of cemeteries is the amount of ea pottery. At site 409 it seems that almost everyone buri there in the Early-Roman Period received one or mo imported vessels or other items as grave-goods. At t native cemeteries, this was the case with only a few inc viduals. All other burials either contain nothing but t

77 Site 396: Bogaers/Haalebos 1980a; site 490: Remouchamps 1924; Oss-Ussen: G.J. Verwers 1981. This cemetery is located just outside the area covered by Appendix 2, a few kilometres due west of square 166/419.

78 G.J. Verwers 1981, 36 and *afb.* 25–26. The same is probably true for a cemetery near Zoelen in the central river area (pers. comm. R.S. Hulst). Another early burial ground h been discovered in Bladel (Slofstra a.o. 1982, 98–100), on t coversands in Brabant. The Veldhoven cemetery in the sa area (see overview by Verhoeven/Ector 1984, 139–40) is prob bly later.

(undatable) cremated remains or their contents are datable to the Middle-Roman Period.
In this respect, the data from the cemeteries conform well to those of the settlements: Roman pottery was not widely available and is only sparingly encountered on sites. The early settlement-finds, or rather the lack of them, indicate that it is unlikely that the absence of imported pottery in native burials is entirely a positive choice, because it was not yet widely available. On the other hand, the absence of (pottery in) Iron Age burials indicates that it was not at all unusual to omit grave-goods. Therefore, the evidence from the cemeteries cannot be interpreted as straightforwardly as that from the settlements.
It is, however, very important because the apparent scarcity of imported pottery among the native population and the fact that some burials have grave-goods at all both indicate that the individuals in question must have held a special social position. It is not possible to determine precisely what sort of position was involved: it could be one of leadership, but also that of a middleman, a soldier in the Roman auxiliaries, or some other position. There are only two things certain about it: it must have been an important position and one which was recognized at the death of the individual by the community in which he or she lived. The latter fact is especially important, because the burials involved show, compared to the preceding period, at least two new elements (the *presence* of *Roman* pottery) which are therefore likely to represent Roman influence. The presence of these elements as symbolic recognition of aspects of the *social persona* of the deceased[79] therefore implies that, whatever that dimension of the *social persona* may have been, the community expressed it in Roman terms. This means that Roman elements were incorporated in an important element of native society very soon after the start of the Roman occupation. They represent one of the earliest instances where the start of an acculturation process can be observed. In order to pursue this line of analysis further, the full publication of the cemeteries has to be awaited. It would be a reasonable assumption that the burials in question are those of people in a position of authority through whom contact with the Roman army was channelled. But as long as not even the context of the entire cemeteries, let alone the age and sex of the burials are known, such an assumption cannot be substantiated.
Apart from the burial sites considered so far, there are a number of other cemeteries indicated on Appendix 2 which have not been properly investigated and of which the interpretation is therefore mostly problematical. There can be little doubt that the tombstone from site 182 (fig. 17) represents a Roman grave or cemetery. Also the cemetery in Cuijk (site 501) is probably a military burial-ground. The other sites can, unfortunately, not be reliably classified. The cemetery at site 336 near Wijchen, which became very large in the Middle-Roman Period, may well have been a native cemetery like Hatert or Oss-Ussen. The cemeteries in Millingen (440) and Donsbrüggen (454) cannot be evaluated at all. The remaining sites in Puiflijk (208), Wijchen (324), and Mook (385) are not even certainly early-Roman, although the latter two are at least definitely pre-Flavian.

Conclusions

From the excavated sites, it appears that there is a difference between native and Roman cemeteries. Because none of the excavated sites has yet been fully published, it is only possible to observe the most evident differences between the two. These are to be found in the different structure of the burials (and, by implication, in some aspects of the burial-ritual) and in the presence of Roman pottery as a normal feature or as an exception.
It is not possible to differentiate between military and civil cemeteries, unless these concepts are equated with Roman and native. The latter could well be the actual situation in the Early-Roman Period. It is possible to differentiate between small and large cemeteries. Although early-Roman native burials are only barely detectable, the continuity of early- to middle-Roman burial sites should have drawn attention to early cemeteries which could potentially be large. The only large cemetery ever discovered is site 409, with probably hundreds of early burials, and its location next to the only really large population-centre conforms to expectations.
To sum up, at least four different sorts of graveyards can be distinguished: one large Roman cemetery, several smaller groups of Roman graves, a few native cemeteries which show Roman influences in a very restricted number of burials, and an unknown number of completely native burial-places. All that has been said about late-Iron Age disposal-of-the-dead practices applies to the last category. Future research will undoubtedly reveal additional types of cemeteries or produce refinements to this rather crude differentiation. At this mo-

79 For an introduction to the theoretical concepts involved here, see Willems 1978, esp. 84–8.

ment, published data on mortuary behaviour in the eastern river area during the Early-Roman Period are too limited to allow more detailed conclusions or even hypotheses.

8.4 THE MIDDLE-ROMAN PERIOD

The evidence presented in chapters 4 (fig. 23) and 5 shows that a maximum of 293 settlements existed during the Middle-Roman Period. Of this total number, not more than 38 sites are only probably or possibly middle-Roman settlements. This means that the number of definite middle-Roman settlements is 255, which is considerably more than the numbers of definite late-Iron Age or early-Roman settlements, which are 155 and 142, respectively. This difference is, of course, primarily due to the finds which are, in contrast to the preceding periods, both abundantly present and accurately datable.

Nevertheless, there is also a clear absolute increase in the number of settlements. As far as the total number is concerned, there will undoubtedly be some undiscovered settlements in the river area proper but these should be a negligible quantity. There may, however, have been a number of sites on river banks, which have been eroded. Also at least some of the Pleistocene deposits have surely been much more densely settled than Appendix 3 or fig. 62 show.

For the coversand-areas south of the Meuse in Brabant and along the western slope of the ice-pushed ridge south of Nijmegen, this is already a certainty. These two areas alone are likely to have had several dozens of settlements, as is demonstrated by the cluster east of Middelaar, which was discovered by systematic field-walking in just a few years, and also by at least 19 known sites from Brabant which could not be included in the catalogue and Appendices. Virtually nothing is known about the loess-deposits between Nijmegen and Kleve, most of which are covered by the Reichswald. A really dense occupation there would probably have yielded more sites than the few which have been found so far, but the inaccessibility of the terrain for archaeological observations may well have left a considerable number of settlements undiscovered. The same is not true for the Montferland and the area north of the Rhine, not so much because these regions are beyond the *limes* but especially because large portions of them are less suitable for habitation.

Given these rather general and vague considerations, it is impossible to reach a precise and reliable estimate of the original number of settlements. There are, however, certain limits which can be proposed. On the basis of what has been said so far, it can be taken for granted that at the very least about 100 settlements have either been eroded or, more importantly, are yet undiscovered. On the other hand, it has been established that, for example, in the most densely settled loess areas of the Kreis Bergheim (Germany), the density of settlements approaches 1 site per km^2. The average is certainly less than that. Comparable figures of 1 site per km^2 after intensive fieldwalking were recorded in the Thames Valley (England).[80] Approximately the same can be said of the settlements on the Holocene river-deposits of our region.

On the basis of data like these, and in view of the very diverse nature of the Pleistocene deposits compared to the loess and river clays, it is very unlikely that the maximum number of unknown settlements is more than 250. The theoretical upper and lower limits of the original number of middle-Roman settlements can thus be established at 350 and 500. For reasons to be discussed below, the latter figure is to be preferred.[81] This means that the 255 sites to be considered here represent approximately 50% of the original number, which is a quite satisfactory rate of recovery.[82]

It also means that the settlements which are available for analysis probably include the total range of variability. It remains possible that certain specific settlement types escape attention by processes of selective destruction or coverage, but it is very unlikely that an important settlement type can be overlooked. One example are the definitely selectively destroyed frontier-forts on eroded parts of the southern bank of the Rhine. These have still left many direct and indirect clues to their existence and location. Besides this, the chance of not noticing differentiation for this reason is minimal, compared to the overall difficulties in tracing settlement types, especially where this has to be done on the basis of surface finds only.

Fortunately, that is not necessary for all sorts of settlements. There is some historical information about specific sites as well as on types of settlements, and there are

80 Bergheim: Hinz 1969, 57 and *Taf.* 50 or 54; Thames Valley: Miles 1982, 63.

81 See paragraph 11.4.2, p. 416 f.

82 For the Holocene river clays, the percentage is much higher (cf. chapter 3, p. 75).

also quite a few sites of which sometimes considerable parts have been excavated. Unfortunately, virtually none of the most important excavations (those of the last 15 years) have yet been published completely, which means that detailed chronological and especially structural data on these settlements are not yet available. On the other hand, the preliminary reports usually give at least an impression of the structures and sorts of settlements involved. These excavations are of vital importance as points of reference in the discussion of differentiation between settlements.

Just as during the preceding period, there is a basic dichotomy between civil settlements and military establishments, which were necessary for the control of the frontier. And because of the definition of the Middle-Roman Period, which starts with the fixing of the frontier along the Rhine and ends with its disintegration, this dichotomy lasts during the entire period. From a chronological perspective, there are variations in the structure of the military settlement system as a whole and also in the development of individual sites, but except for a legionary fortress all different classes of military settlements are continuously present.

8.4.1 Military Settlements

Although a reasonable amount of data exists about military establishments in the eastern river area, these are not the primary source for discussing the different sorts of sites. The Roman military presence may be adapted to specific circumstances, but its organizational aspects are not specific for the region as such. They are determined by the overall structure of the Roman army and by the form in which that was expressed in the northwestern provinces.[83] In order to gain an overview of the types of military sites which are or could be present in the eastern river area it is more profitable, therefore, to look at the range of possibilities first.

In addition to the legions the Roman army consisted of auxilia. The composition and organization of auxiliary troops changed with the passage of time,[84] but they were organized mainly into infantry and cavalry regiments of nominally 500 men, the *cohortes* and *alae quingenariae*. Units of double size, the *cohortes milliariae* and – outside Germania Inferior – a few *alae milliariae*, also occurred, as well as a mixed form of infantry regiments with additional cavalry units of about 120 men, the so-called *cohortes equitatae*. There are several other auxiliary units, such as the irregular tribal troops (*tumultuarii*) which were especially important during the Early-Roman Period, and the later *numeri* and *cunei*, which seem to have been special task forces at a time when the regular auxiliary units became more and more indistinguishable from the legionary forces.

Although auxiliary troops were attached to legions, their main task during the Middle-Roman Period became the control of the frontier. Units were stationed in forts (*castella*) along the *limes*. Not all these forts were alike, their size depending on the strength and type of unit which constructed the fort. For example, the *cohors II Brittonum milliaria equitata* in Vechten needed a fort of c. 2.7 ha, while the fort in Zwammerdam (Period 3) measured only c. 1.2 ha, which implies that it probably housed only a detachment (*vexillatio*) of, in this case, a *cohors equitata*.[85]

This last observation and, for example, a succession of forts such as found in Valkenburg,[86] also indicate that the forts were built to fit the unit using it and not to a prescribed universal format. Therefore, the actual situation is that of an almost continuous size-distribution of forts. The same is true for legionary fortresses, but there is still a gap between relatively small fortresses, such as that in Nijmegen (period 4) which measured c. 15 ha, and the larger frontier forts.[87] At the other end of the scale, there seems to be a similar gap between the *castella* and even smaller military establishments.

The *limes* system was not intended as a Maginot line, a defence against major attacks.[88] It could cope with smaller incursions but it was mainly intended as a baseline for controlling the area beyond the border and as a supporting infrastructure for the forward defence, that is, offensive operations to intercept large-scale attacks beyond instead of at or even behind the frontier. In such

83 Although the regional level is too low for an evaluation of military settlements, for the empire as a whole it is too high because there were all sorts of variations in its different parts. Even between provinces as close together as Britannia and Germania Inferior and Superior, there are clear differences (Frere 1978, chapter 11).

84 Kraft 1951; Alföldy 1968, chapter 3; Sebesta 1972, part 1; Holder 1980, chapter 1 and conclusion.

85 Haalebos 1977, 87.

86 Glasbergen 1967.

87 Even this gap is closed in some instances, e.g., by the so-called '20–30 acre' fortresses in Britain (Frere 1978, 253–4), although auxiliary forts there tend to be rather small again.

88 See e.g. Von Petrikovits 1978, 105–6 and Luttwak 1976, 61–80.

a system of defence, communication and transport networks are vital, which implies an adequate road system and a signalling system between the forts and fortresses. The *castella* were built at more or less regular intervals, but the exact location was also determined by the structure of the landscape. For surveillance purposes, and also because communication took place by fire and smoke signals, intermediate posts and watchtowers were necessary additional elements in the *limes* system. They are at the lower end of the size-scale and a few examples from Germania Inferior, such as the fortlets of Rheinhausen-Werthausen (c. 0.17 ha) and Neuss-Reckberg (c. 0.11 ha) are clearly different from even the smallest *castella*. The watchtowers are even smaller, of course.

As far as the typology of military settlements is concerned, there are only two additional types which should be mentioned. These are, first, the so-called *stationes beneficiariorum consularis*, which were posts along important routes and in communication centres. They were manned by detachments from legions in the province, under command of a *beneficiarius consularis*, and their main function can be described as police work. Unfortunately, the presence of *stationes* is known mainly through inscriptions from *beneficiarii*,[89] not through excavations.

According to MacMullen, the detachments lived in their own tents, in private houses, or in unfortified *mansiones*. The last remark is important because it distinguishes middle-Roman road stations from the late-Roman examples, which were always fortified, but also because it illustrates why it is still largely unknown what they looked like. They will be difficult to identify when one of the elements which are usually characteristic for a military site, namely, its defence-works, is absent. To some degree, the finds may offer a clue. *Stationes* in the hinterland can, perhaps, be recognized from the presence of military tile-stamps, but for the region immediately behind the frontier this alone is not sufficient, as witness the finds from the eastern river area (paragraph 6.5.1). It seems, therefore, that identifications of settlement sites as *stationes beneficiariorum consularis* can only be tentative as long as excavations are lacking, and even then the nature of the sites is not immediately obvious.[90]

The confusion about road stations is also illustrated by the excavation of a military establishment in Rheinberg, where a central wooden building was surrounded by a ditch. For this reason, it should not be regarded as a possible *Beneficiarierstation* but as a fortlet, watchtower, or signal station.[91] A second reason is the location of the site: in isolation along the *limes* road. Apart from the issue of defence works, small isolated military sites along the *limes* should, for reasons of terminological clarity, not be referred to as road stations. Their location implies a primarily military, not a police function. *Stationes* along the *limes* are only understandable in combination with fortresses, forts, or towns.[92]

The second and last type of military settlement which should be mentioned, is the naval base. The Rhine fleet (*classis Germanica*) is one of the better documented fleets of the empire.[93] Its headquarters were probably at Alteburg near Köln, and there must have been other stations further downstream. Examples are Vechten and a (major?) harbour in Katwijk, but it is questionable if any specific naval stations existed, at least along the rivers. Vechten was also – indeed primarily – a frontier fort, and it is likely that most if not all forts along the Rhine had a landing-stage.[94]

To sum up, it can be said that five or six different types of military settlements have to be reckoned with: legionary fortresses, auxiliary forts (probably also serving as naval stations), fortlets, watchtowers, and road stations. These should be identifiable as military sites on the basis of the finds and, except for the road stations, by their structure, especially the defence works. Unfortunately, the latter criterion is only useful after excavation, which means that in several cases the finds are the only indication for the fact that a site may be military. It also means that it may not be possible to indicate exactly what kind of military site is involved. It is hardly likely that a legionary fortress would not be recognized as such, but the ancient settlements soils in the river area are not necessarily indicative of the size (which could point to the

89 Bogaers 1962–3, note 131 and MacMullen 1963, 66–70. On *stationes* in general, see a.o. Von Petrikovits 1960, 72–6, Bogaers *op. cit.*, 82–4 and MacMullen *op. cit.*, 55–7.

90 An example is the identification of the *statio* in Köln by Von Petrikovits 1960, 75–6.

91 *Beneficiarierstation*: Binding 1968, 132; fortlet: Bogaers/Rüger 1974, *Karte* 2 and 124–5. The first ditch enclosed an area of c. 0.1 ha.

92 Cf. Von Petrikovits 1960, 74.

93 See Starr, 1941, 141–52 for an account of its history.

94 Cf. Van Es 1981, 117. On Katwijk, with 20% CGPF stamps, see Bloemers/De Weerd 1984.

type) of the settlement involved. The recognition of unexcavated military sites is further obscured by the fact that they are normally associated with non-military settlement(s), the *canabae legionis* and the military *vici*. These have been excluded here, although they were in some respects also military sites. In practice, however, they were inhabited by civilians who came to live there as a spontaneous,[95] but more often probably induced,[96] response to the presence of a fort and the various possibilities which that offered. For this reason, the settlements associated with forts will be discussed below, together with the other settlements in the river area.

Although there are several difficulties in identifying unexcavated settlements as military establishments of some sort, the inventarisation of findspots in the eastern river area has produced several additions to the already known or assumed military sites, some of which are (partially) excavated. In order to present the available data in a coherent way, all these sites will be discussed separately, within the above-mentioned typological framework which is, in part, also a hierarchical ordering.

Legionary fortress

After the end of the occupation of the large early legionary camp at site 412, presumably in AD 16,[97] there are no definite traces of a fortress in the eastern river area until one was built at the same site in AD 70. The current assumption is that no legion was stationed in Nijmegen between AD 16 and 70. There are, however, quite a few as yet uninterpreted and/or unreliably datable ditches on and around the Hunerberg, as well as two sites (403 and 417) which were occupied until c. AD 70 and which are very unlikely to be entirely – or at all – civil settlements.[98]

Pending further excavations, it is quite well possible that part of a legion was stationed in Nijmegen under Claudius and Nero on any of these sites. The fact that no less than two legions, the *legiones V Alaudae* and *XXI rapax* (which was replaced by *XV Primigenia* around AD 46) were stationed in nearby Xanten-Vetera until 69 offers the possibility of envisaging legionary detachments in Nijmegen. At least the *legio XV Primigenia* is also attested to by tile-stamps (fig. 50) and the evidence for military presence in Nijmegen before AD 70 is certainly too extensive to attribute it to a simple *statio*.[99] All this is, however, no more than a possibility. It is equally well possible that Nijmegen was occupied by auxiliary units, such as the Batavian cohorts and ala, which may have been located in their own tribal area before AD 43 and, partly, also afterwards.[100] In fact, sites 403 and 417 could well have started as camps for auxiliary troops on both sides of the legionary camp (412) and have remained in use after the departure of the troops stationed in that camp.

Whatever the solution of these problems may prove to be, it is unlikely that full-fledged legionary fortresses were located anywhere except on site 412, and the first tangible one is period 4 of the local sequence, occupied by the *legio X gemina*. The fortress was occupied by a succession of different troops from AD 70 until IId,[101] but with the possible exception of the early-Flavian or still older phases, it never housed an entire legion. Both periods 4 (c. 15 ha) and 5 (c. 16.5 ha; see fig. 77) are too small, which implies the permanent absence of one or more legionary detachments. For the other units which later occupied the fort, the *vexillatio Britannica* and a detachment of the 30th legion, it may have been too large, but apparently it was never reduced in size.

Auxiliary forts

Outside Nijmegen, there are no less than nine forts or possible forts in the area covered by the map, most of

95 Webster 1969, 204.

96 For an argumentation, see Sommer 1984, 15.

97 Bloemers 1979a, 27. It remained a perhaps unoccupied but at least precisely bordered area, which is why its perimeter is still indicated on fig. 66.

98 For the Hunerberg ditches, see Bogears/Haalebos 1979b, 45–7, esp. *Abb.* 44. For the interpretation of sites 403 and 417, *supra*, 233–6.

99 Rüger 1968, 62. This does not, of course, imply that there cannot have been a *statio* in Nijmegen (see p. 261). It should also be mentioned here that the stamps of the *legio V Alaudae* recorded for Nijmegen by Rüger (catalogue nos. 18 and 20) are unreliable (pers. comm. J.E. Bogaers).

100 Alföldy 1968, 13–4, 45–8, 145.

101 According to Bogaers/Haalebos 1979b, 41, the fortress was abandoned in about AD 175. This date is based a.o. on a relatively early dating of stamps of the *excercitus Germanicus inferior*. Although Bogaers' (1965a) critique of the Holdeurn chronology (Holwerda/Braat 1946) is in itself correct, his conclusions regarding the EX GER INF stamps are contradicted by chronological data from elsewhere, as was already indicated by his co-author (Haalebos 1977, 177–8). The end of the occupation or, rather, the maintenance of the fortress should therefore be put at some time *after* AD 175 (and preferably shortly after). There is no evidence for 3rd-century occupation or maintenance.

them located along the Rhine. Only tiny parts of a few sites were ever excavated, so that many uncertainties arise. Nevertheless, the picture of this part of the *limes* as a whole is emerging clearly, and it is quite certain that the old and very unlikely idea of an unfortified Batavian *limes* is not correct. The evidence employed to identify (or reconstruct) sites of forts is varied: in the absence of sufficient data on size and structure, locational factors and finds are the primary sources of information. The classical sources are, unfortunately, of very little value in this respect. There are at least three good reasons why they can only be used afterwards, as secondary sources to be contrasted with the combined archaeological and geological data.

First, historical data such as distances between forts are not always unambiguous and they have occasionally been shown to be wrong. Second, in the absence of clear traces of Roman roads, it is not possible to calculate distances exactly, even though the essentially reliable reconstruction of the geological situation in the Roman Period, which is now available, offers reasonable compensation. Third, the number of fixed points (identifications of sites with names from classical sources which are beyond any doubt) is very low indeed. Even for Nijmegen-Noviomagus, which certainly is a fixed point, one can never be sure from which site the distances were calculated (sites 399 and 412 are almost 3 km apart!).

For these reasons, historical data on specific sites provide information which cannot be used independently. This does, of course, not imply that they do not add valuable information once an acceptable identification has been reached. The mere fact that a site is mentioned at all on a map or in a book or itinerary may say something about its importance or nature, which may not yet or even never be archaeologically traceable. In the eastern river area, the following sites are possible locations of auxiliary forts.

Site 37 Kesteren

Kesteren is generally considered to be the fort Carvo(ne), mentioned on the *Tabula Peutingeriana* and in the *Itinerarium Antonini*. The available evidence has been compiled by Bogaers and shows that a military establishment in Kesteren is indeed very likely.[102] As mentioned in the catalogue, the location of Carvo cannot be site 38, which was partially excavated and shown to be a settlement.[103] Both sites 38 (a *vicus*?) and 39 (a cemetery) have yielded enough military finds to assume that a military settlement must have existed in the immediate vicinity. Because there is no other settlement which could qualify as such south of the dike (*Rijnbandijk*) the conclusion must be that the missing fort was located on the southern bank of the Roman Rhine to the north of the present dike, an area which was eroded in post-Roman times. The fort and, incidentally, also the northern part of site 39, are therefore likely to have been washed away and site 37 is a hypothetical site only, even though its existence need not be doubted.

There is no real clue to the nature of the military establishment near Kesteren. Bogaers, and also Van Es, have considered the possibility that it may have been nothing more than a station of *beneficiarii*. This is based on a sherd with graffito BEN(e)FICIARIORVM found on site 38. This could imply that site 38 is or includes a road station which would explain both the military finds and the relative lack of native material and, consequently, even remove the need to postulate site 37.

This is, however, rather unlikely because the presence of a station would normally presuppose rather than exclude a fort and it is difficult to see how an unprotected road station would fit into the *limes* system. Site 38 does not qualify as, for example, a fortlet or other intermediate frontier-post. Therefore, it remains very likely that the specifically military settlement in Kesteren is indeed missing and therefore eroded. It does not necessarily have to be an auxiliary fort, unless the identification with Carvo is correct. This is because it is improbable that a relatively insignificant element in the *limes* system would have been mentioned on the Peutinger map or in the *Itinerarium Antonini*. Although there is no reason why the generally accepted identification should be questioned, it is certainly not beyond all doubt. The available data on its location: 8 *leugae* (17.5 km) east of Levefanum (Rijswijk-G.?) and 13 *leugae* ($\pm$ 29 km) west of Castra Herculis (Meinerswijk?) or 22 ($\pm$ 49 km) west of Harenatium (Rindern?) are only sufficient for an approximate localization of Carvo. All that can be said further is that, in view of the geological possibilities, the distances and identifications do not disagree with each other.

Site 18b Randwijk

As shown in paragraph 3.4, one of the most important

102 Bogaers, in Bogaers/Rüger 1974, 70–1.

103 *JROB* 1977, 32. See also Van Es 1981, 103–4 and note 281, and below, p. 269 and note 170.

north-south routes through the river area runs from Herveld over Zetten to Randwijk and crosses the Rhine at Lexkesveer, after which there are three ways further north: one along the brook of the Heelsumse Beek, one along the Renkumse Beek, and a third route along the border of the high-lying coversand from Wageningen towards Ede. Especially the latter one seems to have been important during the entire period covered by this study.

This route not only crossed the *limes* road along the Rhine at Randwijk-Lexkesveer but, as proposed in chapter 3 (p. 68), it had itself probably military importance. This importance is rather obvious in the context of the forward defence system, even without specifically military finds or a direction towards an outpost-fort such as perhaps in Ermelo,[104] but it is only an additional argument for attaching special significance to the area around Randwijk: no doubt any route crossing the Rhine, and thereby the Roman frontier, would have been guarded. In view of the archaeologically visible importance of the route in question it is, therefore, necessary to propose the presence of a military settlement of some importance also. The fact that its assumed location is virtually midway between Kesteren and Meinerswijk provides further support for this argument. A comparable situation is found in Cuijk (site 499), where the highway from Tongeren to Nijmegen crossed the Meuse and was guarded at first by a fort, later probably by a *statio*. But this crossing, located in the hinterland, was not part of the *limes* system. The crossing at Lexkesveer was part of it, and the need for troops here, both to guard the crossing and as a link in the entire chain of forts, would have been permanent.

Taken together, the analysis of the landscape, the habitation pattern, and the military situation indicate the presence of a military establishment near Randwijk,[105] which must have been at least a fortlet, but more probably a fort.

In view of this conclusion, there are two facts which need to be discussed. If there must have been a fairly important fort, then why has it left no archaeological or historical traces of its existence? The first of these is the most easily accounted for. The geological survey of the area around Randwijk indicates the presence of substantial post-Roman (channel-fill) deposits, occasionally as far as one kilometre south of the dike. With the exception of a very small area around the church in Randwijk, the Roman deposits seem to have been eroded, and therewith all traces of habitation. This area is not indicated on the Appendices because it is very small. It must be at least a deposit from the Roman Period because it was suitable for habitation in the Early-Middle Ages (site 71). Unless site 71 eventually proves to contain more than is known today,[106] it is very likely that the military settlement near Randwijk was washed away and the remains now lie deeply buried under later deposits. This is, in fact, the same situation as in Kesteren and also in the Loowaard and Bijlandse Waard.

The only finds which may be associated with the proposed fort are one (or two) tiles with a military stamp, possibly a few sherds, and presumably secondarily used Roman building stone, all found on the Westberg in Wageningen, directly opposite Randwijk (site 18a). It is conceivable that this material was robbed from the fort before it was eroded, but that is merely a possibility which proves nothing.

As far as historical data are concerned, the absence of references to the fort is curious, at least when it is assumed that the current identifications of other forts are correct. If the reported distances are more or less reliable and correctly interpreted, it is not well possible to identify Randwijk with Carvo (instead of Kesteren). But then it has to be explained why a fort in such a prominent geographical location has not been indicated in the surviving sources, while others are. There are too many uncertainties involved to warrant a serious discussion of this problem (if it is indeed a problem!), but the absence of an obvious identification is not in accordance with expectations resulting from the discussion presented above.

There is, however, another source which could, to some degree, support the arguments for a fort near Randwijk, and that is the toponomy. Randwijk belongs to a group of very old place-names, with *-wijk* suffixes, which includes Poederooien (*Podarwic*), Vreeswijk (*Fresionouuic*), Zandwijk (*Sandewihc*), Wijk (bij Duurstede)

104 See chapter 3, 68 and notes 207–210.

105 Modderman (1949, 75, no. 3) already reached a similar conclusion, but the possibilities which he indicated, sites 70 (his no. 59) and 73 (his no. 70), are too far away from the Roman Rhine.

106 For unverifiable references to possible Roman material from site 71, see *ER* III, 115, paragraph 6.

(*Uuic*), Rijswijk-G. (*Riswic*), Slijk Ewijk (*Euuic*), and Meinerswijk (*Meginhardiswich*).[107] All these names are known from the early-9th to early-10th century. The origin of *wijk* is probably the latin *vicus* and the Early-Medieval meaning of the term is primarily 'emporium'.[108] In Wijk bij Duurstede, and perhaps some of the others, *wijk* has a slightly different meaning according to Blok and Koch. However this may be, it is probable that these names refer to fairly important medieval settlements. Such sites often turn out to be directly related to important Roman sites, such as forts, and at least two of the above-mentioned settlements, Rijswijk-G. and Meinerswijk,[109] belong to that category. Randwijk could thus be another case where the presence of an important early-medieval settlement is, as it were, caused by the location there of a valuable piece of property (for the king or nobility) in the form of the remains of a Roman fort.

Site 117/118 Driel

As was also noted by Modderman,[110] the channel deposits between Driel and Elst and from there to Nijmegen provide an excellent north-south route through the river area. Although these deposits can no longer be regarded as belonging to the main Rhine channel in the Roman Period, a small branch of the river was still active between Driel and Elst. The Drielsche Veer is a crossing of the Rhine which may go back as far as the Roman Period, but there is no apparent route of any importance going further north.[111] Nevertheless, Driel is at least a point where an important land and water route to the hinterland reached the Rhine frontier.

Although there was a fort in Meinerswijk about 3.5 km to the east, Driel is a spot which is likely to have had a military occupation itself. It is very curious that the suggestions put forward by Modderman, who reasoned along the same lines, never received the attention they deserved. He was even able to identify a site which could qualify as a military site because of the relatively large amount of Roman material known at that time.

This site is identical to our site 117, which is located at the confluence of the secondary river branch coming from Elst and the major channel of the Roman Rhine. Both the general and the precise locations are thus indicative of an important and probably, at least partially, military settlement. Because there has been only very little erosion of deposits in post-Roman times at this spot, the settlement (sites 117 and 118) has survived intact. In fact it has, so far, escaped almost all imaginable modern destructive activities as well. Most of it became grassland shortly after 1945 and part of it is now covered by greenhouses. The detailed soil survey[112] indicates that it was quite large: about 8.5 ha. This could well be the approximate size of the settlement, because the entire area has almost constantly been surveyed over the past 40 years and Roman material was found almost everywhere. Moreover, although at least site 117 was occupied from the Middle-Iron Age onwards, the entire site was never fully occupied after the Middle-Roman Period. There are only a few Merovingian sherds which could point to a small settlement, and some occasional later material, which is completely insignificant. It is probable, therefore, that the size of the ancient settlement soil is directly related to the size of the Middle-Roman settlement.

Although exceptional, the size of the settlement itself does not allow any conclusions to be drawn about its nature. It is large enough to have contained an auxiliary fort and associated *vicus* but, in the absence of excavations, nothing can be said about the structure of the settlement. Fortunately, there are at least a few clues in the rather large number of finds. Most of these are surface-finds, although some material was collected after occasional digging activities by farmers. As far as pottery is concerned, it is important that site 117/8 has yielded predominantly Roman or Gallo-Roman wares. The general ratio between Roman and native pottery is c. 4:6, but the latter category appears to consist mainly of late- (and some middle-) Iron Age pottery. For the Roman Period, the percentage of native pottery is therefore quite small. As far as Roman wares are concerned, the

107 See Blok/Koch 1964, 50 and Blok 1965, 53.

108 For an overview of the relevant literature and theories, including a new and different one, see Schütte 1976, 141–64 and 196–7.

109 Rijswijk (G.): see Van Es 1984; Meinerswijk: see chapter 9. Note that in this and the following chapters there are two different places called Rijswijk, one (the fort) in the Province of Gelderland, on the Rhine east of Wijk bij Duurstede (Rijswijk-G., see fig. 130), and one in the Province of Zuid-Holland (Rijswijk–De Bult, see fig. 80).

110 Modderman 1949, 74–5, sub 1.

111 Compare, however, chapter 3, 68, note 207 and see Heidinga 1984, 249, 263. See also Holwerda 1931 on the finds of site 125.

112 Zegers/Zandbergen 1958.

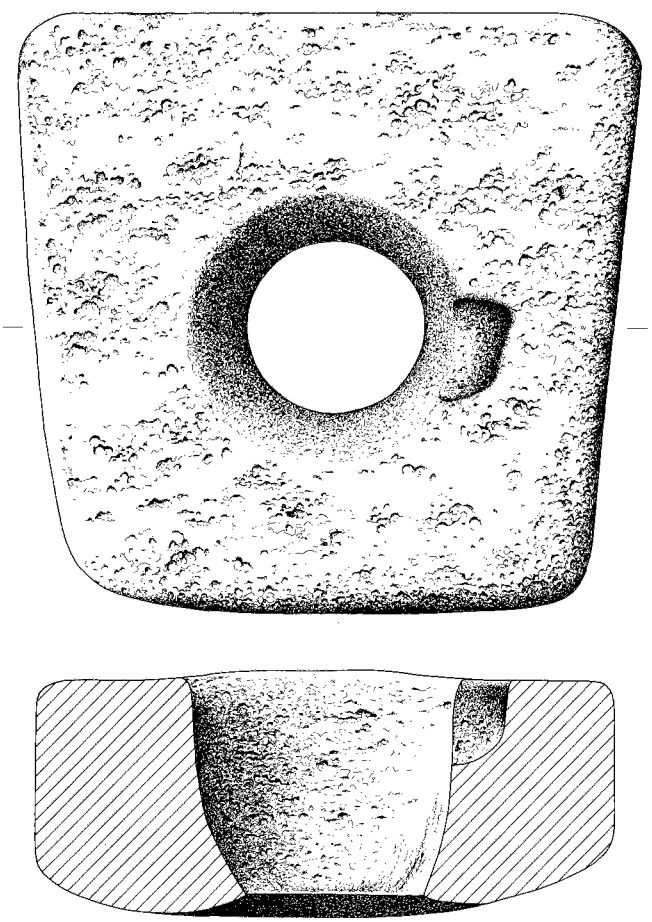


Fig. 70 Tuff pivot-stone from Driel (site 117). Scale 1:4.

large number of terra sigillata sherds (over 200) is remarkable. They include many very small fragments and therefore point to some collecting bias but, on the other hand, such quantities have never been found on the surface of other settlements in the river area and, in most cases, not even during excavations. Also the presence of 'fine Nijmegen' ware, which seems to have been made primarily for the army, is worth noting.

The tile fragments are fairly small, and none of them bears a stamp. Most of the other building material (fragments of tubuli and tuff) are also rather small, with the exception of a number of tuff blocks from a hole which was dug at site 117 in 1980. These include a broken but complete block of c. 63 × 63 × 24 cm, with a slightly conical hole in the middle (fig. 70; fracture not drawn). The hole is somewhat worn out to one side, and no longer completely round. It is evidently a pivot-stone, with something turning in it on one side only. Although the central hole is rather large (the diameter is 18 cm) this makes it likely that the stone served to hold the pivot of a door. It may have been broken deliberately to remove the door.[113]

In association with the tuff, four fibulae were discovered (fig. 71). No. 1 is a large wheel-shaped plate fibula with rich polychrome enamelling, partly in millefiori technique. It belongs to Ettlinger's type 42.3.[114] No. 2 is a hinged plate-fibula with openings and plastic 'trumpet'-decoration and no. 3 should probably also be called a hinged plate-fibula, although its shape looks like a mixture of certain types of enamelled bow-fibulae and knee-fibulae.[115] Of special interest is no. 4. It is a tinned knee-fibula with spring-cover, a bow which is trapezium-shaped in section and a transverse pin-catch. According to Böhme[116] this sort of fibula is almost exclusively found in forts (on the Upper German and Raetian *limes*) and can therefore be considered a typical soldier's fibula. The same is true for the fibula no. 2.

As long as excavations are lacking, all these finds are insufficient to prove beyond doubt that site 117/118 is definitely a military settlement. But especially because they are mostly surface finds, they certainly support the proposition that it is. The presence of stone buildings and a large amount of rather precious pottery at such a prominent location along the *limes* is, in itself, considered to be quite convincing. The absence of military tile stamps and equipment is, considering the find circumstances, not really significant. The only somewhat discomforting fact is that as yet no graffiti have been noticed on the pottery, which are to be expected at a military site, especially when so much terra sigillata has already been recovered.

113 This is apparently the only practical method. For a discussion of the technical aspects of pivot-stones, see Gillam/Mann 1970, 3–5.

114 Ettlinger 1973. For an almost identical specimen from Weissenthurm, see Eiden 1983, *Taf.* 152, 1.

115 No. 2: Böhme 1972, type 46c; no. 3: Ettlinger 1973, type 42.3, but see Van Buchem 1941, Pl. 14, 16–24 and Pl. 17, 20–5 respectively.

116 Böhme 1972, 20–2, type 21b.

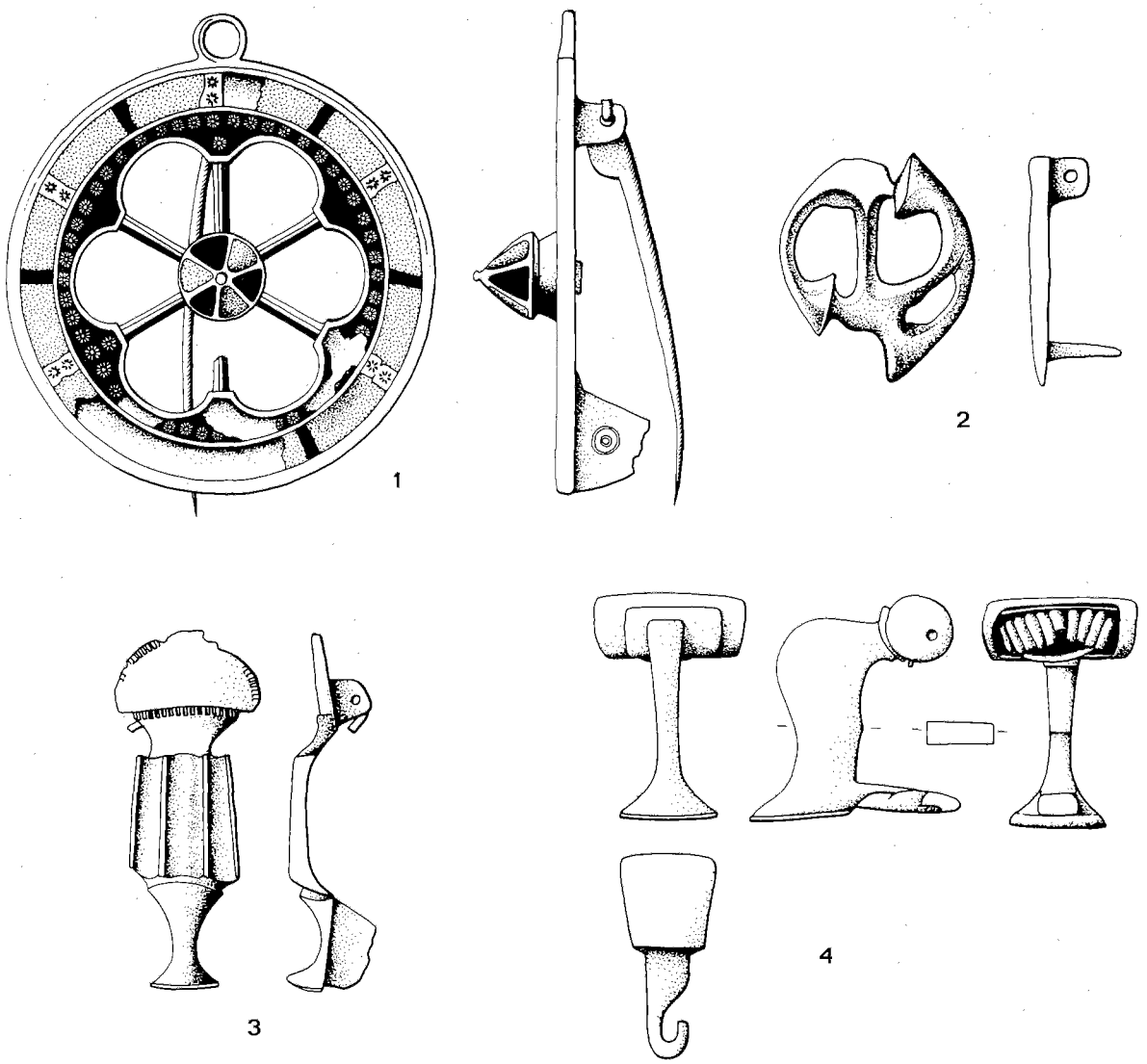


Fig. 71 Fibulae from Driel (site 117). Scale 1:1.

On the other hand, some of these features occur in the immediate vicinity, at sites 120 and 125. Site 120 is the findspot of (part of) a flagon bearing a graffito (fig. 72). It is an offering to an unknown deity from a certain Sallios, who belonged to the *turma* of Caius. A *turma* is a small cavalry unit, usually part of an *ala*. The metalware from site 125 is also undoubtedly of military origin. The interpretation of this find is dubious: it may be a hoard, lost booty, or also represent one or more offerings. But its military origin is hardly disputable, and it may not be mere chance that the finds can also be connected to a cavalry unit.[117] Both finds could mean that the settle-

117 Cf. Holwerda 1931. The graffiti M. Muttieni could point to a cavalry officer having several horses.

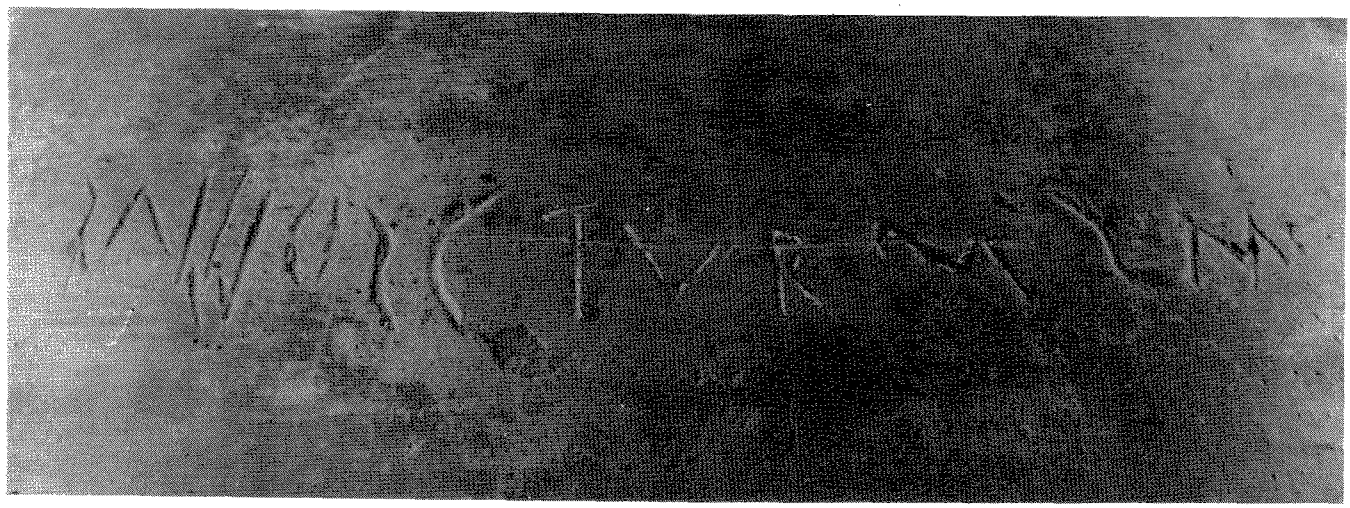


Fig. 72 Inscription on a smooth ware flagon from Driel (site 120). The text should be read (cf. J.E. Bogaers) as *Sallios C(ai) turm(a) s(olvit) m(erito)* and translated as 'Sallios, from the turma of Caius, has [by offering this flagon to the (unknown) deity] fulfilled [his vow], with reason'.

ment in Driel is the cavalry fort which Holwerda assumed to be the place of origin of the finds from site 125 or that a *cohors equitata* stayed there.

It is, of course, not at all necessary to assume that site 117/8 is the location of a complete auxiliary fort. A smaller intermediate fort or even a fortlet, in combination with a fairly wealthy commercial settlement, is just as possible. It may be difficult to locate the actual fort-(let) without extensive excavations, although the surface finds indicate that the military settlement is most probably located on site 117 (the parcels known as Baarskamp, Lizesland, and Ruttens Hofstede). This is why site 117 is indicated as a (probable) fort and 118 as a settlement, presumably the associated *vicus*. Both sites were occupied during the entire Middle-Roman Period.

Site 126 Meinerswijk

The evidence concerning the fort in Meinerswijk is presented in chapter 9. The existence of a fort at site 126 was confirmed by a small excavation carried out especially for that purpose in September 1979. Before that time, it was only a hypothesis, based on some locational aspects and on the finds. This is a situation which is more or less comparable to the present state of affairs for the three sites in Kesteren, Randwijk, and especially Driel. Although this does not imply that the hypotheses on those sites are therefore also correct, it does supply additional support for them and it certainly proves the value of the assumptions involved. It is worth while, therefore, to outline the circumstances which led to the excavation in 1979.

Site 126 was discovered because it was indicated as an ancient settlement soil on the detailed soil survey map of the northern part of the Overbetuwe.[118] The accessibility of the site for finding archaeological material was minimal, because it was all grassland (cf. Driel). Finds were collected in the spring, in molehills. They included 109 small sherds, of which only 3 were hand-made native ware. Among the 106 Roman sherds were no fewer than 14 pieces of terra sigillata. Such a low value for native ware and high percentage of terra sigillata are very unusual for sites in the river area (but they are paralleled by site 117/8). The finds also included dozens of tiny fragments of tuff (often with some mortar) and one of limestone, as well as 20 pieces of roof-tiles, none bearing a stamp. After some sounding for stone walls, several were found and one was investigated by means of a small test-pit, which yielded several large blocks of tuff.

The mere fact that site 126 is situated on the bank of the Rhine was, in combination with these findings, enough to interpret it as a probable fort. There are, however, several other locational aspects involved, concerning both the general and the specific location of the site. As far as its general location is concerned, the absence of a direct route to the hinterland makes the presence of a fort not as immediately obvious as in the cases of Driel

118 Zegers/Zandbergen 1958.

and Randwijk. It is, however, important to note that Meinerswijk is located at the spot where the Rhine turns westwards, and therefore at the easternmost point where the river could be crossed to go north over land.[119] It is also situated just downstream of the presumed Roman Rhine-IJssel fork, which provides a water route to the north as well. These factors may have played a part in the decision to build a fort in Meinerswijk, especially because that decision had already been taken in the Early-Roman Period. The presence of an early-Roman camp in Meinerswijk was demonstrated only after the excavation, but the sherds from the molehills included at least one early fragment (of Arretine t.s.) as an indication for early occupation.

The specific location of the fort became clear after detailed borings by the Netherlands Geological Survey which followed the discovery of the site (see fig. 18). It is located on the convex bank of a meander curve, and therefore at a relatively high spot. This meander had been cut off, so the fort was located south of the main Rhine channel and probably surrounded to the south by an oxbow lake. The latter may, at the same time, have formed a convenient harbour. It is clear that conditions such as these must have been very attractive, both for a base camp during the early-Roman offensives to the north and for a later *limes* fort.

Site 135 Huissen

The evidence for a military settlement in Huissen is rather problematical. Although a small excavation in 1951 produced a fair amount of material, including several military tile-stamps, all of it was found in secondary position, under a medieval *château à motte,* and the same is true for the recent finds at site 546.

Bogaers[120] interpreted these finds as material from a fort of unknown location, but with the assumption that it was in Huissen. The former interpretation is very likely correct, but the latter need not to be true. The Roman material may have been transported to Huissen in late- or post-Roman times, but there is no proof for that either: only the circumstances that possible sites of origin are close (126) or very close (194).

Although the Roman Rhine-IJssel fork cannot be located precisely, it is clear that Huissen was probably nearer

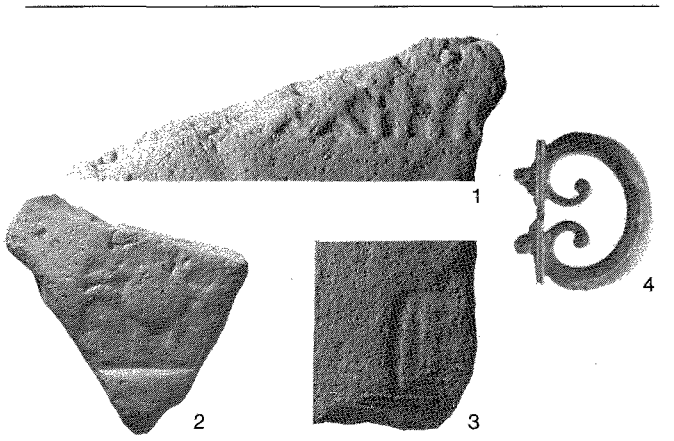


Fig. 73 Military tile-stamps and bronze belt-buckle fro Loowaard (site 194). The stamps are read as follows:]XXIIPF[, 2 LEGXXX[]AC, 3 P[. Scale 1:2 (Photos 1 and 3, IOGA Nimegen).

to it than either site 126 or site 194. This could be an ar gument in favour of a military settlement in Huissen but the presence of site 126 just downstream and site 19 immediately upstream of the fork would seem to b quite sufficient for control purposes.[121] If Huissen wa indeed a military settlement it is, in view of its locatio and the number of finds,[122] also curious that so far not single sherd of early-Roman pottery has been foun there. But this is, of course, not a *conditio sine qua non* and the chances of finding recognizable early pottery ar also small.

All in all, there are no convincing arguments for a mili tary settlement in Huissen, but the possibility cannot b ruled out entirely (cf. Appendix 3). Because it is consid ered very unlikely, Huissen has not been indicated o figs. 6 and 130.

Site 194 Loowaard

During the 1970s, extensive dredging started at the east ern side of the Pannerdensch Kanaal, in the Loowaarc In the soil that was brought up, amateur archaeologist discovered large amounts of mostly Roman (but als later) pottery, as well as other material. Examination c

119 On the importance of the medieval ferry between Meinerswijk and Arnhem, see Verkerk 1983, 22.

120 Bogaers, in Bogaers/Rüger 1974, 73.

121 See also below, p. 260.

122 Not all finds could be examined by the present autho but any determinable early sherds would surely have been nc ticed during the excavation.

these finds immediately showed that the site could not have been a normal settlement and should almost certainly be interpreted as a military fort.
From the composition of the collections of pottery, which contain dozens of completely insignificant wall sherds including sub-recent ones, it is clear that pottery has been collected in an unbiased way. It is remarkable, therefore, that the Roman pottery contains a large number of terra sigillata sherds (well over 100) and, as a peculiarity, an extraordinary amount of smooth ware. In addition, a few sherds of 'fine-Nijmegen' ware may also be present but these are in such a bad condition that the determination is not fully reliable. Native hand-made pottery is virtually absent. The few sherds present are probably datable to the Roman Period, although there is one fragment, a large piece of a pot with a handle, which could be an early (!) Iron Age type.[123]
Even more convincing than the composition of the collections of sherds, is a number of other finds. There is building material such as tuff, indicating the presence of stone buildings, and an ample amount of brick, mostly *tegulae* and *imbrices*. Three fragments bear military stamps (see fig. 73), of which only one is clearly legible. It is a stamp of the *legio XXII Primigenia*. The second stamp is read as LEGXXX[ ]AC and could be of the *legio XXX*, while the third is only a fragment. As far as epigraphy is concerned, the presence of two graffiti on terra sigillata is also important. At least part of the metalware mentioned in the catalogue, such as the belt-buckle (fig. 73,4), is also military. Only one item, an iron spearhead, presumably dates to the Early-Middle Ages and not to the Roman Period.
In view of these finds, there is little doubt that site 194 was a military settlement. Structural details or even a clue to the size of the place are, unfortunately, lacking. It is not even known from what depth the finds were dregded up. Quite a few sherds were, however, somewhat rounded off, so it is probable that the dredging did not destroy a site which was still *in situ*. It is more likely that the fort and later occupation traces were eroded by a post-Roman Rhine channel and redeposited at a lower level. Fortunately, the original geological situation of the site is quite clear. Although it is now at the eastern side of the river channel, the Pannerdensch Kanaal was dug in the first decade of the 18th century and site 194 is located on the western bank of the previous Rhine channel, as is evident from Appendix 3. The site is approximately midway between Meinerswijk (126) and Herwen-De Bijland (182) and, as already mentioned, probably immediately upstream of the Roman Rhine-IJssel fork.

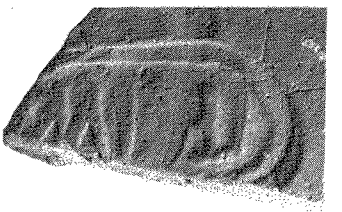


Fig. 74 Military tile-stamp LEGIM (retro) from Herwen-De Bijland (site 184). Scale 1:2.

Another factor which may have influenced the choice of this location is that there was probably a route to the south and west over the channel deposits around Doornenburg that provided a reliable and direct connection to Elst and Nijmegen. If the reconstruction of the geological situation during the Roman period is correct at this point, the choice of location of site 194 becomes self-evident (fig. 75). If the argumentation is turned around, it can also be said that the presence of site 194 supports the reconstruction. If such a reconstruction had been available before dredging started in the Loowaard, the presence of a military settlement somewhere between Angeren and Groessen could even have been predicted on account of the favourable geological situation.

Site 182 De Bijland

The finds from the Bijland were also discovered as a result of dredging which started here even before World War II. A complete inventory of all finds could not be provided in the catalogue, but a summary and the relevant literature have been published by Bogaers.[124] They include ample evidence for stone buildings and also military metalware and tile-stamps (fig. 74) of the *legio I Minervia (Antoniniana)* and the *excercitus Germanicus inferior*.
These finds indicate beyond any doubt that site 182 was a military settlement of which, thanks to the fortunate discovery of an inscription on the tombstone of the soldier M(arcus) Mallius (fig. 17), even the name, Carvium,

123 For the shape, see Kooi 1979, 49, no. 861.

124 Bogaers, in Bogaers/Rüger 1974, 90–2.

is known. Other epigraphical data indicate that the probable auxiliary fort was occupied by the *cohors II civium Romanorum equitata*.

Site 182, which must have been eroded by a post-Roman branch of the Waal, was located near and, for reasons, explained in chapter 3 (p. 53), preferably just downstream of the Roman Rhine-Waal fork. The strategic importance of this location is obvious, but not only because of the water-routes. It should be remembered that the existence of a *limes* road implies a crossing of the Waal very close to the fork. The functioning of the *limes* system must have required adequate means to ensure a reliable way across the Waal during all seasons. These provisions, whether or not they included a permanent bridge or were indeed identical to Tacitus' *agger*, as proposed earlier (chapter 3, p. 53), needed to be guarded as well.

Site 450 Rindern

The finds under and around the church of Rindern have, for a long time, been considered the remains of the auxiliary fort Harenatium (also: Arenatium and Arenacium). They testify to the presence of stone buildings and include military tile-stamps and inscriptions. An overview of the evidence and the relevant literature is provided by Follmann.[125]

If the identification is correct – and there are no reasons to dispute that – Harenatium must have been a temporary legionary camp (of the *legio X gemina*) during the winter of AD 70/71. It is also attested to as an auxiliary fort, but it may have ended as a simple intermediate station.[126]

The fort was probably located just upstream of the Rhine-Waal fork on the western bank of the undivided Rhine. The geological situation is such that a direct road from Arenatium to Noviomagus, as indicated on the *Tabula Peutingeriana*, is indeed quite plausible. As discussed in paragraph 3.4.2 (p. 69), there are even alternatives for this road: a very direct route over the Pleistocene fluvial deposits around Mehr to Zyfflich and from there to Nijmegen, a route along the northern flank of the ice-pushed ridge between Kleve and Nijmegen, and a route along the western and the southern banks of Rhine and Waal over Millingen (see fig. 75).

Site 499 Cuijk

Small-scale excavations in the centre of Cuijk[127] have re vealed the existence of a fort, which is identified with th Ceuclum on the Peutinger map. Unfortunately, only th features and merely a few finds from these excavatio have been published. The former clearly indicate th presence of a fort, because they include a probably re tangular system of ditches (two phases). Parts of woode buildings inside the fort were also uncovered (thr phases), but the excavated surface was too small t determine whether they were barracks.

The fort must have been built under Claudius and e isted until about AD 100, which means that it was built the same time as the construction of the *limes* systen The location of the fort is easily understood because it situated at the point where the Meuse turns westwar and is crossed by the important road from Tongeren t Nijmegen. The fact that apparently a full-fledged fort[1] was deemed necessary at this place implies a direct rel tionship to the *limes* under construction in Claudia times. Its prolonged existence until AD 100 could be r lated to the rebuilding necessary after AD 70, althoug the fort may have been occupied by a smaller force in th last decades of its existence.

After AD 100, the terrain became part of a civil settl ment (site 500). On account of the presence of a 2n century military tile-stamp, a continuation of militar presence has been proposed, presumably in the form a *statio beneficiariorum consularis*.[129] Although militar tile-stamps cannot be interpreted in such a straightfo ward way in the river area, the location of Cuijk is in i self a very convincing argument for the same assump tion.

Other military settlements

Apart from the sites discussed so far, there are a numb of others in the area which may be military settlemen of some kind. Because tile-stamps cannot normally b used as reliable indicators, it is, however, impossible reach definite conclusions on the basis of surface finds.

As already mentioned, the small military posts can be d vided into two categories: *limes* stations (fortlets watchtowers) at or beyond the frontier and road statio in the hinterland or in combination with towns and for

125 In Bogaers/Rüger 1974, 93–5.

126 Auxiliary fort: *Itinerarium Antonini* 256, 3; intermediate station: inscriptions *CIL* XIII, 8702 and 8703.

127 Bogaers 1966 and 1967, esp. *Afb.* 7.

128 The exact size is unknown because the eastern part of t settlement was eroded by the Meuse in post-Roman time The north-south axis measures c. 160 m.

129 Bogaers 1966, 68.

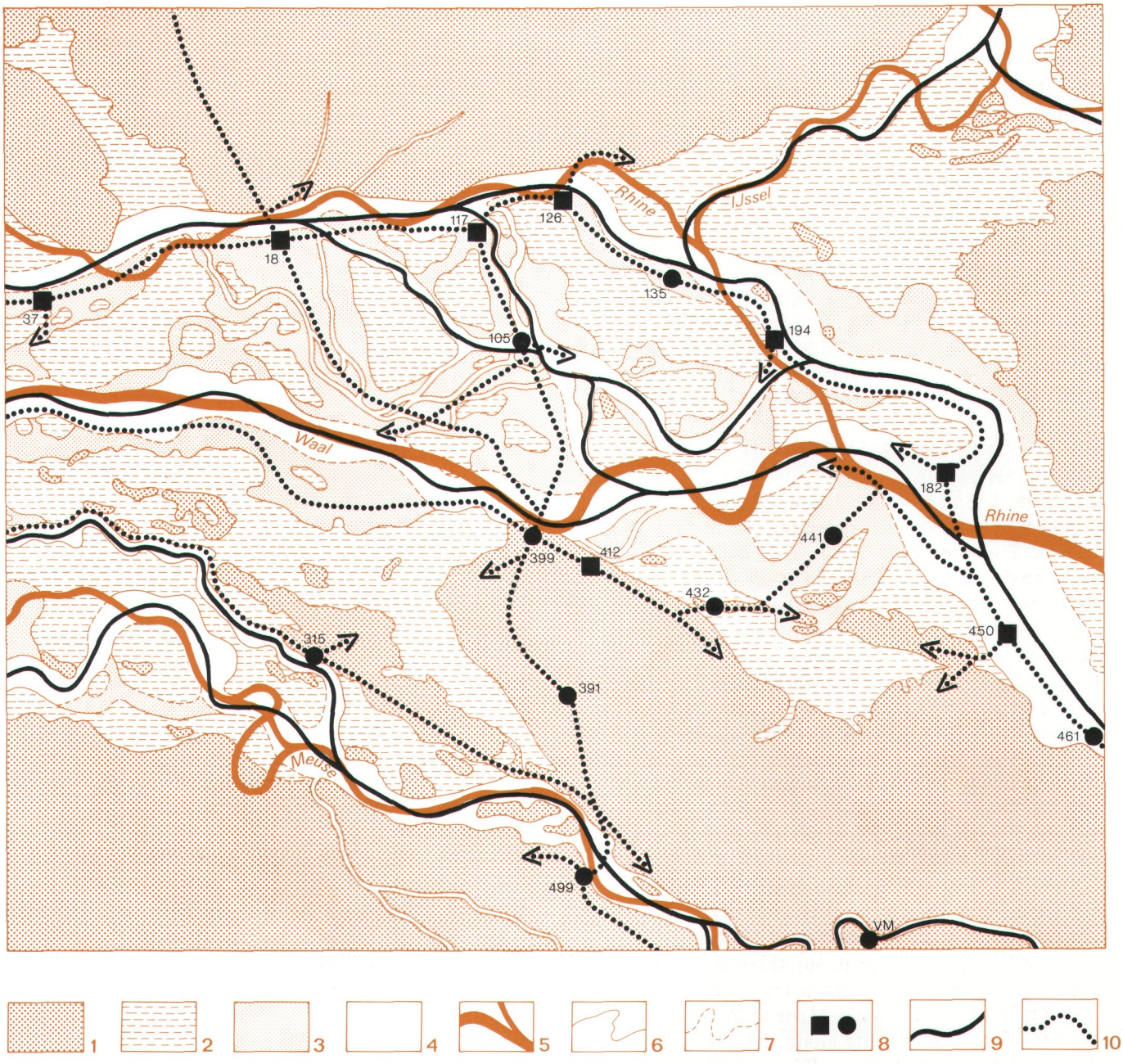


Fig. 75 Military settlements and sites discussed in paragraph 8.4.2 as possible locations of *stationes* or other installations such as signal stations, watchtowers, *mansiones*, or *mutationes*. The system of land and water routes is derived from fig. 22 and thus not intended as a precise reconstruction of the location of navigable channels or Roman roads. VM is the findspot at Viller Mühle mentioned in the text: 1–7 see fig. 58, 8 (possible) military settlements (squares) and other sites (circles) as described above, 9 water-route, 10 land-route. Scale 1:250,000.

along the *limes*. As far as possible *limes* stations at the frontier are concerned, there are only two sites which have not been mentioned before in this chapter, namely, 441 and 461.

Although site 441 (Millingen-Eversberg) must have been several kilometres to the west of the *limes* (fig. 75), it may have been a watchtower or signal-station. This is because small-scale excavations from 1885 to 1887 apparently revealed the presence of a Roman stone building, surrounded by an elliptical ditch.[130] The reliability of these observations is, however, questionable. Later finds have confirmed the presence of a stone building, but the interpretation as a military site depends entirely on the credibility of the feature(s?) presented as an elliptical ditch. Only new excavations can resolve this problem, but for the moment the claims made for site 441 had better not be taken too seriously.[131]

Horn considers site 461 (Qualburg) a possible *Benefiziarierstation*[132]. That is not very likely, but a fortlet or watchtower in Qualburg is indeed a possibility, also because there was definitely a military site (no. 460) in Qualburg from the late-3rd century onwards. In addition, the distance between Harenatium (Rindern, 450) and Burginatium (Altkalkar) is large enough to expect intermediate posts.

The argument of distances may also indicate that one or more of the sites discussed above as possible *limes* forts, are actually smaller posts. It is, for example, difficult to accept that sites 117 as well as 126, 135 and 194, which are very close together (fig. 75), should all be full-sized auxiliary forts. The accumulation of possible forts on such a short stretch of the *limes* is, in fact, an additional argument for dropping the secondary finds from site 135 (Huissen) from further consideration, but it also indicates that sites 117 or 194 may eventually turn out to be an intermediate station of some sort.

It is not clear whether additional military sites are still to be expected along the Rhine. The sites discussed so far, the distances between them, and the flatness of the landscape may have been sufficient to allow a reliable communication system by fire or smoke signals and other means. The erosion by post-Roman Rhine channels, which has destroyed or negatively affected most of the evidence available today, indicates that if there were more military sites along the Rhine, the probability of discovery is rather low.

On the other hand, the geological situation also indicates that it could have been very profitable for the *limes* system as a whole, to have included outposts like watchtowers or signal stations at strategic spots on the high grounds north and east of the Rhine. Such posts have never been found, but the presence of military tile-stamps beyond the *limes* could be a significant indication for them. Although, for example, sites 1 and 7 do certainly not qualify as military settlements themselves and thus provide sufficient warning against a too hasty interpretation of tile-stamps as an indication for actual military buildings, the presence of military tile-stamps there is at least curious. The same is true for a number of stamped tiles from the Montferland, just east of the area covered by this study.[133] Unlike those from the river area, the presence of military tiles beyond the *limes* can be more readily interpreted as a direct result of activities by the army, just as the examples from sites along roads further to the south.[134]

The location of sites 1 and 7 along a major route to the north is an acceptable explanation for the presence of military artefacts, but it does not answer the question whether they came from a site south or north of the Rhine. In the absence of indisputable evidence for actual military stations north of the Rhine, the concept of a *rechtsrheinisches militärisches Nutzland* as used by Rüger has to suffice the moment, although his argumentation to reject outposts in that area is not at all convincing.[135] The explicit orders of Claudius to Corbulo, instructing him to withdraw the *praesidia cis Rhenum*,[136] very clearly

130 Spann 1972.

131 The well-known 'helmet from Millingen' (Klumbach 1974, 22, no. 8) was found during dredging operations in the Waal, close to the German border, and is not associated with site 441.

132 H.G. Horn, in Bogaers/Rüger 1974, 96.

133 Rüger 1968, sites 16, 25, and also 17 (Elten) which is in the area covered by the map (c. 209.0/430.9). The Roman finds from this site are, however, considered to have been brought there in early-Medieval times (cf. Binding a.o. 1970, 13–4).

134 We even know of Roman military tile-works, the *tegularia transrhenana*, north of the Rhine. Their location has not yet been discovered, but it is probably somewhere between Nijmegen and Xanten on the opposite side of the river. The Montferland is a promising area to look for such tile-works. The geological situation is comparable to the ice-pushed ridge on which the Holdeurn tile-works (site 433) are located.

135 Rüger 1968, 65. On military land, see also below, paragraph 11.4.2, p. 413–5. Possible instances of military outposts are also mentioned by, e.g., Schönberger 1969, 168–9.

136 Tacitus, *Ann.* XI, 19. Also in Cassius Dio, LX 30, 4, 6.

refer to camps in the context of a new offensive campaign. They do not necessarily imply that no military posts beyond the Rhine were allowed any longer, especially not if they were designed to function in the context of the early-warning and intercept system that the *limes* actually was.

The last category of military sites to be treated here, are the *stationes beneficiariorum consularis*. In a discussion based primarily on the distribution of military tile-stamps and a straightforward interpretation of them, Rüger felt justified in suggesting a whole series of these *stationes* in the river area. This is no longer possible without additional specific arguments for each site. These are hard to supply in the absence of excavations and also because it is difficult to identify a type of site which may not have recognizable characteristic attributes.

The only firm case for a *statio* in the river area can be made for site 500 (Cuijk). As already mentioned above, the location of Cuijk at the spot where an important road crossed the Meuse, is the ideal type-location for a *statio*. Moreover, the presence of an earlier and also a late-Roman fort which were there for the same purpose testify to the importance that was attached to this crossing.

Of the other claims made for sites as *stationes*, Kesteren, Nijmegen, Huissen, and Qualburg have already been discussed. In Nijmegen, the presence of a *statio*, for example on site 399 at one of the town's exits, is altogether possible. The claims for sites like Zetten, Doornenburg, Niel (437), Wijler-Hochstrasse, and Düffelward (see fig. 50) can be rejected out of hand, because they are based on stray finds usually in secondary position as well. Zyfflich (432), Viller Mühle,[137] and Heumensoord (391) are indeed possible locations for a *statio*. As is evident from the geological situation, site 432 would be a suitable location to control traffic on a possible direct route from Nijmegen to the *limes* road, but there are too many alternatives to give this consideration much weight.

The finds from Viller Mühle are, if reliable, a stronger argument. A *statio* there would control traffic along and possibly across the Niers. Rüger[138] mentions a *Niersübergang* but no motivation for it. If a *statio* controlled a route along the Niers, its position may have been determined by the border between the *civitates* of the Batavi and the Traianenses, as is suggested by the theoretical border indicated on figs. 3 and 135. The middle-Roman occupation at the site of the late-Roman *burgus* of Heumensoord is not well documented, but Bogaers' suggestion that it might have been a *statio*[139] is altogether plausible. The site is located half-way between Cuijk and Nijmegen, and undoubtedly at or very near the road.

Although the presence of additional road stations can never be excluded, there are only two other sites at which the presence of a *statio* is plausible. These are the *vici* of Elst (105) and Wijchen (315) discussed below. Elst is situated in the middle of the Betuwe at the intersection of north-south and east-west routes. As a local centre, it may well have included a *statio*. The same is true for Wijchen, where a road station could control both the traffic over the Pleistocene deposits and that along the northern branch of the Meuse.

Finally, and in view of what has been said so far about small military posts, it should be realised that there is still another possibility for some of the sites involved. As far as they are located south of the Rhine, it is conceivable that they are not military posts at all but stations of the *cursus publicus*, the official imperial courier- and transport-service. This centrally organized government-service[140] used stations, the *mansiones* (hostels) located at a day's travel from each other and the *mutationes* (staging-posts) at shorter distances.[141] *Mansiones* (or *tabernae, praetoria, deversoria*)[142] and *stationes beneficiariorum consularis* could be combined,[143] but even without such a combination the official character of the stations of the *cursus publicus* surely allows one to expect 'military' material there (stamped tiles, horse-gear, and the like) and evidently the choice of location must have resulted from virtually identical motives.

137 See chapter 6, notes 252 and 254. Viller Mühle is located in square 200/413 on Appendix 3. See fig. 75.

138 Rüger 1968, 68.

139 See Bogaers/Rüger 1974, 81.

140 See Stein 1932, 55–6 and Bender 1978, 15–6. For our area, we know, e.g., of a *praefectus vehiculorum per Belgicam et duas Germanias* (*CIL* VIII, 12020 = *ER* II, 578): the director, therefore, of a (late-2nd – early-3rd century) district encompassing several provinces.

141 Cf. Bender 1978, 8.

142 The concepts of *mansio* and *mutatio*, while commonly used in modern literature, are in fact late-Roman designations. During the Principate, other concepts were used, such as *praetorium, deversorium, taberna, stabulum*. See Bender 1975, 19–20.

143 Cf. Von Petrikovits 1978, 119–20. They were not, however, located in the same building (Bender 1975, 20).

8.4.2 Civil Settlements

As mentioned in the introduction, the known middle-Roman settlements probably represent at least 50% of the number originally present. For the river area proper, this figure is certainly much higher. Should there be no different forms of settlement which are specific to the Pleistocene deposits – which is hardly likely in view of the settlement types reported from other regions – a complete spectrum should be present in the available sites.

The analysis of civil settlements is not as straightforward as that of the military sites. Although they are also part of a larger superstructure and Roman concepts may be applicable to designate certain forms of settlement, there are also sites and structures which are specific to the region and not primarily related to the Roman occupation or a direct result of it. It is not advisable, therefore, to present a framework of Roman concepts and try to fit all settlements into that scheme. On the other hand, a description completely in neutral terms is unnecessary, both because of the foreknowledge on specific sites and of the obvious applicability of Roman concepts in several cases.

The clearest example of this situation is, of course, Ulpia Noviomagus Batavorum. In analytical terms, it should be called a regional centre, in funtional terms a *civitas*-capital, in Roman juridical terms (probably since IIB) a *municipium*, and in a current technical/juridical terminology for urban settlements a city.[144] Depending on one's point of view, there are still more terms which could be applied, but the key term is regional centre.

By implication this indicates some degree of hierarchical organization of the settlements. The nature of that organization is not the subject of this chapter, but the fact that the regional centre has been identified as a *civitas*-capital is a source of valuable additional information about the position of the settlement in relation to others within and outside the region, and also about some of its characteristics which have not yet been or cannot be traced by archaeological means.

Similar opportunities are available for other types of settlements, although they rapidly diminish when the primary archaeological data are inadequate. This problem is especially relevant when forms of settlement are discussed of which no example has been sufficiently investigated in the river area and the frame of reference has to be derived from sources outside the region. At a lower level, the attribution of specific sites to a certain type is complicated because the problem of inadequate or insufficient data is endemic as a result of the nature of the data, which are mostly surface-finds. It becomes even more complicated when finer distinctions are sought, in order to achieve an increasingly sophisticated typology. This is, in the end, the mechanism which limits the level of diversification that can be reached without resorting entirely to talking in terms of probabilities or possibilities of which more than enough are involved when specific sites are concerned.

Regional centre

In paragraph 8.2.1 it was concluded that the observable differences between pre-Roman settlements were only marginal and that, at least in the area studied, a single most important settlement which could be a socio-economic and political centre was lacking. Even if, towards the end of the Iron Age, a tribal centre did exist elsewhere, near Rossum/Lith, this implies that the Batavian capital during the Early-Roman Period, identified as site 403, was a completely new phenomenon and therefore directly related to the Roman conquest. What is known about site 403 is in agreement with this, because an identifiable native component is definitely lacking at this settlement. It presumably started as a military camp and its function as a tribal capital can only have been the deliberate result of Roman policy.

Such a creation of a regional centre is precisely what could be expected in a situation where no, or no conveniently located, pre-Roman centre existed, and comparable developments are known from elsewhere.[145] Whether or not one is prepared to consider Batavodurum as a *civitas*-capital, which depends on the period for which one is prepared to accept the existence of a Batavian *civitas*,[146] is not really relevant in this respect. Batavodurum was the *caput* of at least the Batavian tribal area during the first decades of the Middle-Roman Period (and for some time before).

It is virtually impossible to say anything with certainty about the function of the regional centre at site 403. By definition, it must have been the formal administrative and political centre although not necessarily the resi-

144 E.g., Wacher 1978, 65–6, for the proposition to call a chartered town (*colonia, municipium*) a city. See also the terminological analysis by Crickmore 1984, 16–9.

145 E.g., Webster 1966 and Von Petrikovits 1978, 115–9.

146 For different views on this subject, see Bogaers 1960–61, Rüger 1968, 93 f., Bloemers 1978a, 80–4, Van Es 1981, 215–20.

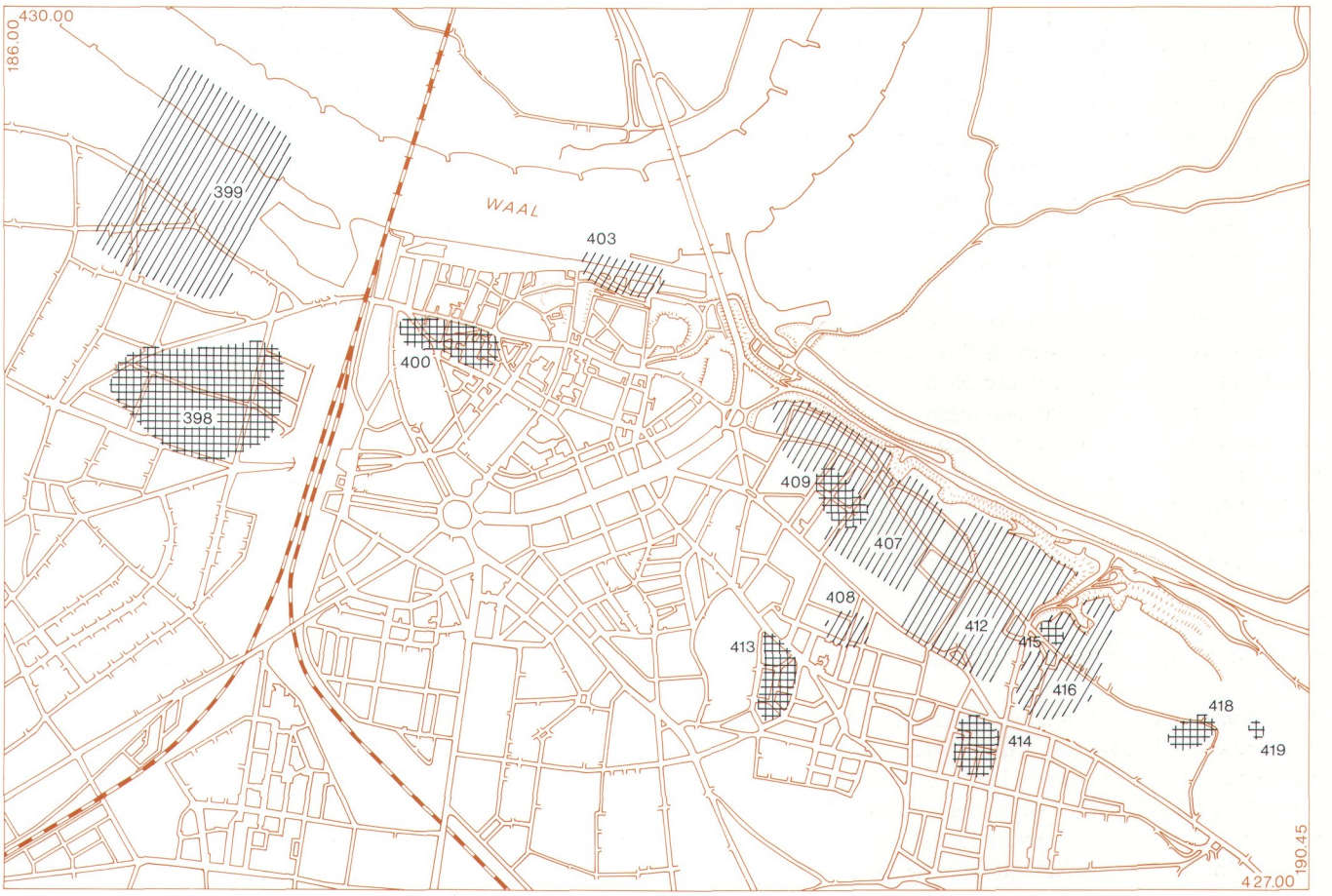


Fig. 76 Nijmegen in the Middle-Roman Period (AD 50–270). For site nos., see chapter 5 (catalogue). Hatched: settlement, cross-hatched: cemetery. Scale 1:25,000. For details of Nijmegen-east from AD 70-early-2nd century, see fig. 77.

dence of Batavian chieftains like Flavus, son of Vihirmas, who (presumably in IA) dedicated an altar to Magusanus Hercules, on which he is mentioned as *summus magistratus*, supreme magistrate, of the *civitas* Batavorum.[147] The military characteristics of site 403 have already been discussed, but they could also point to the presence of a military *praefectus civitatis*. In pre-Flavian times the *praefecti civitatis* were military commissioners who had to establish tribal self-government and train the chiefs in its methods in regions where adequate tribal organization was apparently lacking.[148] Although the only known functionary of this kind in Germania Inferior is Olennius, who was *praefectus Frisiorum* in AD 28, the existence of similar officials south of the Rhine cannot be excluded.[149]

The concentration of the Roman administrative apparatus in Nijmegen makes it also likely that site 403 functioned as an economic centre, even though it may not have been situated at a very suitable location for that purpose. As already mentioned above, the locations of all early-Roman sites in Nijmegen must have been chosen with a military objective in mind. In fact, the shift of the regional centre from site 403 to 399, which took place

147 See chapter 2, note 21 and below, chapter 11, fig. 127 and p. 398.

148 See Bogaers 1960–61, 268 and note 21; Rüger 1968, 28–31; Frere 1978, 235.

149 Olennius: Tacitus, *Ann.* IV, 72. See comments by Rüger 1968, 28–9 and Bloemers 1978a, 82.



Fig. 77 Nijmegen: sites from AD 70–early-2nd century. For site nos., see chapter 5 (catalogue): 1 contour lines, 2 railway, 3 excavated area, 4 topographical coordinates, 5 buildings, 6 ditch, 7 inhabited area; investigated and/or many finds, 8 inhabited area; not investigated and/or few finds, 9 ditch, certain and hypothetical trajectory, 10 cemetery; investigated and/or many finds, 11 cemetery; not investigated and/or few finds, 12 road. Scale 1:10,000.

after the events of AD 69-70, may be just as much or even more a result of the unsuitable location of 403 for a regional centre, than of its suitability for military purposes, which is usually considered as a motive.[150] In any case, most of the area occupied by the old centre was no

150 E.g., Bogaers 1979b, 57: *...; mann kann sich vorstellen, dass die Römer auf der Stauchmoräne, in unmittelbarer Nähe des Lagers, nun keine ausgedehnte Zivilsiedlung mehr geduldet haben.*



longer settled after AD 70. What remained of it (compare figs. 66 and 77) was a small settlement along the Waal, at first probably in connection with – if not as a part of – the *canabae legionis* of the new fortress at site 412.[151] See figs. 76 and 77.

151 Cf. Bogaers 1960–61, 276.
152 See chapter 11, note 332, and also below, note 182.
153 See Bogaers 1979b, *Abb.* 63.

In view of its alleged importance, site 399 (fig. 76) has undergone surprisingly little in the way of archaeological investigation. A no longer entirely up-to-date overview of available data, current interpretation, and relevant literature is provided in the *Noviomagus* volume. Very recent excavations have provided new evidence in support of the interpretation given there.[152]
From the plan of the available traces of buildings,[153] it appears as if the settlement did not grow naturally into a

town but was a planned development with a regular street-grid and therefore the deliberate foundation of a new administrative and economic centre. The latter function is expressed most clearly in its name, Noviomagus (Newmarket), which replaced the old name Batavodurum. It is generally assumed that site 399 retained the old name until it was granted the *ius nundinarum* by the emperor Traianus, which is an explanation for the honorary title *Ulpia*. Too little is known about site 399 to decide whether this grant was the official recognition of an established practice, which would seem to be a logical assumption, or the actual beginning of a new function related to the departure of the 10th legion, as proposed by Bogaers.[154] In any case, the departure of the legion must indeed have caused a shift in diverting to the town those activities around the fortress that did not disappear with the legion. This assumption has been confirmed by the excavations of the *canabae*, which indicate a decline after IIa when the fortress was occupied by a detachment of the 30th Legion.

As far as a settlement typology is concerned, the fact that Noviomagus later also received a charter and became Municipium Batavorum is not particularly relevant. This took place in IIB or even later[155] and had only formal, juridical consequences. If it happened after AD 212, when Caracalla issued his edict conferring Roman citizenship to all free subjects of the empire, the real significance must have been virtually nil. The old name (Ulpia) Noviomagus continued to be used after this event and, in fact, survives in the present-day Nijmegen. The more extensive designation Ulpia Noviomagus Batavorum is used on Appendix 3 to avoid multiple names, but it has never been attested epigraphically.

Because of the restricted amount of work done on site 399, there are only a few archaeological indications for the structure and function of the town and its institutions. These are supplemented by some scattered epigraphical evidence, but most of its characteristics have to be derived by implication from what is known about other *civitas*-capitals. As already mentioned, the excavated traces of buildings indicate that the town was a planned development, presumably with a regular street grid. The estimated size is c. 40 ha, but it is quite uncertain if this entire area was completely built over. In this respect it is worth noting that there are kiln-sites within it, such as those under the Maasplein and under the former starch factory along the Waal.[156] These kilns are incompatible with a dense occupation in the immediate vicinity and should be outside the town, unless one is prepared to consider the possibility of potters' workshops on (still) empty *insulae*. The Maasplein kilns were in operation during the early 2nd century, so it is conceivable that the town either grew to its full size somewhat later, or that the area within the original perimeter only gradually or never filled up.

As far as this perimeter is concerned, there were until recently no other data than the distribution of finds. Recent findings mentioned above show that the town did indeed have peripheral defences like the walls around the nearby Colonia Ulpia Traiana or, as a later addition, Atuatuca Tungrorum and Forum Hadriani/Municipium A. Cananefat(i)um. Especially in view of these last two towns, it was to be expected that Noviomagus was also walled at some point in its history. The only clearly interpretable buildings inside the town are two adjacent Gallo-Roman temples: an indication for the religious function of the town as a central place. Remains of another structure, partially excavated in 1834, could point to another temple, a *thermen*, or to a so-called *cryptoporticus* in relation to a *forum*.[157]

Whatever the true nature of these foundations may be, both a *thermen* and a *forum* were necessary elements of any Roman town and they should be present in Noviomagus as well. For the presence of a *forum* with a *curia*, a town-hall, there is also indirect evidence in the dedications of altars by three or four *decuriones*[158] of the city-council, the *ordo decurionum*. Such an *ordo* must, by the way, also have been present when Noviomagus was not yet a *municipium* and therefore juridically a *vicus*. *Vici* could not have an *ordo* of their own,[159] but the council of

154 Bogaers 1960–61, 290–1.

155 Bogaers 1960–61, 309–10. The event took place under Marcus Aurelius or Septimius Severus, possibly even as late as Caracalla's reign, but in any case before AD 227 (Bogaers 1972b, 9).

156 Maasplein: Daniels 1927, 90–2; starch factory: Daniëls 1955, 202–3. This kiln may have been used for the production of lamps.

157 The excavators originally interpreted the structure as a *thermen* (Brunsting 1949, 55), which is not at all improbable (Bogaers 1979b, 61), but a temple (Brunsting 1949, 57) or a *cryptoporticus* (Von Petrikovits 1955, 11) remain possible.

158 Bogaers 1960–61, 287–9 and note 146; Bogaers 1972b. See also chapter 11, note 336.

159 Rupprecht 1975, 44–5; Vittinghoff 1976, 89 and note 82.

decurions which governed the *civitas* had its seat in its *caput*, which could well be a *vicus*.

In addition to the designation 'Ulpian Newmarket' and the fact that at least two of the decurions who dedicated an altar to the goddess Nehalennia were traders, there are also other inscriptions pointing to trade and industry on and around site 399. These include a Nervian *negotiator frumentarius* (see fig. 136; *CIL* XIII, 8725 = *ER* II, 191) working in Nijmegen and evidence for a local *collegium fabrum tignariorum* (*ER* II, 203; not in *CIL*), a guild of carpenters (or artificers). Apart from the kilns mentioned above, the inscriptions are the only direct evidence for Noviomagus as supply and trading centre but they do indicate the presence of a market-place and the shops or workshops whose actual remains still await excavation. Likewise, the presence of a harbour need not be doubted, although it was presumably eroded by the river Waal when the northern part of site 399 was washed away. A second possible location for a harbour is site 403 which must have stayed in military hands after AD 70 as part of the *canabae legionis*. Although it may also have served the town, especially from the end of the 2nd century onwards, it is, as will be explained below, more likely that it always remained primarily military.

Although in this case the inheritance by the town of a part of the military infrastructure is only a possibility, there is at least one instance where a similar process can be demonstrated, namely, the amphitheatre at site 408. The amphitheatre was part of the *canabae legionis* belonging to the fortress (412), but in contrast to the rest of the *canabae*, the finds clearly indicate that this structure continued to be used even into the 3rd century.[160] This implies that it was probably used for the entertainment of the people from the town and the region.

Apart from indicating yet another function of the regional centre, this probably means that site 399 never had its own amphitheatre. Because no decent Roman town could normally do without one, this suggests at least the possibility that there may have been other facilities lacking at site 399 as well. This is understandable for the period when there was still a sizable number of soldiers present but some amenities of town life may never have been available.

160 Bloemers 1979d, 56.

161 Von Petrikovits 1978, 118–9. For an overview of relevant literature and opinions, see also Crickmore 1984, chapter 2.

162 The latter only if one is prepared to accept the propositions regarding dependent *vici* under military control and on military land. See below, p. 268 and note 169.

Secondary centres

There are a number of places in the region which surpass the level of the various sorts of small settlements which constitute the bulk of the sites presented in the catalogue. The difference between these settlements and the others is not only expressed in their size, but also in the presence of structures which are absent at other settlements and which imply a central or special function. For this reason, they have all been termed secondary centres, although there may be considerable differences between some of the sites included in this category. The best way to explore these differences more fully, and the sorts of sites involved, is to discuss first the term by which they are all indicated, namely the Roman concept of *vicus*.

Literally, *vicus* simply means 'dwelling-place'. In practice, the term is used for all kinds of settlement, from non-urban villages generally not exceeding 20 ha to small towns, covering up to 60 ha.[161] The reason for this rather wide use of the term is of an administrative and juridical nature. The Roman administrative organization was based on the local self-governing unit (*res publica*), and (in this part of the empire) a *vicus* could either be part of the territory of *civitates peregrinae*, of a chartered town (*municipium* or *colonia*), or of the army.[162] It was not, therefore, an independent entity on its own, even when it was an urban centre and *civitas*-capital such as Ulpia Noviomagus. *Civitas*-capitals without a charter are, although *de facto* a town, juridically not different from other, smaller *vici*.

The legal position of the *civitates* and their capitals, which also gradually changed during the Roman Period, has been hotly debated during recent decades,[163] but it is not the subject of the present discussion. A *civitas*-capital and *de facto* town can be described in the same way as a city, namely, as regional centre or central place, thereby avoiding all the intricacies connected with the legal position. It also sets it apart from the other *vici*, which did not have such a function.

For these other *vici*, the dependency of their status is obvious from the different meanings in which the word is used. A *vicus* can be a separate settlement site, functioning as a subordinate local centre for the civilian popula-

163 See e.g. Reynolds 1966, Rivet 1966, Bogaers 1960–61 and 1967a, Rüger 1968, Vittinghoff 1976 and the literature cited in these publications.

tion. It can also be a ward of a town[164] or city, and thus not even be a separate settlement. In a military context, *vicus* is used to describe the settlements attached to auxiliary forts or, for the large settlements (*canabae legionis*) near legionary fortresses, the constituent parts or wards of them.

From the different meanings of the word *vicus*, it appears that there are two ways in which the concept of *vicus* can be divided: it is either a ward or a separate settlement and it is either civilian or attached to a military site. The first distinction is relevant, although for a settlement-typology only those *vici* are considered which are separate and archaeologically observable entities. This excludes the possible wards of site 399, but not the spatially segregated *vici* of the *canabae legonis* around site 412 which can – to some degree – be treated individually.

A more fundamental distinction may be that between civil and military *vici*. At a structural level, both share many similarities. They may include several sorts of public buildings, such as a temple, an inn, and a bathhouse, but also more humble public facilities such as shops and workshops. The houses are usually of the narrow rectangular strip-house type. At a functional level there may, however, be a difference. The military *vici* could undoubtedly function as a centre for the surrounding area, but their main purpose was to provide services for the soldiers. Regardless of the impetus for their origin, the civilian *vici* did not usually have such a limited purpose.

There are several different causes for the origin of a *vicus*[165] but the presence of one such cause, for example a junction of roads, did not necessarily lead to a *vicus* there. *Vici* can thus be seen as developments which were succesful not just because of one favourable circumstance, but because other important factors in the area, such as the presence of and distance from other *vici* or towns, size of the population, political, religious, ethnic, and a whole array of other circumstances, provided a stimulus. Therefore, civilian *vici* can generally be regarded as local secondary centres, tied into the regional network and providing a variety of services for the surrounding countryside, while military *vici* often seen be more single-purpose, special centres.

The use of the term civilian v. military *vicus* may, h ever, not be completely adequate. On the one hand, possible that some civilian *vici* did not function as ondary centres but had a rather limited, for example dustrial or religious function. On the other, military may also have serviced a civilian hinterland, thus being real secondary centres, as may have been the in northern Britain.[166] In effect, many or most of tl continued to function as such in other areas when frontier moved on and a military camp was no longe quired.

This last consideration, forwarded most strongly Frere,[167] is one of the reasons why it is very difficu determine on the basis of their location when, to v degree, and for what aspects of life *vici* serviced rounding areas. The spacing of *vici* is influenced by n tary considerations, although perhaps less strongly t in Britain. But apart from this argument the usual r random spacing of *vici*, whether 'artificially created 'naturally grown', is in itself already an indication f function as secondary centres, as long as there are convincing arguments to the contrary.

The alleged juridical and administrative status of n tary *vici* is at least one argument that is not convincin this respect. Although traditional views hold that settlements at the gates of forts were completely ur military jurisdiction and control,[168] this hypothesis been seriously challenged. For several reasons,[169] the called military *vici* were probably fairly independent in fact normally under the nearest civil authority, w the larger *canabae* were at least partially independ This does not exclude military influence, but it also c not set these settlements apart from the administra and economic organization of the region.

On the basis of the foregoing arguments, it migh concluded that different kinds of *vici* should not be tinguished because they fulfilled a similar func under the same authority, namely, the *civitas*. Altho they were undoubtedly all local (secondary) cen

164 Because of these different meanings of the term a *vicus* in the sense of a ward can even be part of a *vicus* in the sense of a town (cf. Bogaers 1967a, 232).

165 Von Petrikovits 1978, 119–23.

166 Salway 1965, 1980. A different opinion is voiced by Sommer (1984, 51–2), who suggests that the military *vici* primarily serviced the forts with only a limited function for the surrounding native population.

167 Frere 1975. See also the comment of I. Hodder in same volume. On the military origins of non-military *vici* also Webster 1966 and Vittinghoff 1970.

168 See Rüger 1968, 51 ff. and the literature cited therein

169 See Vittinghoff 1970, 343 ff.; 1974, 111 ff., Salway 1 13 ff., and Sommer 1984, 22–3, 26, and 29 for a discussic the arguments. A similar view on the status of settlements forts is held by Von Petrikovits 1979, 242.

some, and most notably the military *vici*, however, were also special centres. The difference between the two does, as will be shown below, indeed seem to conform to the division between 'civilian' and 'military' in the eastern river area.

When the sites in the region are considered, it turns out that the number of possible *vici* is fairly limited, which is to be expected in view of the size of the area studied. As far as the forts along the Rhine are concerned, only one of the sites discussed in paragraph 8.4.1 can be shown to have an adjacent settlement worthy of the name *vicus*. In view of the present state of the evidence, which in general is rather meagre, that is hardly surprising. Further research will surely produce enough data for a reasonable evaluation of the 'military' *vici* even though erosion may have destroyed part of the sites. At this moment, only a brief discussion for all the proposed forts can be provided.

The best information is that available for Kesteren (37), although the actual fort is still missing and probably eroded as mentioned earlier (p. 250). An adjacent *vicus* might thus have been eroded as well, but there is increasing evidence that it can be identified as site 38, where a considerable quantity of military material has been recovered. The excavations in 1968 and 1977 have not produced clear evidence for or against this interpretation, although it was supported by the military finds and the pottery assemblage (see paragraph 8.4.3). New excavations in 1984, however, produced additional evidence in the form of firm indications for strip-houses, more military finds, and even V-shaped 'military' ditches surrounding the site.[170] The acceptance of the now almost inescapable conclusion that site 38 is a military *vicus* produces two further implications. First, it is additional evidence for the assumed presence and location of site 37. Second, the military *vicus* in this case is hardly likely to have functioned as a local centre because it is fairly small, c. 3 ha, and indications for public buildings, or even buildings in stone,[171] are absent.

For a settlement near site 18b in Randwijk there is, of course, no information at all.

As mentioned before, site 117/8 in Driel is exceptionally large (c. 8.5 ha) and may be the site of a fort together with a *vicus*, which has been tentatively localized at site 118. In any case, even if one is not prepared to accept the presence of a fort, it is clear that here we have at least a sizeable settlement which is likely to be a *vicus*. Because of the lack of specific data, the structure and purpose of the occupation remain unknown.

For civilian occupation near the fort in Meinerswijk (126), there is some evidence on the other side of the present-day Rhine, namely, at site 23. The Roman Rhine was probably located immediately at the foot of the ice-pushed ridge under Oosterbeek (north of site 23) and must have eroded all or most of site 23 in post-Roman times. Dredging operations in 1953 yielded a number of artefacts, mostly sherds, which is all there is to indicate the site of a possible *vicus* (see fig. 93). The presence of tuff indicates that it may have included stone buildings.

The data from Huissen (135) indicate nothing specifically related to a *vicus*. Nor, at first sight, do those from site 194 in the Loowaard, but the quantitative analysis of the finds in paragraph 8.4.3 indicates that material from a temple might be involved, and that could indicate a *vicus*. For the moment this is, however, nothing more than an interesting speculation: part of the finds may also originate from a cemetery or be explained in another way. Data on *vici* near the forts in De Bijland (182) and Rindern (450) are completely lacking. Only the middle-Roman settlement in Qualburg (461), which may be related to some sort of small military station, could possibly be considered a *vicus*. The very limited excavations by Von Petrikovits in 1937 do not provide any real arguments for further discussion.

The evidence from excavations elsewhere along the *limes* shows that civil occupation in relation to forts may vary from a few insignificant structures to large settlements with strip-houses, public buildings, workshops, etc. For the eastern river area, the data are too scanty and deficient to reach any definite conclusions. There are, however, several reasons to suspect that most, if not all, of the settlements involved were closer to the lower than to the upper end of the scale and that they functioned more as special centres to provide services for the army than as local centres for the native population.

First, there are too many military sites to suppose that they were all large auxiliary forts. As mentioned above, some may have been no more than fortlets and the smaller the number of soldiers, the smaller the '*vicus*' is likely to be. The number of military *vici* would also be

170 *JROB* 1984, chapter II B, sub 2c.

171 Cf.note 170.

far too large in relation to the size of the entire area and the number of settlements involved, and their linear configuration along the Rhine is not very favourable for a function as local centre. Second, diverse evidence from elsewhere suggests a strong orientation towards the army.[172] Third, there are other settlements, including the regional centre itself, which are close enough and in a much better geographical position to provide the necessary services to the native population.

For the region between Rhine and Waal, the Overbetuwe, it is quite clear that Elst (105) must have been the local centre, thereby removing the need to promote most of the presumed military *vici* to that status. Site 105 may have been as large as 15 ha and, apart from the well-known temple (see fig. 129), it included several other stone buildings of which only parts have been recorded so far. It is located in the geographical centre of the Overbetuwe and can be easily reached from all directions. It must have been a cult-site for the entire region, maybe even for the *civitas*,[173] but there is no reason to assume that this was its only function. The construction of the temple around AD 50 may have contributed to the growing importance of Elst, but it can also be seen as confirmation of its 'natural' status as a local centre for the Overbetuwe. Because of its strategic location, Elst is also a plausible location for a *statio*, which would control most of the traffic through the Overbetuwe.

In the region between Waal and Meuse, there is also only one site which can be interpreted as a *vicus* and local centre, namely, Wychen (315). No part of this settlement has ever been excavated, but amateur archaeologists have recovered remarkable amounts of material from several closely associated findspots. The settlement may have covered about 9 ha, and on several findspots clear evidence for stone buildings was found.[174] The settlement is conveniently located on the Pleistocene deposits, next to a branch of the Meuse. It does not appear to have a long pre-Roman history itself, but it is located in the area where the more important pre- and early-Roman settlements have been tentatively localized. It may, therefore, represent a continuation of some sort of traditional centre.

In any case, it is favourably situated in relation to traffic routes, which was a reason to consider it, just like Elst, as a possible location for a *statio*. Because of the lack excavations, nothing can be said with certainty ab public buildings. The ample evidence for building stone does not constitute definite proof of their existe either, although it may certainly be taken as a first in cation.

A final remarkable feature of the *vicus* at site 315 is t on all findspots quite considerable amounts of mate from the 4th and 5th centuries, and also from the M rovingian Period, were found. Although presumably unchanged, Wijchen must therefore have remaine centre of some importance after the Middle-Roman I riod, in contrast to Elst.

The third site which can be interpreted as a *vicus* is Cu (500), undoubtedly the local centre for the area south the Meuse. Just like Elst and Wijchen, it is situated at ideal spot in relation to traffic routes. In this case strategic importance of the site is very evident. Exca tions have revealed the presence of a fort (499) in the century and again in the Late-Roman Period. For period in between, the presence of a *statio* is very lik indeed.

The *vicus* of the 2nd and 3rd centuries included the s of the earlier fort, but it also extended over a wider ar Its size is difficult to determine, but it probably did exceed 10 ha.[175] Excavations have revealed the prese of at least six stone buildings, including two Gallo-R man temples and wooden striphouses, presumal shops. Even though the preceding fort must have p vided a regular pattern, the recovered traces of buildi do not indicate a regular street grid. The fortification Cuijk in the Late-Roman Period and the evidence the early- as well as later-medieval occupation at the s show that, like Wijchen, it must have continued to b local centre of some importance.[176]

The only area that lacks a *vicus* as local centre, is the a to the southeast of Nijmegen. It is possible that Rinde (450) or Qualburg (461) functioned as such. At least theory Qualburg would be in a favourable situation a secondary centre, being located on the major route (t *limes* road) between Nijmegen and Xanten and, ev more important, exactly midway between the two gional centres. It is located precisely on the theoreti boundary between the *civitates* of the Batavi and t

172 Sommer 1984, 30–52.

173 As proposed by Bogaers 1955, 186 and 191 ff.

174 On parts of the Tienakker and a findspot along the Oude Ravensteinseweg.

175 The area is largely built over. The minimum size of *vicus* probably was 6 ha (± 200 × 300 m). For plans, see B gaers 1967, *Afb.* 8 and Koeling/Koolen 1978.

176 See below, p. 308–9 and 313.

Traianenses (cf. figs. 3 and 4). It should, however, be repeated that the archaeological evidence is too scanty to test this proposition. In addition, even though the known distribution of sites is certainly deficient, the numbers of sites in the area and especially on the Pleistocene deposits may have been far fewer than in the river area. Thus, there may have been no need for a separate local centre.

The archaeological evidence indicates that each of the geographically separate regions of the Maaskant, Land van Maas en Waal, and Overbetuwe had a small *vicus* as its own centre. These secondary centres are situated at geographically very favourable locations in a semicircle around the primary centre. As far as the military situation is concerned, the location of each of them can also be interpreted as strategically important for communication and control purposes. Even though the military aspects of Elst and Wijchen are less obvious than those of Cuijk, they are no less certainly present, for example, in the military involvement in the construction of the temples in Elst and they may, in the final analysis, well prove to be the initial reason for the growth of these settlements into *vici*.

Their main function can, however, not have been a military one. Elst and Cuijk were definitely religious centres and at least for Cuijk there is evidence that it was a centre for economic activities also. Presumably all three *vici* were religious and economic centres, and also the sites of a *statio*. They may even have been administrative centres for their respective regions. Elsewhere[177] it has been argued that the *civitas Batavorum* can be divided into five geographically separate entities, namely the Overbetuwe, the Land van Maas en Waal, the western river area (Nederbetuwe, Bommelerwaard), the (largely) Holocene deposits south of the Meuse (Maaskant, Land van Cuijk), and the sandy soils of Brabant (Meierij van 's Hertogenbosch). Each of these areas had a *vicus* as its centre. For the western river area the local centre could be Rossum, for Brabant it is obviously Halder near St. Michielsgestel. This matter will be returned to in chapter 11, but at this point it is important to note that these regions may well be identified as *pagi*, the administrative units into which a *civitas* could be subdivided.[178] The fact that they each have a 'capital' of their own could support both the notion of *pagi* and the interpretation of these *vici* as subordinate administrative centres. All in all, the evidence for 'civilian' *vici* is – although limited – much better than for 'military' *vici* related to frontier forts.

The only military *vici* for which there is considerable evidence are the *vici* which are the constituent parts of the *canabae legionis* around the Nijmegen fortress. These are sites 403, 407, 409, and 416, and in a way also site 433. The present knowledge about sites 407, 409, and 416, which is the result of the ROB excavations since 1972, has been summarized by Bloemers and need not be repeated here.[179] It is remarkable that no houses have as yet been found. The buildings of site 407 seem to have a public, possibly religious nature: they may be related to the cemetery and burial monuments around them. Indications for industrial activities (iron slags) are, however, present as well. On site 409, the amphitheatre is also a public facility and the eastern ward, site 416, was clearly a centre for trade and industry. The large tile-works at De Holdeurn (433), some 4 kilometres to the east, were undoubtedly a military operation, although that does not exclude the presence of civilians nor the production of tiles for non-military purposes. Site 433 is a separate, industrial settlement, but it can also be seen as an outlying *vicus* of the *canabae*. It was in any case directly related to the fortress during the Flavian period and the early-2nd century. Later, it may have been a more independent settlement. After AD 175, the tile-works produced brick for the army of Lower Germany (*exercitus Germanicus inferior*).

As far as the interpretation of sites 407, 409, 416, and 433 is concerned, there are no problems because they were clearly special centres, primarily (but not exclusively) servicing the army. After the departure of all or most of the soldiers activities continued only at the tile-works and the amphitheatre. Although there are still substantial areas which have not been investigated, one has the impression that not too many people actually lived in the *canabae* directly around the fortress.

There is, however, one *vicus* which has not been mentioned so far, namely, the so-called 'commercial ward' along the Waal, at the site of the former Batavodurum. If our interpretation of Batavodurum is correct, that it was essentially a military site with a developing civil settlement around a military core, then it could be a logical

177 Willems 1983a. See also chapter 11, 421–2.

178 On *pagi* and their centres, see also Crickmore 1984, 13, 24, 45f.

179 Bloemers 1979d. See also fig. 77.

development that part of the site continued to be inhabited by dependants of the soldiers after AD 70. Both the character of the sites directly around the fortress and the more favourable location of site 403 indicate that most of the *canabenses* probably lived along the Waal.
Although there have been some small excavations on this site, until recently only a limited number of occupation traces had been found.[180] Part of the settlement must have been eroded by the Waal and the remaining portion is severely disturbed by late- and especially post-Roman occupation. It is noteworthy that the dating evidence indicates that most of the middle-Roman features belong to the Flavian period and the first half of the 2nd century.[181] Although the site was continuously occupied it seems, therefore, that most of the activities ended around the same time as on sites 407 and 409, which is another argument to consider site 403 as a part of the *canabae legionis*, most likely a residential ward around a harbour and thus also a special centre. The continuing occupation from the end of the 2nd century onwards was presumably also related to military affairs. Unless the town had no harbour of its own, which is not very likely, the only plausible function for site 403 would seem to be that it served to ship the tiles produced at the Holdeurn.[182]

Villae

As is evident from a comparison of Appendices 1-5, rural settlements of the Middle-Roman Period are more diverse than in any other period. As a group, they differ from the villages described as *vici* because they are smaller and do not have public buildings or shops. Their location is determined primarily by the habitability and exploitability of the land and most of the excavated structures are clearly farms or otherwise related to agricultural activities. Nevertheless, they are not all the same and there are substantial differences between sites, not only between excavated settlements but also between collections of surface-material.
In virtually every study on rural settlement in the Roman Period, a basic distinction is made between two major types of settlements: the villa, which is an adaptation of a Roman form of settlement,[183] and the native village (or hamlet, or single farmstead), which is primarily a continuation of the traditional way of life, although it need not be unchanged. There is no reason to assume that the situation in the eastern river area was completely different from this, even though it is a frontier area where conditions may not have been the same as further to the south.
In order to be able to identify settlements as villae, it is necessary to review briefly what the concept of villa entails and, secondly, by which archaeological phenomena it can be recognized.
Exactly what constitutes a Roman villa is a subject that will probably be debated forever, but there are several basic aspects that are generally agreed upon. The first of these is that a villa is an exclusively rural type of settlement, but a second and related property of a villa is its dependence on towns. In his well-known article on their social and economic aspects, Rivet[184] stressed the fact that villae were intimately associated with towns, which provided a market for their surplus production, another basic aspect of villae. They were owned by wealthy individuals, according to Rivet in principle by townsmen, and represented a large capital investment which had to bring some sort of profit. The estates, the villae and the land that belonged to them, were therefore themselves also property that could be bought and sold.
It is obvious that the application of the concept of villa to settlements in the river area will, by implication, have far-reaching consequences because of its presuppositions on the social and economic systems involved. If there are villae, then there has to be some sort of town-based cash-market system and there have to be people who have both the means and the desire to participate in such a system. In this chapter, we are not concerned with questions like that, but rather with an analysis of observable differentiation which may contribute to answer those questions. Nevertheless, villae are such typical means of exploiting the land that the question of their absence or presence in the river area cannot be postponed.
Fortunately, it is not too complicated to demonstrate

180 Van Tent 1973, 131–4. Van Tent's interpretation of Roman terrace-wall(s) is not correct. Recent excavations are discussed in *JROB* 1983, 47–8 and 1984 (in press).
181 Van Tent 1973, 132.
182 Van Tent's suggestion (1973, 134) that site 403 rather than 399 should be identified as Ulpia Noviomagus is very unlikely indeed (cf. Bogaers 1979b, 58–9). It is contradicted by the available data on the features and size of both sites and also by other information, such as the location of the large cemetery (398) south of site 399.
183 Hence the use of terms like Gallo–Roman or Romano-British.
184 Rivet 1969.

that, in principle, they may have been present. First of all, the continuous presence of troops must undoubtedly have led to an economy that was at least partially cash-based. In addition, the area had a town which, as Ulpian Newmarket, testifies to the presence of a regional market centre already by its name, and there were also local centres in each of the geographically separated areas. Without going into too much detail, and without having to suggest that the river area had a full-scale market economy, we can therefore assume that a basis existed whereby villae could function.

As far as people are concerned, the group of potential villa owners is restricted to those with adequate financial resources. Although members of a wealthy native aristocracy are the first to come to mind in this respect, there are other groups which could also qualify. These are retired soldiers who could be awarded land (and had saved enough money during their time of service) and also rich businessmen from towns. There may also be some overlap between these groups, for example, in the case of army officers who were members of the aristocracy. Direct (epigraphical) evidence on the ownership of villae is difficult to obtain and is sometimes ambiguous,[185] but at this point it is sufficient to note that there is no reason to exclude any of the three sorts of potential villa owners for the river area.[186] There is also no reason to assume that villae were absent for other reasons. All three groups are likely to have been strongly influenced by Roman culture – most of them may even have been Roman citizens – and therefore not averse to owning a villa. As long as the security of the area was not questioned, the investment in a villa could be a profitable enterprise, although it may not even be necessary to presuppose such capitalistic motives for all groups of owners. Depending on the degree of acculturation, it is also conceivable that the profit of the investment was more social than economical, especially in the cases of native owners such as veterans who had returned home and of members of leading families.

The main reason why a villa could be a status symbol as well as a source of income is the fact that it differed from a normal settlement. That is the second issue which has to be dealt with here: what are the attributes by which a site can be identified as a villa?

A villa cannot really be defined, but it can at least be described by some of its most important aspects. It can thus be said that it is, in principle, an agricultural settlement producing a marketable surplus for its owner's profit. The owner may live at the villa and direct it, have it administered by a *vilicus* (bailiff), or lease it to a *colonus* (tenant farmer),[187] but in all cases it was centrally organized around a main house that exhibited a smaller or larger degree of Roman influence in shape and construction.

When attributes like this have to be derived from features and plans or, even worse, from surface scatters of ploughed-up rubble, several difficulties arise. A villa has two aspects, the *pars rustica*, where the agricultural activities take place, and the *pars urbana*, the living quarters of the person in charge. These parts may, however, take several different shapes and in some cases only the structure of the site may lead to its recognition as a villa. After all, native farmsteads also have an agricultural part and living quarters, but a villa characteristically exhibits a spatial segregation of both parts, with a main house as *pars urbana* surrounded by various other buildings.

This segregation can go further to the point where the main house, as *villa urbana*, stands on its own, the agricultural activities being carried out in other parts (archaeologically different settlements) of the estate. The main building itself exhibits parallel features, changing from fairly plain and small to luxurious and huge, and from structurally united to very differentiated. The latter point, which was made by Rivet,[188] is important because the introduction of corridors and wings opens the possibility of segregating the master from this staff. The simpler plans are socially more comparable to the native farms.

The recognition of these different types of settlements which may be called villae depends primarily on excavations, of which there are at least a few in the eastern part of the Dutch river area. The most extensive one is that at site 214 in Druten. Nearly 2 hectares of this settlement were uncovered and it can be called a villa for several reasons.

First of all, the plan (fig. 78) clearly shows that the settle-

185 See e.g. a list of 26 cases in Britain (Applebaum 1972, 20–5) with direct or indirect clues regarding the owner.

186 As possible owners of villae qualify, for example, the family of Rufius Similis and Mucronia Marcia (*CIL* XIII, 8706; site 445), the *decurion* Valerius Silvester and the trader (also *decurion*) Hilarus from Nijmegen (Bogaers 1972b), and the veteran Marcus Traianus Gumattius (*CIL* XIII, 8806; site 57).

187 On *coloni*, see esp. De Neeve 1984.

188 Rivet 1969, 204–5.

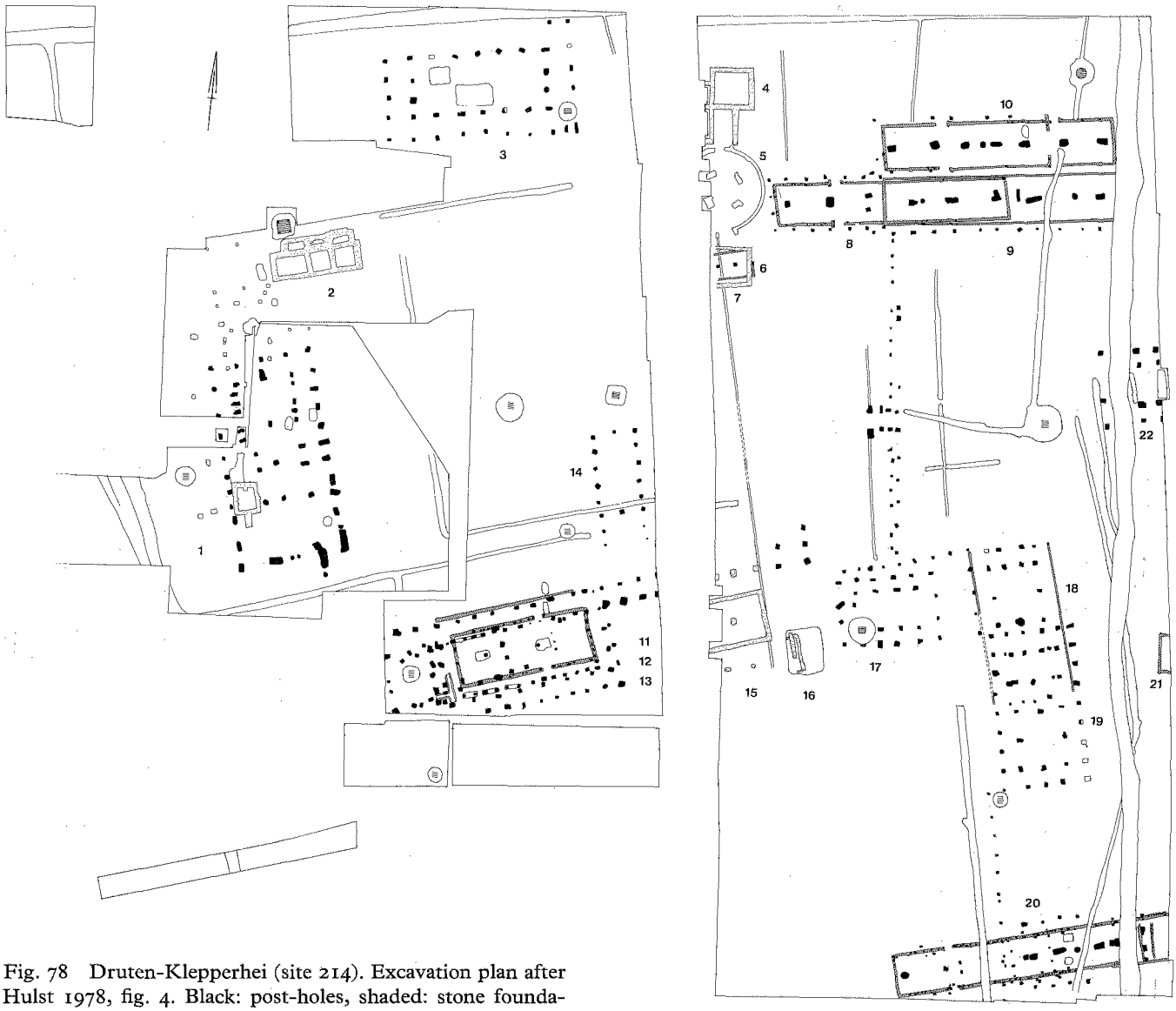


Fig. 78 Druten-Klepperhei (site 214). Excavation plan after Hulst 1978, fig. 4. Black: post-holes, shaded: stone foundations, hatched: foundation ditches, circle with hatches: well. For a description see the text. Scale 1:1000.

ment had a very regular layout with buildings arranged on three sides of a rectangular courtyard. Although several phases and constituent parts can be discerned, it is clear that this was a planned development in which different parts are combined into one centrally managed and organized whole. House no. 1 on the west side and the adjacent bath-house (2) can easily be identified as the residence of this central authority.

The separation between the *pars urbana* and the *pars rustica* is visible in the narrow foundation-trench between the large pit (hut) 16 and the round structure (5), later replaced by a wooden partition-wall with gate between

buildings 17 and 8-9. In the first phase in which the *pars urbana* was fully developed (IIA), it consisted of the buildings 1, 2, 3, 4, 12, and 15.[189] The farms and/or barns 8-10 and 17-20 in the eastern part constitute the *pars rustica* in its successive phases.

A second reason to regard site 214 as a villa are the constructional details of the buildings themselves. Although, as Hulst mentions, the two-aisled plans are clearly a development from the native tradition of house-building, some structures seem to have a *porticus* as an additional feature. There are, however, also one-aisled buildings which are not native and then there is the bath-house which testifies both to the wealth and the degree of its owner's acculturation to a Roman life-style.

The bath-house is built in stone, together with a few other buildings and part of the main house. The latter is somewhat surprising, because the main house of a villa is usually to a larger extent, or completely, constructed of stone. This is not in the last place due to the presence of a heating system, which is also lacking in house 1. It must, however, have been present under the two western rooms of the bath-house, which essentially constitutes one unit with house 1. Both buildings had an interior decoration of wall-painting, a feature that was also present in the stone building 4 and the one-aisled house 12.[190]

Although the native roots of site 214 are very clear and the villa may well have been a continuation of a late-Iron Age settlement situated on an adjacent, unexcavated parcel, there need not be any doubt as to its status as a villa. Roman writers like Varro and Columella would probably have recognized it as such, even though they would certainly not have been impressed by it. The socio-economic implications of the settlement plan are too clear to be misinterpreted, but the aspects of acculturation and wealth are only relevant in the context of the region. There is no point in comparing the villae from the river area to those further to the south in order to compose a list of attributes in the absence of which a settlement cannot be called a villa. Evidently, there are quite a few differences but they are in degree, not in kind. As far as they are centrally organized and, compared to the surrounding settlements, represent a significantly higher level of adoption of Roman norms and values, there is every reason to consider this category of farms in the river area as villae.

Unfortunately, large-scale excavations of sites similar to Druten-Klepperhei are lacking in the eastern river area. There are only three villae where some degree of archaeological investigation has been carried out, namely, Winssen (222), Overasselt (355), and Mook (377).[191] In all three cases, the investigations were limited to small-scale excavations of the main building, in the case of Winssen not even enough to allow a reconstruction. The largely reconstructed plans of the buildings at sites 355 and 377 (fig. 79) are without any substantial context of surrounding features; only a few traces of adjacent structures have been partially excavated (355) or located (377).

The main building in Overasselt measures 10 × 32 m and, in view of the position of the cellar,[192] may have been three aisled. Although its construction is different, it is comparable to building 1 in Druten which measures 15-16 × 31 m, but includes a porticus. In any case, both houses are a very different category of villa compared to the large building in Mook (22.5 × 84 m). Certainly in the context of the river area, this is a monumental and luxurious structure and, even though the terminology is not entirely unambiguous,[193] the excavator's conclusion that it must have been a *villa urbana* is entirely justified. There can hardly be any doubt about its status as the owner's residence and its scenic position high on the slope of the ice-pushed ridge is rather unfavourable as far as industrial and certainly agricultural activities in the immediate vicinity are concerned. It is more likely to be the centre of an estate, in this case conceivably extending over the coversand area to the south and including settlements such as sites 375 and 376.

Although the evidence on sites 214 and 377, and to a lesser degree on 222 and 355, is sufficient to allow definite conclusions, the level of information about other potential villa sites is very much lower. It is restricted to collections of surface material and incidental records of observations in the field. It is necessary, therefore, to develop qualitative or quantitative measures of surface material which permit identification of attributes specific to villae as opposed to other settlements. Unfortunately, as will be discussed in paragraph 8.4.4, the available data

189 Hulst 1978, 148.

190 See also Peters/Swinkels/Moormann 1978.

191 Winssen: Van Kouwen 1978; Overasselt: Braat 1934; Mook: Braat 1934.

192 Cf. Van Es 1981, 183. The foundations in the limited excavation trenches were very poorly preserved.

193 Van Es 1981, note 430. For site 377, see also chapter 6, notes 256 and 257.

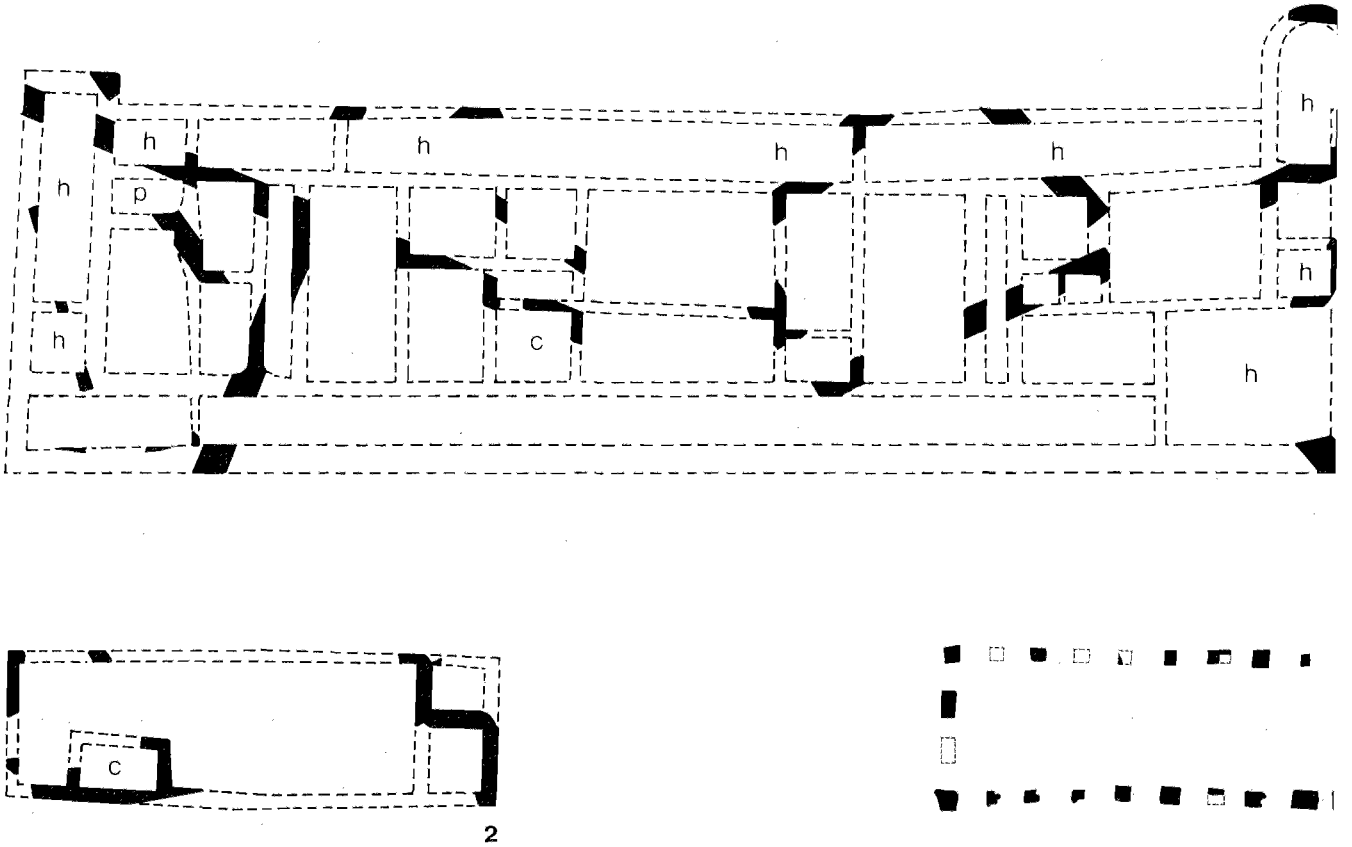


Fig. 79 Plans of the villae at Mook (no. 1: site 377) and Overasselt (no. 2: site 355), and of the building at Lent (no. 3: site 158): h = hypocaust, p = praefurnium, c = cellar. Scale 1:500.

proved to be insufficient to establish any quantitative measures for this purpose. The only attribute which seems to be available is qualitative, namely, the presence of stone buildings at a site.

The application of this criterion does, however, pose certain problems. As was discussed in paragraph 6.4.2, the presence of stone buildings is deduced from the presence of stone, especially tuff, *at the surface* of a site.[194] For some sites, namely, those with an important medieval occupation, the presence of Roman stone buildings cannot be assumed off-hand, because the material may have been brought to the site in post-Roman times. The presence of stone buildings may also indicate other types of settlement. In general, this is not a prol lem because it concerns the military sites, the *civitas* ca ital, and the *vici* discussed previously. But in addition these sites, there may also be small military posts or, f example, rural shrines built in stone. Occasionally, th may lead to a faulty interpretation of the evidence but can only be of minor importance.

It is much more relevant to determine whether the crit rion of stone buildings is indeed suitable to differentia villae from other rural settlements. If it is, then the should not be any villae without them or other settl ments with them. In the absence of abundant excavatic data, it is difficult to determine if such sites are inde lacking. It is possible to eliminate the first possibilit

194 Occasional fragments of tuff or other stone-building m terial are sometimes found during excavation on sites witho stone buildings, but these are not comparable to surface find

villae without stone buildings, by accepting them as a *conditio sine qua non*. There is indeed much to say for such a definition because for a site to be called a villa it must exhibit at least a fairly large degree of Roman influence in shape and construction. Elements such as a cellar, a porticus, wall-painting, a heating system, or a bath need not all be present and do not all presuppose stone building, but they cannot all be absent. Site 214 is a good example in this respect. It is probably as native as a villa could possibly be, but it would still be recognized as such even if the characteristic, centrally organized layout were unknown. Conversely, it is hardly imaginable that someone would start such a planned, new development and at the same time refrain from all amenities of civilized Roman life and do completely without the necessary stone building.

The second possibility, a native village with stone buildings or a stone building which was not or did not become a villa, is the more difficult to evaluate. As far as the eastern river area is concerned, such settlements have never been excavated but there are several potential villae with a very long occupation history, often starting in the Iron Age. It is useful, therefore, to examine a similar and completely excavated site further to the west, namely, the settlement at Rijswijk-De Bult (see fig. 80).[195] Unlike Druten, this settlement was not a planned development: it grew in successive stages from a single farm built in c. AD 30 or, more probably, c. 10 BC.[196] to a maximum size of three households (houses and related structures) and – presumably – a temple, on an area of 1.85 ha surrounded by a ditch (see fig. 81). During the final (3rd century) stage, one of the houses, still at the same spot as the original single farm, was rebuilt in stone and two smaller square stone buildings (one at the place of the wooden temple) were erected. The house, with a plan reminiscent of that in Overasselt, has at least one room with a hypocaust and several with an interior decoration of wall-painting. It seems to have been used for living purposes only, because the normal area for stables is lacking. At the same time, and in addition to other changes, the existing partitioning of the settlement by ditches into separate yards is abandoned.

The interpretation of the data from Rijswijk is not as straightforward as in Druten, due to the long development, and it is further complicated by a possible function as a religious local centre. Nevertheless, during the 3rd century one person, quite possibly a descendant but at least in the line of successors of the first native farmer, controlled the settlement to such an extent that he could plan its reconstruction, build himself a fairly luxurious home, and relocate all agricultural facilities at the two other, subordinate, households. The settlement thus became a centrally organized whole, directed by a person purposely striving for a Roman standard of living and therefore, as Bloemers and Van Es did not fail to notice,[197] was essentially a villa. There are still a few problems with this designation, because not all technical and social aspects of the concept of villa are readily applicable. The absence of a bath, for example, is conspicious, but that may have been simply due to technical or financial difficulties. Also, the *dominus* of the main house may not have been the owner of the settlement, which is suggested by its rather gradual development. He may have been a *primus inter pares*, possibly a lineage-head, and certainly the village chief, but it is very questionable if he owned the settlement in the sense that it could be bought and sold.

It is conceivable that his descendants would have reached that position (if it is at all vital for the interpretation of the site) in the 4th century if developments had continued unchanged. As it is, they were interrupted about or shortly before AD 270, and the (embryonic?) villa that Rijswijk had become, could not develop further. It is, however, still appropriate to call it a villa. For the present discussion, it is especially relevant that the concept can only be applied during the final phase, from c. AD 200-270. Building in stone occurred at the same time as the other relevant changes and was directly related to them.

Although the Rijswijk example does not constitute incontrovertible evidence for the significance of stone buildings in unexcavated settlements, it provides strong support for the hypothesis that such buildings are evidence for a villa. In combination with the other settlements discussed so far, it also shows that there are, in the river area, at least three types of villae:

1 Type Mook: a large and relatively luxurious (Gallo-) Roman building newly erected during the Middle-Roman Period and presumably either the centre of a fairly large estate or a country house.

2 Type Druten: a modest and less luxurious (Gallo-) Roman (cf. Overasselt) or romanized native (cf. Druten)

195 Bloemers 1978a; see also note 109.

196 Bloemers 1979e, note 2, and p. 12 (*Afb.* 6).

197 Bloemers 1978a, 54, 115; 1979e, 11–4; Van Es 1981, 163.

building as the main house of a planned, new settlement during the Middle-Roman Period.

3 Type Rijswijk: a main building similar to 'type Druten' but heading a reorganized, pre-existing settlement usually beginning before the Middle-Roman Period.

To some degree, it is possible to ascribe the unexcavated villae in the eastern river area to these types on the basis of surface material. Sites with occupation starting in the Flavian period or later would then be assumed to be comparable to Druten or, in special circumstances, to Mook; those with a longer occupation to Rijswijk. Such an exercise would, however, be of no great value because there are usually too many uncertainties involved. Not every villa site that begins in the Flavian period necessarily started as a villa. Conversely, older sites which seem to be occupied continuously (cf. fig. 23) may in fact have been deserted for several decades and then reoccupied to build a fully fledged new villa.[198] Even the presence of material from all successive typochronological 'horizons' is no guarantee for absolute continuity because especially during the 2nd and 3rd centuries these horizons are rather broad. Nevertheless, it is useful to examine briefly those settlements which are presumably villae, in addition to sites 214, 222, 355 and 377.

Site 43, Kesteren–De Hoge Woerd. Very limited investigations in 1904, 1960, and 1961 produced no traces of buildings. In 1961, a Roman-Period well was discovered. The site was occupied from the Late-Iron Age onwards and yielded fragments of tiles (including one with a military stamp) and slate, tuff, and plaster with wall-painting. It is noteworthy that De Hoge Woerd is one of the few sites with some extraordinary late-Iron Age artefacts, presence of early-Roman pottery, and evidence for a villa. It may have grown into a villa fairly quickly, possibly during the Flavian period, for which the tile-stamp (probably of the 10th legion) provides some additional evidence.

Site 513, Kesteren–Broekdijk. Limited surface material found in 1981 on a previously known settlement soil included tuff, limestone, quartzitic sandstone with traces of cement, and tiles. The site was already occupied during the Late-Iron Age. The available Roman pottery is datable to the Flavian period and later.

Site 59, Hien–De Wuurdjes. A vast amount of material was salvaged during the stubbing of an orchard, but no excavation was carried out and traces of buildings were not found. The material included tuff, limestone (a.o. a possible fragment of a column), slate, tiles (one with an LXG stamp), fragments of *opus signinum*, and at least one *tubulus*. The settlement must have been a villa and the quantity of the finds permits its construction to be dated to the Flavian period, without previous occupation at the site. The settlement seems to have existed until the very end of the 3rd century, but not into the 4th. A single late-Merovingian sherd must be regarded as a stray find, possibly indicating quarrying for building material, not as evidence for continued occupation.

Site 80, Andelst–De Hoge Hof. The limited surface material includes some tuff and other stone. Evidence for pre-Roman occupation is lacking. On the other hand, there is late-Roman, 5th-century, and Merovingian material, as well as some later medieval sherds. The latter may have landed there through manuring in the Middle Ages, but they make an interpretation as villa less certain.

Site 81, Andelst-dorp. The large amount of mainly surface material indicates occupation from the Middle-Iron Age up to the present day. Although tuff was found at this site, a medieval context is just as likely as a Roman stone building. In the absence of more detailed data, the presence of a villa is a mere possibility.

Site 83, Herveld–De Woerd. There is only limited surface material, indicating occupation from the Middle-Iron Age onwards, with a hiatus in the Late-Roman Period. The tuff may be Roman but also later. Like site 80 it is possible that the late-medieval sherds were contained in manure, but that is not certain. In any case there is a 19th-century report mentioning tiles, tuff, and even pieces of marble.

Sites 85, Herveld–De Legt. The surface material covers all periods from the Late-Iron Age onwards. There are also Merovingian weapons, probably indicating burials. The amount of late-medieval material is considerable which makes the presence of tuff less significant. In addition to tuff and tiles, pieces of *tubuli* and Roman cement are present. Nevertheless, the presence of a villa remains doubtful.

Site 97, Homoet–De Hoge Woerd. The evidence is comparable to site 83, but the total amount of finds is even less and the presence of tuff not securely established. A villa is, therefore, quite uncertain.

Site 99, Elst–Grote en Kleine Zuiling. During soil removal in 1972, several sorts of building material were discovered at this site, including tuff, tiles, *tubuli*, cement and slate. A limited number of sherds indicates occupation during the end of the 2nd and early 3rd centuries

198 See Branigan 1982, 92–4 on some recent British evidence regarding (dis)continuity of occupation.

only. The material is certainly indicative of a villa, but the site is somewhat curious because an ancient settlement soil is lacking. As mentioned in chapter 3 (p. 74), it is possible that we are dealing with a very short-lived villa, which may explain the absence of the characteristic settlement soil. If that were true, it would be the only example of a clearly unsuccesful villa as an agricultural enterprise. Unfortunately, although the dating evidence supports such a notion, its quantity is too limited to be acceptable as proof.

Site 109, Raaijen–De Woerd. Apart from some surface material, most of the finds from Raaijen originate from a very small excavation in 1929, which was never published.[199] The recorded features consisted of ditches and post-holes, but the finds include, in addition to various sorts of tiles, fragments of tuff and limestone, some of them identifiable as parts of ornaments. The apparently native character of the settlement (there are hundreds of sherds of late-Iron Age and Roman Period native pottery) may be a result of the location of the excavation trench, but it is possible that we are dealing with a villa of the 'Rijswijk' type. The excavation-material also includes late-medieval pottery.

Site 212, Druten–Brouwerstraat. A fairly large number of sherds from this site is proof of occupation from the Late-Iron Age to the Middle-Roman Period, but presumably not extending into the 3rd century. Stratigraphical observations in 1971 showed three separate occupation layers, the lowest containing only native sherds, the middle one with Roman finds, including much tuff, slate, and tiles, and the top one with some early-, but primarily late-medieval pottery. Although the total collection of material contains quite considerable amounts of late-medieval pottery, the limited stratigraphical data indicate that the building material belongs exclusively to the Roman Period occupation. Traces of foundations are lacking, but the available evidence makes a villa quite probable. The settlement is located on the northern part of a small Pleistocene dune (*donk*).

Site 219, Deest–Grotestraat. The settlement was discovered during construction work in 1972. The salvaged material includes tiles, tuff, quartzitic sandstone, slate, *opus signinum*, and painted wall plaster. Although late-medieval sherds are present, the presence of a villa was established beyond doubt. In two adjacent construction trenches, the corner and an 18-m-long stretch of a gravel foundation was recorded.[200] The dating evidence indicates occupation from the 1st to the 3rd centuries but the stone structure must have been built relatively late because the foundation cut through an earlier refuse pit filled with 1st- or early 2nd-century material. As on site 59, a single late-Merovingian sherd is a stray find, possibly indicating late-7th- or early-8th-century quarrying activities.

Site 220, Deest–De Hosterd. Surface finds in 1973 included some tuff which may, however, be medieval or related to site 219 only 300 m away. It is quite likely that both sites constitute one settlement but definite proof is lacking. In any case there is a difference in dating because site 220 yielded late-Iron Age pottery and site 219 did not.

Site 234, Ewijk–De Hoge Woerd. Especially during the digging of new ditches in the early 1970s, large amounts of settlement debris were collected at this site. The dating evidence indicates occupation from the Middle-Iron Age onwards; stratigraphical observations showed that the site was raised by 0.5 m only during the later Middle Ages. The tuff and tiles are not likely to belong to that phase and the substantial post-Roman clay cover may explain why the precise location of the Roman stone building has not been located so far.

Site 239, Ewijk–De Grote Aalst. After deep ploughing in 1977, and again in 1982, large quantities of material were collected and several concentrations of building material were recorded. These produced tuff and various other sorts of stone, including slabs of limestone and even marble, cement, *opus signinum*, tiles, *tubuli*, painted plaster, and, as a feature unique in the river area, pieces of a black-and-white mosaic. Even though this is by far the most luxurious villa in the river area known to date, the site itself was already settled during the Middle-Iron Age and continuously occupied until the end of the Late-Roman Period. Most of the pottery, however, dates to the period between c. AD 40 and 270. The settlement, like no. 43, belongs to the category with extraordinary late-Iron Age material, in this case Celtic coins (rainbow-cups), and some early-Roman pottery. In addition, the site has yielded an astonishing amount of late-Roman coins.[201]

Site 245, Beuningen–De Hosterd. Only a modest amount of Roman finds is known from De Hosterd. Building materials include tiles, cement, and some tuff, but on account of the later medieval occupation on and around the site, the presence of a villa is doubtful.

199 Braat 1937, 23, mentions the excavation.
200 Van Kouwen 1972, 11, fig. 1. See also chapter 6, 184.
201 See below, 315 and 446–9.

Site 248, Beuningen-dorp. The situation is similar to site 245, although the building material is somewhat more numerous. A rich Merovingian burial was discovered at this site, which is otherwise characterized by an especially large ancient settlement soil (probably partly medieval) with several findspots of Roman material situated fairly far apart. Observations in trenches in 1969 allow the conclusion that some of the intermediate space was definitely not settled in the Roman Period.

Site 250, Lienden–Reekstraat. On the large, elongated ancient settlement soil along the Reekstraat, several concentrations of habitation have been established, the earliest dating to the Middle-Iron Age. Roman material is found over the entire surface, but observations at the surface and especially in ditches dug in 1971 and 1973 indicated Roman stone buildings on the eastern part. The collected building material includes tuff, limestone, quartzitic sandstone, slate, tiles, and *tubuli*. Chronologically, the settlement is comparable to site 239, because there is evidence for occupation from the Middle-Iron Age onwards and most of the Roman finds date to between AD 40 and 270. There is also evidence for late-Roman and no indication for medieval occupation.

Site 275, Hernen–De Wijnakker. The site was only discovered in 1975. The considerable collection of finds indicates occupation from the Late-Iron Age to the end of the Middle-Roman Period. The late-Iron Age settlement could be a relatively special one because of the large numbers of La Tène glass bracelets discovered there: 37 specimens in the first year and currently over 100. The material includes tuff and tiles; borings in 1975, at the largest concentration of building material at the surface, indicated a stone building of approximately 11 × 25 m. Although recent finds include late-medieval sherds, these are not related to the building.

Site 291, Hernen–De Loffert IV. The site was occupied from the Late-Iron Age until the Merovingian Period. At one spot, stratigraphical information indicates a hiatus between two occupation layers, datable to the Late-Iron Age and the Roman Period. Although no concentrations could be recorded at the surface, the collected material includes tuff in addition to the more common tile-fragments.

Site 298, Wijchen–De Pas, Passerot I. The settlement is part of a conglomerate of sites situated fairly close together but with different datings (cf. chapter 5). A vast amount of material has been collected at site 298 alone, including an equally large number of tile-fragments, two of which had military stamps, of the *vexillatio Britannica* and the *legio XXX Severiana Alexandriana*. The other finds include, for example, Roman window-glass, but in view of the large collection only little tuff and no other building material. It is possible that building activities in stone were minimal, cf. the main house in Druten, but the absence of more tuff or other stone fragments in such a large collection is too curious to accept the presence of even a simple villa except as a mere possibility.

Site 341, Lunen–dorp. Most of the fairly numerous finds were collected during sand-winning. They indicate late-Iron Age to middle-Roman occupation and include tuff, limestone, window-glass, and tiles, one with a stamp of the *legio X gemina*. Unfortunately, there are also clear indications for medieval occupation and in the absence of actual traces of Roman stone buildings the presence of a villa cannot be established beyond all doubt.

Site 358, Overasselt–Valenberg. There are only a few finds from this site, located only 300 m northeast of the excavated villa at the Scheiwal (site 355). They include mostly limestone, quartzitic sandstone, and numerous tiles, with only a few sherds. Although this material probably indicates a Roman stone building, it is also quite probable that it should be considered part of the same settlement as site 355.

Site 424, Beek-Ravenberg and site 425, Beek-Kalorama. These sites are located very close together (less then 100 m) but they stand on opposite sides of a steep valley that may not, however, be entirely natural. It is uncertain whether they should be considered one settlement. In any case, there is little doubt about the presence of middle-Roman stone building at both sites. The material includes tuff, cement, tiles (one with a military stamp v[), hypocaust tiles, and a fragment of a brick tube. In view of the location of these buildings, they may well be comparable to the villa at Mook.

Site 426, Beek–Keteldal. The finds consist of several blocks of building material with cement, washed down the slope of the ice-pushed ridge in 1940. They presumably indicate a villa as on sites 424/425, but it has not been localized precisely. Datable sherds have not been reported.

Site 439, Millingen–Nieuw Zeeland. Although an apparently unsuccesful and in any case unpublished excavation was carried out here in 1936, little material is known. Nevertheless, there is a reliable report by M.P.H. Daniëls, mentioning a large quantity of tuff, *tubuli*, and tiles.[202] It is possible that these materials were

202 See Spann 1967, 27, note 2.

in secondary position and related to a former chapel at this spot, but sufficient data are lacking. The presence of a villa at Nieuw-Zeeland is only a possibility.

Site 441, Millingen–Eversberg. This site has already been discussed as a possible military site (watchtower) which is, on the basis of the available evidence, not very likely. In any case, the 19th-century excavations did show the presence of a stone building of c. 12 × 15 m, which may have been part of a villa. The site also yielded evidence for late-Iron Age occupation, and in the Merovingian Period it was used as a cemetery.

Site 453, Kleve–Kanalstrasse. The little material discovered here during construction work in 1951 may indicate the presence of a villa because the finds include tuff in addition to tiles. The information is too scanty to warrant any definite conclusions.

Site 492, Escharen–Graafsche Raam. In addition to some Merovingian material, there are older reports on Roman building material, including tuff, from this site. If at all reliable, the finds could be related to a recently excavated early-medieval settlement somewhat further north. The available evidence, which could not be verified properly, is hardly enough to consider a villa a remote possibility.[203]

Site 508, Middelaar–Witteweg. The fairly large amounts of material, gathered here during various sorts of construction work in 1980 and 1982, include tuff, hypocaust tiles, and numerous other tiles. Although some stray medieval and especially sub-recent material is present, the concentrations of Roman artefacts indicate the presence of one or more stone buildings. The settlement was occupied from the Late-Iron Age to the end of the Roman Period.

The available evidence shows that there are, in the eastern river area, a maximum of 34 villae. Among these are at least 18 sites or combinations of sites which can be interpreted as a villa with a reasonable degree of confidence based on finds as well as find-circumstances: nos. 43, 513, 59, 99, 212, 214, 219-220, 222, 234, 239, 250, 275, 341, 355/358, 377, 424-425, 441, and 508.

As far as the location of villae is concerned, there are several different groups. Site 513 is the only villa which is close to the Rhine, or rather to the fort Carvo (37). Site 99 lies in the vicinity of the *vicus* at Elst (105) and similarly, sites 275, 341, and 355/358 are likely to be related to the *vicus* at Wijchen (315). Site 508 may be related to Cuijk (500) and the same may apply to site 377 but that villa may also belong to a special group with 424-425, located in scenic positions on the slopes of the ice-pushed ridges and conceivably related to Nijmegen.

By far the largest group, however, is related to the Waal (43, 59, 212, 214, 219-220, 222, 234, 239, 250, and 441) and thereby provided with a direct connection to Nijmegen. Although this does not necessarily imply a direct, let alone an exclusive relation with Nijmegen for each of the sites concerned, or an absence of such a relation for the other sites, the general distribution of villae is clearly indicative of important trade relations with Nijmegen. When the more doubtful villa sites are also taken into consideration, this pattern emerges even more clearly.

There is, of course, nothing really surprising about this, because the presence of Nijmegen as a social and economic focus in the area was one of the main reasons to assume that villae could be present. As such, their distribution is just another example of a pattern familiar from elsewhere. On the other hand, the fact that the theoretically expected distribution emerges so clearly is in itself additional justification for the criteria used to define individual sites as villae.

Other settlements

Unlike the types of settlements discussed so far, the remaining and by far the largest group of settlement sites is only partially identified by positive criteria, by the presence of certain characteristics. A large number of sites is included simply because they lack all characteristics as a result of the low level of information about them. Although the implicit assumption that special traits are indeed lacking will generally be true, it is, of course, also true that some cases will eventually prove to be quite different sites, such as villae, small military posts, or some other constructions.

For the present discussion of settlement typology, this problem is only relevant when individual sites have to be interpreted; the evaluation of different types of settlement depends entirely on sites which have been more thoroughly investigated.

Even for these investigated sites the traditional and almost unavoidable way of thinking is partly based on neg-

203 Further research was impossible because the site is located in the Province of North Brabant, cf. chapters 1, 14 and 5, note 25. For the same reason, the Merovingian settlement was not included in the original catalogue (for further data, see chapter 7.1, no. 548).

ative argument. They are generally termed 'native' and that designation at least partially refers to the absence of 'Roman' attributes. In that sense, terms like humble, poor, less sophisticated, and the like quickly come to mind and are indeed often used. They reflect explicitly the same appraisal which is also encountered in more disguised or unintended forms such as the structure of the present chapter: the succession in which settlement types are discussed! In principle there is, of course, nothing wrong with an ordering of data along those lines which are the most easy to handle and are fruitful for analytical purposes. There is even nothing to be said against the use of terms like poor or humble as long as they are based on an analysis of absence or presence of distinctive attributes. The only trouble is that such terms are also used in a traditional type of archaeological reasoning, which is entirely focused on Roman culture and history and its more prominent manifestations in the provinces, and which is still very much alive. Within such a framework, not only is the presence of diverse native elements easily overlooked but the significance of their absence is not fully appreciated either. A clear example of this are the early-Roman settlements 403 and 417 discussed above. Whatever their true nature may prove to be, on the basis of the historical and presently available archaeological evidence not only the 'Romanness' but especially the utter 'non-nativeness' of both sites may lead to fruitful insights into their function in the region and the initial character of the Batavian elements in the river area.

The lack of native artefacts and the absence (403) or disputability (417) of native structures on these Nijmegen sites are, at the same time, two indicators which can be used positively in the recognition of 'real' native settlements. They depend, however, on a high level of investigation of a site. That may be obvious for structural details which can only be observed after excavation as long as remote sensing techniques are not succesful. But it also applies to artefacts, usually limited to pottery. The significance of the relative proportion of native pottery in surface collections is very difficult to assess for several reasons. First, it is always very difficult and often completely impossible to separate Roman Period from late-Iron Age material, and then the fact that characteristic *middle*-Roman *native* wares are non-existent is even ignored for convenience. Second, as far as selective destruction is at all important, native ceramics disintegrate more quickly than Gallo-Roman pottery. Third, surface collections are definitely biased. Brightly coloured Roman sherds are more easily noticed than the grey or black native material and the experience of the collectors varies. Perhaps even more important is that sometimes the effort spent on collecting native sherds is definitely less than for Roman finds.

These practical difficulties are sufficient to reject all plans to use the quantity of native material as a basis for classifying a settlement, and then the major question of the implications of such a classification has not even been asked. For the Early-Roman Period, when wheel-turned pottery cannot have been widely available, it is evident that the presence of Roman finds or the near absence of native ceramics are suitable to identify the new 'Roman' settlements and possibly also those native sites whose inhabitants were the first to acquire the new materials, the implication being that they are the most important in a social and/or economic sense.

For the Middle-Roman Period, such a line of reasoning cannot be used. On the one hand, the occurrence of very precisely datable types of Roman pottery indicates a strongly increasing general availability, (cf. p. 240), and on the other the use of native pottery diminishes rapidly during the 2nd century (cf. paragraph 6.3). The significance of the ratio of Roman v. native pottery may be quite varied, but on the basis of the present data and on the regional level of analysis it is primarily chronological. It has been explicitly used as such (cf. chapter 6, note 202), which makes it unsuitable for comparisons between middle-Roman sites.

Just as information about structural details, regional data on native pottery can thus only be derived from conclusions reached at the site level of analysis. It is very unfortunate that, while several large or even very large excavations of native settlements in or very close to (see also fig. 80) the river area have been carried out during the past 15 years, virtually nothing has yet been published about the finds. Some of these excavations have only recently been completed, but for all of them there are fairly general preliminary reports which at least give some idea of the structure of the settlements involved. These are Ede-Veldhuizen (site 1; 7.5 ha), Bennekom-Achterstraat (site 7; 2 ha), Heteren-Het Lage Land (site 93; 1.35 ha), Ewijk-Ewijksche Velden (site 232; 2.1 ha), Oss-IJsselstraat (site 487; 0.5 ha), Oss-Ussen (35 ha), and Wijk bij Duurstede-De Horden (excavation still in progress, to date 11 ha).[204] Only the excavation of Rijs-

204 Indicated sizes refer only to the total area excavated and *not* to the size of (the middle-Roman phase of) the settlement.



Fig. 80 Various sites mentioned in the text (chapter 8).

wijk-De Bult (3.5 ha), which was also a native settlement until the end of the 2nd century, has been fully published.

An overview of the structure of middle-Roman native settlements has already been provided by Van Es. In accordance with his analysis,[205] it is possible to conclude that real villages, in the sense of nucleated and structured inhabited areas, are absent among the native settlements south of the Rhine. There are, of course, the *vici*, but these are not native. Most settlements are fairly small, being single farmsteads or consisting of two or three contemporary farms, such as Rijswijk-De Bult before the 3rd century. There are also larger settlements such as those in Wijk bij Duurstede-De Horden and, presumably, Oss-Ussen. From a regional perspective, these places definitely represent a clustering of occupation. At the site level, however, clustering is not so obvious. The published data from Ussen do not allow discussion here, but Van Es' remarks on De Horden indicate that the site is a very loose conglomerate of single farms, and small groups of farms, dispersed over a wide area as far as it was suitable for habitation, the only unifying element being a parcelling system of ditches on habitable parts of the stream-ridge.

A settlement such as De Horden thus has a very open structure and does not exhibit features pointing to some form of central organization. It may properly be called a 'dispersed settlement' type of habitation. In the eastern river area, there are also a few conglomerates of sites which may well indicate such dispersed settlements. Examples are sites 230-232, or perhaps even 227-232, south of Ewijk, 296-303 north of Wijchen,[206] and 507 and 525 in Middelaar. Although the excavations at site 232 were too limited to contribute much to such an interpretation, the evidence from De Horden at least suggests that a seemingly isolated small settlement may actually be part of a larger dispersed settlement. In theory, the lack of nucleation and ordering could indeed lead to 'one' dispersed settlement of more than one kilometre in length as is suggested by the chain of sites south of Ewijk (with many stray finds in between). In any case, the situation there is contrasted sharply by other chains of sites such as 138-144 east of Elst. These are a very clear example of small native sites separated by regular intervals of 600–1000 m so that there is no doubt that each site represents a different settlement.

The designation of clusters of sites as dispersed settlements is, unfortunately, much more tentative when it has to be based on surface collections or small excavations. De Horden has shown, however, that the dispersed settlement as a separate type of habitation does exist,[207] and that it is not comparable to the large nucleated native settlements north of the Rhine, which can be properly called villages. The settlement of Wijster (see fig. 80) is the best known example of such a village, with a considerable number of contemporaneous farms arranged in a more or less orderly fashion on a limited area and some form of central authority located in one relatively large and wealthy farm (*Herrenhof*).[208] There

205 Van Es 1977, 120–5, esp. 125 and 1981, 158–81, esp. 159 and 169–70.

206 See chapter 5, site 231 (situation of the site) and p. 117, note 23.

207 I am grateful to W.A. van Es for discussing with me his provisional conclusions regarding his excavation of the site which was in its final phase at that time.

208 See Van Es 1967, Willems/Brandt 1977, Van Es 1981, 164.

are comparable villages further to the north on the North Sea coast, but these are terp-villages and a site like Wijster could be a more suitable example because the nucleation on terp sites may to some extent be attributed to the particular problems posed by the landscape. But it is not even necessary to look for examples of large native settlements too far afield because they have even been found immediately north of the Rhine in the area covered by the present study. Although only provisional data are available (cf. chapter 5), the settlement in Ede-Veldhuizen (site 1) and possibly that in Bennekom-Achterstraat (site 7) should be included in this category.

It is important to note that the sharp division between types of middle-Roman settlements north and south of the *limes* thus also included native settlements, in addition to the obvious absence v. presence of non-native settlements. Possible implications of this conclusion are discussed in chapter 11.4.2.

Although the large village communities north of the Rhine represent an organized whole that goes beyond the smaller settlements which occur on both sides of the frontier, all are essentially composed of the same elements. These basic habitation units consist of a farmhouse with a number of related features which need not be present in each case, such as a granary, an outhouse or (north of the Rhine!) a sunken hut, and a well, and usually surrounded by a ditch or a fence. Two examples of such yards (*erven*), from Rijswijk-De Bult and Bennekom-Achterstraat, are illustrated in fig. 81, but others have been found over a much wider area and in various 'cultural' and chronological contexts.

As far as the eastern river area is concerned, virtually all middle-Roman native settlements probably consist of one or a few of such household-units. Examples are the farms excavated in Ewijk (232) and Oss-IJsselstraat (487), but in all probability also the post-hole swarms from Heteren (93 and 95), Zetten (75), Bemmel (143), and Ressen (150 and 151). After all, at the best known of these sites, clear groupings are discernable (cf. the quote on page 224 above).

Nevertheless, the continued presence of the post-hole swarm type of settlement into the Middle-Roman Period indicates the continuation of a difference between two types of small native settlements. For the proposed interpretation of this fact, namely, a difference in subsistence-strategy, it is very important that the small houses of the post-hole swarms apparently disappeared during the Middle-Roman Period. The settlement in Heteren (93) was deserted by the middle of the 2nd century

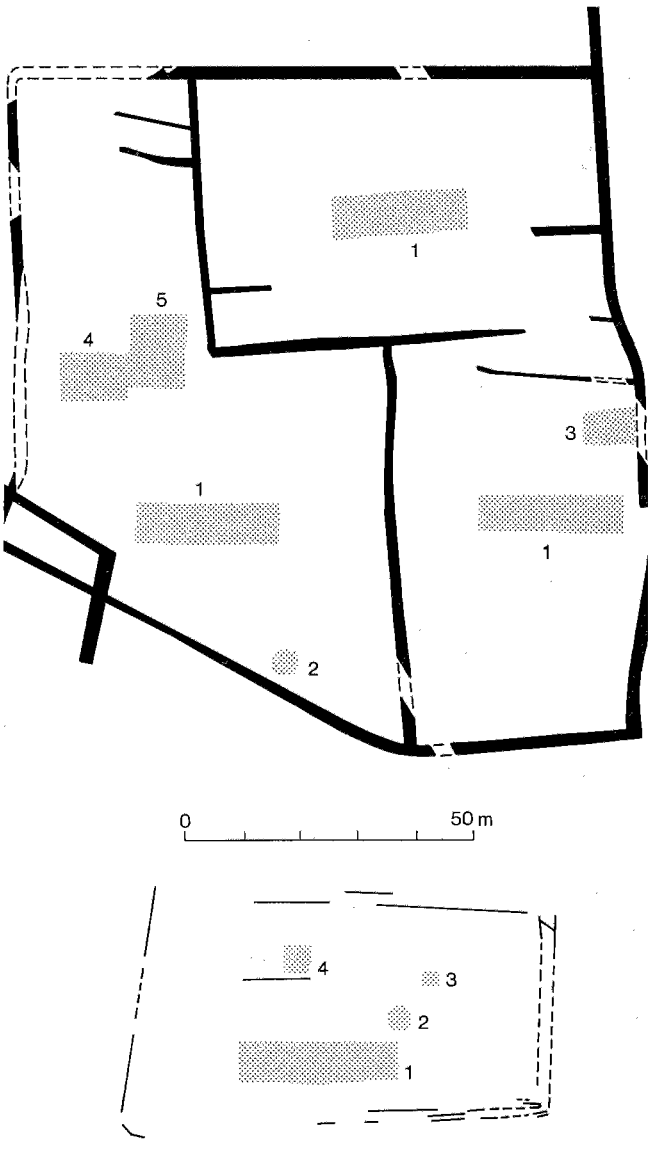


Fig. 81 The settlement at Rijswijk-De Bult, phase 2b (AD 165–200) and household-unit from Bennekom (site 7). After Bloemers 1979e, fig. 5 and Van Es 1973, fig. 6: 1 farmhouse, 2 well, 3 outhouse or sunken hut, 4 granary, 5 temple?

and the same is true for that in Ressen-De Woerdt (151). The excavations in Ewijk (232) have shown that the Iron Age post-hole clusters were replaced by clear Roman Period house-plans situated at the edge of the terrain inhabited during the Iron Age.

The evidence from the trenches dug at Heteren-Uilenburg (95), Bemmel (143), and Ressen-Kerkenhof (150), cannot be reliably evaluated. Only Zetten (75) may provide a case against a proposed termination of the constructional tradition leading to the archaeological phenomenon of the post-hole swarm in the early part of the 2nd century. The finds seem to indicate more or less continuous occupation (cf. fig. 23), but it is possible to disregard this suggestion because the vast majority of the pottery consists of native wares and most of the sparse Gallo-Roman pottery is datable from IB-IIA. One might, therefore, argue that the site was abandoned around AD 150 to be reoccupied again only in the 4th century, and attribute the few sherds which chronologically could cover the intermediate period to the mid-2nd century. They are then taken to mark the end of the first occupation, which, in fact, conforms exactly to the interpretation of sites 93 and 151. On both these sites the very few possible later 2nd- to 3rd-century sherds are undoubtedly correctly interpreted by the excavators as marking the end of the occupation around AD 150 when these types (Niederbieber 89 or 104) first appeared. The only difference is that these two sites were never reoccupied. The somewhat formalized dating procedure followed in chapter 4 led to a suggestion of continuous occupation for site 75, which may not be justified in view of the above argumentation but which had to be maintained because of possible additional evidence in this case.[209]

So far, the available evidence thus suggests rather strongly that the settlements with irregular house-plans did not continue throughout the Middle-Roman Period but that the last of them disappeared around the middle of the 2nd century. This conclusion as such supports the interpretation of these sites as a different type of settlement from those with recognizable house-plans, which continued to exist as can be demonstrated by excavations such as those in Ewijk (232) and Oss (487).

If the tradition of building small and irregular houses is indeed functionally determined by a specialized subsistence-strategy, then the reasons for the disappearance of this type of settlement should also be at least partly economic in nature. This is a matter that will be returned to in chapter 11, but it is clear that such reasons are not difficult to suggest. If native settlements which specialize in agricultural production finally disappeared in the first half of the 2nd century AD, the most likely explanation must be that they were replaced. The alternative that they were no longer necessary can hardly be true (at least when they are evaluated in purely economic and not in social terms) because the need for production of grain and other crops or of meat in the region around Nijmegen could only have increased and not decreased. Replacement can only occur when something better is available or, perhaps more to the point, when the need for something more effective arises, and that is precisely what can be observed. The villae in the eastern river area, insignificant as they may be from an architectural point of view, surely represented the centres of much more effective organization of labour and exploitation of the land. They were first established, or in other cases (and perhaps somewhat later) began to be formed as a result of a reorganization of existing settlements, in Flavian times. But the heyday of the villa system is in the 2nd century, and that must be the main reason for the disappearance of specialized native agricultural settlements. There must have been other reasons, such as the 'pull' of a flourishing town and *vici* and the increasing adoption of Roman norms and values, which perhaps motivated the rich Batavian to invest in a villa and the modal Batavian to at least no longer wanting to live in a crummy 9-post house. But all such reasons are related and can be attributed to a general process of social and economic development in the second half of the 1st and first half of the 2nd centuries which led to the rise of one settlement type and the disappearance of another.

Apart from the settlement-types discussed so far, there may be others which cannot yet be recognized as such. An example is the excavated site 158 in Lent,[210] which is essentially a household-unit as described above: a house with associated features such as a granary and a well. The site was occupied from the Late-Iron Age (cf. p. 224) until the 3rd century, and may have been a single unit or have belonged to a larger dispersed settlement (156, 158). The excavated household-unit itself has been dated from c. AD 70-150 or a little later.

In contrast to other native sites, the building from Lent is a fairly large (11 × 27.5 m) one-aisled structure and represents a clear adoption of Roman building methods (see fig. 79). Similar buildings have been found else-

209 The terra sigillata sherd Drag. 37 (fig. 28, 56) from the Argonne belongs to the *ware with ovolo G* which might be too late to allow a dating around AD 150. The arguments for its chronological position (Haalebos 1977, 119–20) are, however, not beyond all doubt.

210 Van Es/Hulst, in prep.

Fig. 82 The composition of the pottery assemblage at 23 sites.

Site	Interpretation	Roman Pottery											Roman: Native v	
		Terra Sigillata		Colour Coated Ware		Belgic Ware		Smooth Ware		Coarse Ware		N (total)	Rom.: nat. Rom.	Rom LIA nat.
		raw	%	raw	%	raw	%	raw	%	raw	%			
1	settlement	8	7	1	1	16	14	31	27	58	51	114		2:8
38	*vicus*	26	8	23	7	32	9	89	26	180	51	351	9:1	
48	settlement	26	13	19	9	31	15	32	16	95	47	203	9:1	
59	villa	21	7	31	10	60	20	58	19	136	44	306	>9:<1	
75	settlement	2	3	1	2	8	13	16	25	36	57	63		1:9
93	settlement	2	3	0	0	10	14	19	28	38	55	69	3:7	
105	temple (*vicus*)	1	1	4	5	13	15	52	59	18	20	88	>9:<1	
109	settlement (villa?)	0	0	3	3	20	21	28	29	44	46	95		2:8
117/8	*castellum* + *vicus*	56	14	37	9	72	17	69	17	179	43	413		4:6
126	*castellum*	78	26	22	7	12	4	86	22	119	41	298	>9:<1	
194	*castellum* + *vicus*	26	12	4	2	11	5	87	41	86	40	214	>9:<1	
214	villa	56	6	53	5	198	19	247	25	465	46	1019		7:3
232	settlement	71	10	40	5	93	13	191	25	349	47	744		5:5
234	villa	9	9	8	8	5	5	22	21	60	58	104		4:6
250	villa	17	11	13	8	14	9	38	24	77	48	159		4:6
275	villa	10	7	2	1	18	12	26	18	92	63	146		5:5
289	settlement	19	15	23	18	17	13	11	9	58	46	126		9:1
298	settlement (villa?)	17	9	21	11	28	15	33	18	85	46	184	>9:<1	
315/6	*vicus*	29	8	14	4	44	11	83	21	217	56	387		6:4
341	settlement (villa?)	4	3	4	3	9	7	24	19	88	68	129		8:1
407	*canabae* (west)	210	14	30	2	79	5	502	33	707	46	1528	>9:<1	
412	*castra*	367	20	52	3	52	3	472	26	892	49	1835	>9:<1	
416	*canabae* (east)	868	17	246	5	238	5	1446	28	2344	46	5142	>9:<1	

where in association with villae, such as the wooden building preceding the villa of Kaalheide (prov. Limburg) or building II at the villa of Garsdorf, Kr. Bergheim (BRD).[211] Comparable from a building tradition point of view are also several one-aisled structures in Druten (see fig. 78).

Although there are no arguments to consider site 158 a villa, it is not a normal native settlement either. The rather large granary associated with the building adds further weight to this conclusion. But there are not enough data for a clear interpretation of the site, let alone for discovering others with the same characteristics. We may be dealing with in some respects a 'romanized' native settlement, with a kind of native proto-villa, or with something else. In this respect, there may be some direct connection with soldiers or civilians from Nijmegen on the other side of the river. But we do not know that and there is no way of testing hypotheses in that or other directions. For the moment, we therefore have no reason to consider site 158 a socially and economically different kind of settlement, and thus a different type.

But the settlement in Lent does illustrate that there may be a much larger variety than we are able to distinguish at present. For the moment, the analysis of native settlements cannot go beyond the recognition of four different types of sites. All of these are essentially composed of one or more basic household-units. The differences between the types are either related to the ordering and number of these units or to constructional differences between buildings which (may) have social and economic implications.

Type 1 Nucleated villages with a considerable degree of

211 Kaalheide: Brunsting 1950; Garsdorf: Piepers 1959 (see fig. 80).

Source
excavation ROB[215]
excavation ROB[216]
surface + soil removal
surface + soil removal
excavation RMO[215]
excavation ROB[216]
Bogaers 1955
excavation RMO[215]
surface + soil removal
excavation ROB[215]
dredging
excavation ROB[216]
excavation ROB[216]
surface
surface
surface
surface
surface
surface + dredging
surface
excavation ROB[217]
Bogaers/Haalebos 1976, 1977, 1980b
excavation ROB[218]

internal structuring occurring only north of the Rhine.[212]

Type 2 Settlements composed of one or a few household-units.

Type 3 Settlements composed of several loosely connected household-units which are so close together that a dispersed settlement is formed.

Type 4 Settlements composed of one or a few household-units but without the well-built houses which yield archaeologically identifiable plans. The last of these sites seem to disappear around the middle of the 2nd century.

8.4.3 Quantitative Analysis of Settlement-Finds

In the introduction to this chapter, the question of quantitative measures has already been raised. It would be useful if it were possible to characterize a type of settlement by the relative proportions of various artefacts, especially different kinds of pottery. A first approach to this issue has been included in chapter 6, in the discussions of various types of pottery, building material, and glass. It was, however, based only on raw scores (numbers of artefacts of type x at site y) in relation to other attributes of sites, such as the presence of stone buildings.

It is necessary, therefore, to examine more thoroughly the feasibility and significance of quantitative measures. The potential usefulness of comparing the relative proportions of different kinds of pottery on different sites has already been shown elsewhere, such as the histograms of pottery from York and from a number of Dutch settlement sites.[213] Of course, it is possible to use other artefacts as well, but those are normally only available in sufficient quantities after excavation, and even then their numbers are often quite small. It is conceivable that increased use of metal detectors will produce a large enough data base of metal finds in the future, but it is not available yet.

The present analysis is thus limited to pottery, but in order to achieve useful results a few other restrictions must be imposed, namely, spatial and chronological comparability. The first of these conditions is easily met by including only sites from the eastern river area, the micro-regional level as defined in chapter 1. It would be very worth while to include other sites from close by or

212 This type of settlement is the only one for which the term village is appropriate (cf. e.g. Waterbolk 1982, 99). Although many of these sites are late-Roman and later, earlier examples are not lacking (cf. *op cit.*).

213 See Perrin 1981 (York) and Bloemers 1983.

214 Instead of late-Iron Age wares, the native pottery of site 1 includes a sizeable portion of late-Roman and early-Medieval sherds.

215 Data based on examination of the finds.

216 Data based on field notes kindly provided by R.S. Hulst. For site 38, these do not include the material from the 1984 excavations of which figures recently became available. The new totals are: t.s. 83 (10%), colour-coated 47 (6%), Belgic ware 59 (7%), smooth ware 217 (27%), coarse ware 399 (50%), with N = 805 and Rom.: native Rom. sherds as >9:<1. It is clear that these new figures would only very slightly – but, as far as interpretations are concerned, in a positive way – influence further calculations below.

217 Data based on detailed field notes from trenches 38–46, 57, 59–60, 67, and 126–8 of the Nijmegen excavations.

218 Data based on detailed field notes from trenches 1–19 (partly), 24, 49, 51, 53–4, 61–6, 68–9, 98–9, 101–3, 106, 109–10, 117–8, 120–2, 124–5, 143, 147–9, 152, 155–60, 167–8, and 177 of the Nijmegen excavations.

further afield for comparison, but such a step would introduce too many uncontrollable variables at this point such as distances from production centres, to mention only one important factor. For the same reason, the results of the analysis should not be compared unconditionally to those from similar quantitative studies in other areas.

Chronological comparability is achieved automatically here, because there are virtually no sites with sufficient data from the Early- or Late-Roman Periods. The point is, nevertheless, theoretically important because the same problems arising from a comparison between sites in different regions are met with settlements in the same area but under very different circumstances in the successive stages of the Roman occupation. Apart from this, chronological comparability also poses a practical problem. When all the pottery of a site is considered and minor errors resulting from incorrect datings of wheel-turned pottery are left aside, there are still the hand-made native wares. It is already difficult to separate late-Iron Age from Roman Period native pottery, but it is impossible to differentiate between native pottery from the Early- and the Middle-Roman Periods.

For this reason, only the potential information contained in the relative quantities of wheel-turned pottery can be evaluated statistically. Although difficult to handle, it is nevertheless important for the final interpretation to know the ratio of wheel-turned (henceforth termed 'Roman' for convenience) to hand-made pottery at each site. These ratios are therefore included in fig. 82, in two columns: one for Roman Period settlements only (Roman: Native Roman) and one for settlements with a longer occupation (Roman:Late-Iron Age + Native Roman).

The data in fig. 82 were collected from various sources but, although some of them are only provisional, there is no reason to suspect that these are less reliable. The relative quantities have been calculated by counting rim-sherds,[219] except for some categories of smooth ware: flagon neck-fragments and handles[220] have also been counted as rims to reach a more balanced estimate of the percentage of smooth ware in relation to other Roman pottery, where breakage produces many more rim-fragments. Special categories of pottery which occur in minute quantities (cf. paragraph 6.2.6) have not been co sidered in fig. 82. The so-called 'Holdeurn ware', whi is sometimes distinguished from other pottery, is n considered separately but (depending on the past divided between the smooth ware and the coarse war or omitted (the fine Nijmegen ware).

The number of sites in fig. 82 is limited, because the on sites included are excavated sites from which the da could be obtained, or surface collections with N > 1(for Roman pottery. Fortunately, however, most of tl different settlement types as discussed in the previo paragraph are included.

Examination of the various Roman wares in fig. 82 in mediately reveals that sites 105 and 289 are very diffe ent from the others. Site 105 scores extremely low (both terra sigillata and coarse ware, and extremely hig on smooth ware. This anomaly may almost certainly l attributed to the fact that the finds are all from the exc vation of the Gallo-Roman temples at Elst. It is not su prising that the pottery assemblage at such a site is di ferent from all others.

Site 289 has a very abnormal percentage of colour-coa ed ware and far too small amounts of smooth ware. F a site without any special characteristics the percenta of terra sigillata is also surprisingly high, as well as tl ratio of Roman to native wares, especially because th include late- and even middle-Iron Age material. In th case, there is no obvious archaeological explanation f these anomalies, so it is assumed that they result from strongly biased sample due to chance discoveries or s lective collecting.

Both sites will be omitted from further calculations. F the remaining 21 sites, the following measures[221] ha been obtained (figures based on percentages):

	Terra sigillata	Colour-coated ware	Belgic ware	Smooth ware	Coarse ware
$\bar{X}$	9.86	5	11.24	24.19	49.90
Md	9	5	12	25	47
S	5.99	3.21	5.43	5.79	7.03
CV	60.75	64.20	48.31	23.94	14.09

219 On the correlation between rims and the total number of sherds, see Perrin 1981, 68.

220 The one- and two-ribbed handles which are especially typical for amphorae and two-handled flagons were counted as 0.5 rim.

221 The arithmic mean $\bar{X}$ and the median Md are measur of central tendency. The standard deviation S and the coefl cient of variation CV, defined as $CV = \frac{100 \times S}{\bar{X}}$, are measur of dispersion.

The CV expresses group variability as measured by S in terms relative to the central tendency of that group as measured by $\bar{X}$. It indicates that only two categories, the smooth ware and the coarse ware, have a clear unimodal distribution with a relatively narrow range. In other words: although the sites in the analysis are very different, they all have a pottery assemblage composed of approximately 25% smooth ware and 50% coarse ware. The same cannot be said of the other three categories, which are flatly distributed and which, for the total population of sites, could even turn out to be composed of several different distributions.

The sample distributions of the percentages of smooth and coarse ware indicate that they are by themselves evidently unsuitable as discriminating characteristics of settlements. Thus, they do not have to be examined further, although a few extreme values require attention. As far as coarse ware is concerned, sites 275 and 341 have rather high percentages. These deviations may be caused by the relatively low value of N in both cases. For smooth ware, the remarkably high value for site 194 should also be discussed.

It is possible that we are dealing here with a biased sample, caused by the find-circumstances. The material was brought up by dredging and large pieces of thick-walled pottery (amphorae, dolia, and mortaria), which are included in the smooth ware category, may be more likely to be brought up and discovered. Such pottery is indeed amply represented in the collection from site 194, especially mortaria. These have been counted separately and their relative frequency in relation to smooth ware is indicated in fig. 83.

In the calculation of the measures S, $\bar{X}$, and CV, sites 105 and 289, but also site 1, have not been taken into account. Although the general proportion of smooth ware on site 1 is average (fig. 82), it is clear that the percentage of mortaria is extraordinary low. Given the indicated S and $\bar{X}$, the chance that a site where only 3% of the smooth ware consists of mortaria belongs to the same statistical population as those in the sample can be determined by computing the *t*-ratio. Because this presupposes a normal distribution and that condition is not satisfied by the distribution of percentages with data outside the 30–70% range, these have to be transformed. This can be accomplished by the arcsine transformation.[222] The transformed percentages are indicated in fig. 83.

To ensure that a comparison between the percentages of mortaria at site 1 and those at the sites in the sample is really significant, a level of $\alpha = 0.01$ is called for. At this level, if the percentage at site 1 is indeed too small to be accidental ($H_o = \mu \geq 32.25$), the critical value of *t* with 19 degrees of freedom (*df*) is $t = 2.593$. The *t*-ratio has to be computed by the formula for comparing a single variate to a sample.[223] This gives $t = 3.863$ which considerably exceeds the critical value. The null hypothesis (H_o) can thus be rejected and we may conclude that there is a significant difference between site 1 and all the others as far as mortaria are concerned.

This difference is especially interesting because it cannot be attributed to a biased sample from site 1. Granted that one would feel more comfortable if the values of N were larger, it is, nevertheless, very important that the

Fig. 83 The relative frequency of mortaria.

Site	N	% of smooth ware	transformed %
1	1	(3)	(9.97)
38	21	24	29.33
48	6	19	25.84
59	16	28	31.94
75	7	44	41.55
93	6	32	34.45
105	17	(33)	(35.06)
109	8	29	32.58
117/8	25	36	36.87
126	20	23	28.66
194	38	44	41.55
214	57	23	28.66
232	33	17	24.35
234	8	36	36.87
250	17	45	42.13
275	10	38	38.06
289	1	(9)	(17.46)
298	9	27	31.31
315/6	25	30	33.21
341	11	46	42.71
407	101	20	26.57
412	137	29	32.58
416	321	22	27.97

$\bar{X} = 32.25$; $S = 7.39$; $CV = 21.71$.

222 For a description see Sokal/Rohlf 1969, 386–7.

223 See Thomas 1976, 240–1. The formula is given as $t = \frac{(\overline{X} - X_i)\sqrt{n/(n+1)}}{S_x}$, where X_i is the value of the single variate, S_x is the standard deviation of the sample and $df = (n-1)$.

pottery at site 1 was collected during an almost complete excavation of an entire settlement. Acceptance of the reliability of the sample leads to an interesting archaeological explanation because, of all kinds of pottery, mortaria are probably the most intimately connected with a roman(ized) kitchen. The fact that site 1 is not just a native settlement but also the only one north of the Rhine can hardly be a coincidence because native settlements south of the Rhine all have more or less average percentages of mortaria. Both statistically and archaeologically site 1 belongs to a different group of settlements and, although more data are needed to draw definite conclusions, this may well point to an aspect of acculturation that was strongly negatively influenced by the presence of the imperial frontier. After all, site 1 is only 10 km beyond the Rhine.[224]

To return to the problem presented by the relative frequency of smooth ware at site 194: it is clear from the data in fig. 83 that the percentage of mortaria is somewhat exceptional. The question whether or not there is a significant difference from the other sites can again be evaluated by computing the *t*-ratio. In this case, $H_o = \mu \leqq 32.25$. At a level of $\alpha = 0.01$ the critical value for *t* with 19 *df* is $t = 2.593$ and the computed value is $t = -1.228$ which is less than the critical value. Consequently, the number of mortaria at site 194 is not significantly different and there is no reason to assume that we are dealing with a biased sample caused by a disproportional amount of collected mortaria.

An alternative explanation for the high percentage of smooth ware may be provided by the data on site 105. That is a cult site, and site 194 is known to be a military settlement on the basis of the qualitative interpretation of the finds. Nevertheless, the dredged-up finds could be a mixture of material from a fort and a temple in the adjoining *vicus*. This interpretation implies, however, a whole series of assumptions which may not be valid and cannot be tested. It is also dependent on the representativeness of the sample from site 105. The N(= 88) is too low to be completely confident in this respect and there is no guarantee that the observed frequencies are generally typical for cult sites. On the other hand, the data were collected through excavation of the temples an there is no reason to assume any directional bias. More over, the discovery of a rural cult site in Born-Buchte (Province of Limburg) in 1976 also yielded an enormou amount of smooth ware.[225] Perhaps it is also no coinci dence that one other site in fig. 82, namely, the part o the *canabae legionis* west of the *castra* in Nijmegen (407) has 33% of smooth ware. As already mentioned in para graph 8.4.2, the excavations at site 407 have yielde comparatively little evidence for trade and industry They did produce evidence of burial monuments and o stone buildings which may have had some public func tion, perhaps of a religious nature, as could be suggeste by a fragment of a gigantic mortarium (diam. c. 1 m) an their proximity to the cemetery (409).

The evidence from site 407 may also point to a differen explanation for the high number of smooth ware vessel from site 194, namely, that it is caused by material fron an eroded cemetery. Although this cannot be backed b quantitative data here,[226] flagons, jugs, mortaria, and th like are normally present in very large numbers in ceme teries.

Unfortunately, any explanation for the percentage o smooth ware at site 194 will remain a mere guess becaus the original context of the finds is lost. All that is certai is that some extraordinary (post-?)depositional proces must have led to the observed relative frequencies an that in this case one should be grateful for the presenc of some qualitative data which allow at least a partial in sight in the nature of the site.

In contrast to the sample distributions of the percent ages of smooth and coarse ware, those of the other thre groups of pottery in fig. 82 have a wide variability, ex pressed by the high value of the coefficient of variation This suggests that terra sigillata, colour-coated ware and Belgic ware might be useful as discriminating char acteristics of settlements.

In that case, there should be an association between th type of settlement and its percentages of these thre classes of pottery. To test the presence of such an associ ation, the settlements have to be ordered according t

224 Mortaria are not completey absent in some settlements further north, for example in Dalfsen, Province of Overijssel (Van Es/Verlinde 1977, 46, *1.1.5b*), and in Wijster, Province of Drenthe (Van Es 1967, 174). See also chapter 11, p. 431.

225 See Bloemers 1977, 19–22 and Willems 1983b, 251–4; the excavation has not yet been published. Cursory inspection of the finds indicates that the percentage of smooth ware (wit some unusual forms) may lie around 50%.

226 There are, of course, publications from which relevan information can be obtained. See e.g. Stuart 1977, 71, Brun sting 1937, 97, and others.

some independent criterion. Although, as already noted, factors such as distance from production centres and chronology are constant for the sites discussed here, it is difficult to establish such a criterion because the reasons for different amounts of certain pottery products may still be varied. This could reflect the (in)ability to acquire some kind of pottery for social reasons (degree and direction of acculturation), plain preferences, or perhaps even specific rules (for example, 'the army gets first choice'). Therefore, the criterion for ordering settlements should be as general as possible and it has been defined as the 'accessibility rate to Roman pottery'.

There is no completely objective way to operationalize this criterion, but there are a number of reasonable assumptions. These are based on the degree to which the inhabitants can be expected to participate in a cash-based market economy and the degree to which they are likely to be acculturated to Roman norms and values. For the eastern river area, the ordering is thus also based on the degree of affiliation with the Roman army which, in theory, could be cross-cut only by the one large town (from which no sample is available). For social and economic reasons, the purely military sites should thus rank first (412, 126) and the two *vici* of the *canabae legionis* second (407, 416). Third are two sites (117/8, 194) of which the finds are probably partly of a fort and partly of the surrounding occupation. Site 38, which must be closely related to a fort, and 315/6, which has been interpreted as a *vicus* and a secondary centre, should rank higher than rural settlements and have therefore been lumped into a fourth category. These rural settlements could be considered a fifth group, but they can be divided into those that are definitely villae in the sense as discussed earlier (59, 214, 234, 250, 275) and those that are not, or not definitely (1, 48, 75, 93, 109, 232, 298, 341). The inhabitants of the villae, insignificant though the buildings may be compared to the rich establishments further south, are nevertheless more likely to have access to Roman pottery: they are more likely to want it and to be able to afford it.

The result is thus a hypothetical ordering in six groups which should at least reflect in descending order the range of options in the acquisition of pottery which are open (for whatever reason) to the inhabitants. The percentages of terra sigillata, colour-coated ware, and Belgic ware can also be ordered from high to low. Because we are dealing with an ordinal scale of measurement, a non-parametric measure of statistical correlation has to be employed, for which Spearman's r_s is adequate. It measures the degree to which rank-orderings along two variables match.[227]

According to the most adequate formula, which takes into consideration the large number of ties, the association between X and Y_1 is $r_s = .72$. The association between X and Y_2 is $r_s = .08$ and between X and Y_3 it is $r_s = -.63$. These values of r_s indicate a considerable positive correlation between the site orderings and those of terra sigillata, very little when colour-coated ware is concerned and a rather large degree of negative correspondence with Belgic ware. It is possible to test r_s for statistical significance, which requires establishing a level of significance (α). In order to be of any real use, the percentage of a certain kind of pottery should be rather strongly related to (characteristic for) a certain type of settlement. Therefore, a significance at the level of $\alpha = 0.01$ is preferred over the more commonly employed level of $\alpha = 0.05$. The significance of r_s can be assessed by establishing the t statistic, using the normal approximation to Spearman's rank-order coefficient:[228]

$$t = \frac{r_s\sqrt{n-2}}{\sqrt{1-r_s^2}} \quad \text{with } df = (n-2)$$

The values for the r_s of Y_1, Y_2, and Y_3 are $t = 4.52$, $t = 0.35$, and $t = 3.54$ respectively. For a one-tailed test with $\alpha = 0.01$ and $df = 19$, the critical value is $t = 2.539$, which implies that there is a significant positive correlation between X and Y_1 and a significant negative correlation between X and Y_3.[229]

A test for statistical significance as employed here pre-

227 The normal formula for r_s is $r_s = 1 - \frac{6\Sigma d^2}{n(n^2-1)}$ in which d = the difference between rank numbers for each variable. The measure varies between 1 (perfect match) and -1 (perfect negative match), 0 indicates no match at all. Sometimes ranks are the same for several units (e.g. 126 and 412 are both 1; 59 to 275 are all 5). When there are many such ties, another formula has to be used:

$$r_s = \frac{\left[\frac{n^3-n}{12} - \left(\Sigma\frac{t_x^3-t_x}{12}\right)\right] + \left[\frac{n^3-n}{12} - \left(\Sigma\frac{t_y^3-t_y}{12}\right)\right] - \Sigma d^2}{2\sqrt{\left[\frac{n^3-n}{12} - \left(\Sigma\frac{t_x^3-t_x}{12}\right)\right]\left[\frac{n^3-n}{12} - \left(\Sigma\frac{t_y^3-t_y}{12}\right)\right]}}$$

t_x = the number of units that, when ordered along variable X, scores the same; t_x indicates how many units are tied.

t_y = analogous to t_x

228 See Thomas 1976, 401–5.

229 Although irrelevant for the present discussion, the two values of t are even significant at the level of $\alpha = 0.001$.

Fig. 84 The ranking for settlement type (X) and terra sigillata (Y_1), colour-coated ware (Y_2), and Belgic ware (Y_3).

Site no.	X	Y_1 %	rank	d	d^2	Y_2 %	rank	d	d^2	Y_3 %	rank	d	d
126	1.5	26	1	.5	.25	7	7.5	−6	36	4	20	−18.5	342.2
412	1.5	20	2	.5	.25	3	14	−12.5	156.25	3	21	−19.5	380.2
407	3.5	14	4.5	−1	1	2	17	−13.5	182.25	5	17.5	−14	196
416	3.5	17	3	.5	.25	5	10	−6.5	42.25	5	17.5	−14	196
117/8	5.5	14	4.5	1	1	9	3.5	2	4	17	4	1.5	2.2
194	5.5	12	7	−1.5	2.25	2	17	−11.5	132.25	5	17.5	−12	144
38	7.5	8	12.5	−5	25	7	7.5	0	0	9	13.5	−6	36
315/6	7.5	8	12.5	−5	25	4	12	−4.5	20.25	11	12	−4.5	20.2
59	11	7	15	−4	16	10	2	9	81	20	2	9	81
214	11	6	17	−6	36	5	10	1	1	19	3	8	64
234	11	9	10.5	.5	.25	8	5.5	5.5	30.25	5	17.5	−6.5	42.2
250	11	11	8	3	9	8	5.5	5.5	30.25	9	13.5	−2.5	6.2
275	11	7	15	−4	16	1	19.5	−8.5	72.25	12	11	0	0
1	17.5	7	15	2.5	6.25	1	19.5	−2	4	14	7.5	10	100
48	17.5	13	6	11.5	132.25	9	3.5	14	196	15	5.5	12	144
75	17.5	3	19	−1.5	2.25	2	17	.5	.25	13	9.5	8	64
93	17.5	3	19	−1.5	2.25	0	21	−3.5	12.25	14	7.5	10	100
109	17.5	0	21	−3.5	12.25	3	14	3.5	12.25	21	1	16.5	272.2
232	17.5	10	9	8.5	72.25	5	10	7.5	56.25	13	9.5	8	64
298	17.5	9	10.5	7	49	11	1	16.5	272.25	15	5.5	12	144
341	17.5	3	19	−1.5	2.25	3	14	3.5	12.25	7	15	2.5	6.2
					$\sum d^2_{Y_1}$ =	411.0			$\sum d^2_{Y_2}$ =	1353.5			$\sum d^2_{Y_3}$ = 2405.(

supposes random sampling. Although for our data randomness can only be assumed, it is important to note that the test as such is not inappropriate because it is directional. If, for example, the surface collections as such were biased, they could only contain too much terra sigillata – which is always picked up and easy to spot – or too little Belgic ware, which could conceivably be overlooked. A negative bias for terra sigillata or a positive bias for Belgic ware, which cannot even be easily identified as such in the field, are completely impossible. This implies that, should some of the samples in fig. 82 indeed not be randomly collected, unbiased samples would produce lower values for terra sigillata and higher values for Belgic ware in the lower rows of fig. 84 and thus even more significant correlations!

Conclusions

It is clear that quantitative measures are a potentiall very valuable tool in the evaluation of sites and will cer tainly lead to unexpected new insights as more an larger samples become available. It is possible to tes most results for statistical significance, which is an ad vantage over the qualitative measures usually employec Although the present number and size of the sample still leave much to be desired, they have provided usefu new information.

In particular, there is a statistically very significant cor relation between the ordering of sites on fig. 84 and th relative amount of terra sigillata. This implies that th more military a site is, the more terra sigillata is found o it, and provides a criterion to interpret the nature of site

in the eastern river area.[230] The town may be an exception to this, because 'degree of affiliation to the army' was not an independent criterion for ordering the sites but the result of ranking along lines of acculturation and economics. The town is, however, so obviously different from other sites that a faulty interpretation is impossible.

Although not discussed in detail here, the data in fig. 84 have also been subjected to a discriminant analysis to investigate if the ordering of sites relevant to all three variables (Y_1, Y_2, and Y_3) simultaneously would provide additional information.[231] For the analysis the sites were divided into four groups (rows 1–4, 5–8, 9–13, and 14–21 in fig. 84). The results showed, first, that the total variance of all variables was almost completely determined by the first function of which terra sigillata proved to be the most influential component. It also showed that the first group (1–4) is clearly distinct from the others, among which there are varying degrees of overlap. The fourth group (14–21) can also be discriminated, but the second and third group are not very uniform.

These results agree with the above conclusion. The two definitely military sites and the two *vici* of the *canabae legionis* are indeed clearly discriminated from the others by their pottery assemblage and, to a lesser degree, that is also true of the native settlements in the fourth group. The overlap between the last three groups is to some extent caused by the fact that not the total pottery assemblage of all sites could be used, but only the Roman pottery. Therefore, the percentage of terra sigillata was calculated relevant to the total amount of Roman pottery and not to all the pottery from a site. For some sites this does not make any difference. As is evident from fig. 82, the percentage of terra sigillata at the four highest ranking sites (412, 126, 407, and 416) will not at all be influenced if native pottery were included. The same is true for sites 194, 59, and 298, while the influence on the figures for sites 38, 48, 214, and 341 will only be slight. The terra sigillata proportion of the total amount of pottery used at the other settlements during the Middle-Roman Period will be lower than the percentages indicated in fig. 82 to varying degrees.

It is clear that consideration of native pottery does in fact increase the significance of the correlation between quantities of terra sigillata and the ordering of settlement types. Of the first seven sites on fig. 84, which are interpreted as military, closely related to the army, or a mixture of both, only site 38 has a relatively low percentage of terra sigillata.[232] That may imply – as was also indicated by the discriminant analysis – that it ranks too high in the ordering of sites. The motivation for this relatively high rank is its presumed close affiliation to a fort in Kesteren: it may have been a *vicus*. However, as already mentioned earlier (p. 269), the excavated features increasingly support this interpretation and so do the finds (cf. note 216).

For the other six sites at the top, the percentage of terra sigillata exceeds 10% and, as far as they are purely military, it reaches 20%.[233] All except one of the remaining sites have percentages below and mostly far below 10%, the highest non-reducible (by including native pottery) percentage being 9% for site 298. The only exception is site 48 with a percentage of terra sigillata that is reducible, but only to 12%. This may imply that site 48 is not the simple native settlement it has been assumed to be, unless the sample is biased. Other finds from this site, especially a fragment of terra sigillata with part of a graffito and a military tile-stamp EXGERINF, which are not by themselves significant, might point to some military function of the site, perhaps a watchtower or signal station. Unfortunately, no structural details are known, although it is certain that the settlement never had any buildings in stone.

Unlike terra sigillata, the ordering of settlements is not significantly correlated with the incidence of colour-coated ware. The values for the different settlements oscillate around a mean of 5% but the variability, as expressed by the CV of 64.20, is very high. As this varia-

230 The relative frequencies of pottery from the probable fort in Rijswijk-G. (Van Es 1984, 279) illustrate this point. Recalculated in the groups as employed here, the percentages are: terra sigillata 22.7, colour-coated ware 12.7 (?), Belgic ware 5.2, smooth ware 24.4, coarse ware 35.1 (?). Only the ratio of Roman to native ware, which is 6:4, could be curious if it were all from the same site and/or contains no late-Iron Age pottery (both are uncertain). The percentages are based on all sherds, not on rims as they are here.

231 The computer analysis (SPSS package, see Nie a.o. 1975) was provided by A. Voorrips (IPP, Amsterdam). I am very grateful for his assistance not only in this respect, but especially for his critical comments on various drafts of paragraph 8.4.3, which have greatly improved it.

232 See, however, note 216 above.

233 Only the percentage of site 117/8 will be reduced by native pottery but this reduction is limited because most of the hand-made wares are pre-Roman (cf. p. 252).

tion cannot be accounted for through differences between the settlements, it is probably caused by the fact that the numbers of colour-coated ware are always rather small and when N is also small accidental factors cause considerable variability. In any case, all excavated sites with a really large N (214, 232, 407, 412, and 416) have 5 % or less colour-coated pottery.

The significant negative correlation between the ordering of settlements and the proportion of Belgic ware shows that the more 'native' a settlement is, the more likely it is to have relatively large amounts of Gallo-Belgic pottery. Although this is in accordance with traditional typological interpretations of these wares, the relation may not be entirely as straightforward as might be concluded. That is because, in contrast to the Early-Roman Period, the rather limited repertoire of middle-Roman Belgic pottery consists of two main groups: 'true' terra nigra, most of which is rather fine tableware, and terra nigra-like pottery, which consists mainly of storage and cooking vessels such as the 'types 1 and 2' discussed in paragraph 6.2.3. In addition, there are Belgic types manufactured in wares that are not different from coarse wares.

The 'true' terra nigra occurs in small percentages ($\leq 5\%$) on almost all sites, but its occurrence is also chronologically determined and most forms are known to disappear during the 2nd century if not even earlier. In contrast, the other Belgic pottery is virtually lacking at military sites, relatively frequent at the various other settlements and amply available throughout the 2nd and 3rd centuries. The negative correlation can therefore be attributed to these wares and to the chronology of the sites and it does not compensate for the positive correlation of terra sigillata.[234] Most of the Belgic pottery is probably simply an alternative for coarse ware. The frequencies in fig. 82 suggest that it may not have been a very popular alternative on military sites, but, because of a possible chronological bias, more detailed data are necessary to examine that proposition.

8.4.4 Cemeteries

Not surprisingly, a considerable number of middle-Roman burial sites has been discovered in the eastern river area. For obvious reasons, there are not nearly as many as there are settlement sites, but the total of 78 listed in chapter 5 is much more than for any other period. These 78 sites include 12 cases for which the interpretation as burial site cannot be conclusively demonstrated (nos. 10, 33, 82, 84, 110, 155, 208, 241, 346, 456, 496, and 503) and 1 special case (no. 378) which is not definitely a Roman findspot.

Most of the evidence from all these sites has been collected through chance discoveries, looting, or incidental and very restricted rescue operations. The number of properly excavated sites is very small: there is only one completely excavated cemetery (site 396) and there are five cases where at least part of a cemetery could be investigated properly (sites 39, 305, 398, 409, and 491). Of the excavations, those on sites 305 and 491 have been published in final form. For the other four, the references in the catalogue are preliminary or partial reports. Although many of the other burial sites are also published in one form or another, often just descriptions of the objects or of the circumstances leading to their discovery, there are only a few detailed reports (for example, on site 474) which at least contribute to our knowledge of individual burials.

This state of the evidence clearly does not permit a sophisticated differentiation of all known burial sites, but at the regional level of analysis the available data give a reasonable insight into the variability of burial sites and practices, as well as their relation to other sites.

The discussion of burials from the Early-Roman Period (paragraph 8.3.3) showed that there was a change in native burial practices which can only have been induced by the Roman conquest, as well as the introduction in the area of Gallo-Roman cemeteries, at that time still directly related to the centre of the Roman military and administrative apparatus. The native ways of disposal-of-the-dead changed in a way that increased their archaeological visibility. Of course, this development continued and can easily be monitored during the Middle-Roman Period. As indicated by the evidence presented in chapters 4 and 5, the number of graves datable to before AD 70 is still limited, but it increases rapidly during the Flavian period, even if the Nijmegen cemeteries are not taken into account. During the 2nd and 3rd centuries, it reaches a peak.

The interpretation of this chronologically straightforward development is, however, rather complicated. It is, for example, very difficult to determine to what extent native burial practices were changed. It is altogether feasible that part of the native population continued to be

234 This may, however, have been true during the Early-Roman Period when Belgic copies of terra sigillata forms may have been both cheaper and more easily obtained.

buried in the archaeologically almost untraceable 'late-Iron Age' way. For all we know, this might even be a very large part, although the (rare) firm associations of small native settlements with cemeteries indicate that it was probably not. A good example are the clearly associated sites 93, a post-hole swarm type of native settlement, and 530, an accidentally discovered cemetery of unknown size with cremation burials.

An additional difficulty in the evaluation of the native component in burials is the fact that aspects of the native burial program such as cremation of the body, deposition in a grave pit, surrounding the grave by a ditch, erecting a barrow over it, and the like occurred over a very wide area. They are native but they are also encountered in Gaul or, for that matter, in Italy. Although future analysis may provide useful insights into this problem, it seems that for the moment the introduction of grave goods in native burials remains the only really tangible form of change. As soon as this change had spread more widely, which must have been the case towards the end of the 1st century, what is native and what is not are difficult to separate archaeologically.

In itself, this observation may be taken as an example of a process of acculturation. But such a conclusion is as obvious as it is valid and requires the detailed research at the level of the cemetery and individual interment for which sufficient published data are lacking. At the regional level, however, it is possible to recognize some cemeteries which are native.

Firm associations with native settlements constitute one of the clues. In addition to the example of sites 93 and 530 mentioned above, there are a few additional cases: 130–131, 152–153, 395–396, 458–459, 473–474, and 493–494. For various reasons, several other associations are less reliable. The most interesting case is that of the completely excavated site 396 (see fig. 69) which shows at least one typical form of a native cemetery. Unfortunately, the associated settlement is not very well known. The same is true for the similar cemetery at Schaijk (site 491), where several findspots indicate a native settlement in the immediate vicinity.[235] Another similar cemetery and a native settlement with recognizable house plans have, however, been fairly completely excavated in Oss-Ussen.[236] Finally, a fourth example from the central river area, in Zoelen (see fig. 80), should be mentioned here as well, although in this case there are several possibilities for an association with a nearby settlement (all native).[237]

Although they are very much alike, there are also differences between these cemeteries. The published plans of sites 396 and 491 show that the number of graves with, compared to those without, round or square ditches varies considerably. There is also evidence indicating that ditches may be completely absent in some cemeteries. Site 530 could be an example, but the rescue operation following its discovery did not lead to a complete excavation. A small cemetery from Wijk bij Duurstede provides a better example, but unfortunately it cannot be said to be definitely associated with a native settlement.[238]

This last conclusion also indicates the limitations of identifying native cemeteries by association with native settlements. A much better insight would be gained if it were possible to identify native cemeteries by, for example, a typical general composition of the finds or the absence or presence of certain grave goods. Even looted or destroyed sites might then yield more information. The lack of published material does not allow many remarks on this subject as yet, but the ongoing research on excavated sites does suggest that at least some systematic differences of this kind are indeed present.[239] The only possible clue that should be mentioned here is the presence or absence of native pottery in graves. A provisional list of cemeteries with native pots includes 22 of the 77 sites: 163, 208, 292, 306, 324, 331, 336, 348, 353, 368, 373, 389, 396, 398, 409, 413, 418, 428, 454, 491, 501, 530, and 543. Evidently, there are a number of cemeteries in this list, most notably nos. 398–418 in Nijmegen, which are definitely not native. The presence of native pots as such thus obviously implies nothing for a cemetery, although it could be relevant for interpretations at the level of individual burials, especially during the Middle-Roman

235 Data in the CAA (findspots 45F/25z or 45F/12z, 17z, 18z, and 27z). See also note 1.

236 See note 77; for a partial plan, see Bloemers/Louwe Kooijmans/Sarfatij 1981, 113. Location on fig. 80.

237 See *JROB* 1979, 42.

238 See Verwers 1975, esp. 122–4. New excavations in 1984, not far from this site, may, however, change these conclusions. A cemetery was discovered, comparable to sites such as 396 or 491, which belongs to the settlement at De Horden, and the small burial ground published by Verwers may eventually prove to be part of this larger graveyard (pers. comm. W.A. van Es).

239 Pers. comm. J.K. Haalebos and R.S. Hulst; see also Bogaers/Haalebos 1980a, 7.

Period. The alternative approach of considering the relative quantity instead of presence-absence of native pots does not appear to be very promising either. There are no sites with more than a few of such vessels and these are, if at all, normally datable to the 1st century.[240]
In paragraph 8.3.3, the continuity of some prehistoric urnfields into the Early-Roman Period has been considered. For the Middle-Roman Period, at least sites 306, 319, 324, 331, 346, 393, 454, and 543[241] are also spatially related to prehistoric burials. This might identify them as native, although it should not be forgotten that the people buried there were not at all necessarily inhabitants of native villages. Even immigrants into the region are likely to have shared the common urnfield heritage and thus not to have necessarily been averse from burial at such a site. If there should be a more than accidental relation between sites 411 and 409 as mentioned in paragraph 8.2.2 (p. 231), continuous or renewed use of the same area to bury people does not identify the Roman Period cemetery as native because, even though quite a few Batavians may have been buried at site 409, it is certainly not a native cemetery. It is, however, just as likely that a relation between sites 411 and 409 does not exist. The same may be true for site 419, with several prehistoric barrows (and site 543) in the vicinity. Furthermore, the development by the army of the entire area in Nijmegen between the Valkhof (403) and the Kops Plateau (417) does not seem to have been conducted with any respect for what was present there. This is exemplified by an urnfield and several older Bronze Age and Neolithic barrows on site 412, which were ruthlessly levelled because they were in the way.[242]

In addition to the continuous or renewed use of urnfields, there is yet another 'prehistoric' way of disposal-of-the-dead practised during the Roman Period, namely burial under a barrow. These tumulus graves are als native, but they are different from the native burials dis cussed above. Instead of belonging to the least acculti rated group of the native population, they are usuall considered to be the graves of wealthy and highly accu turated villa owners. The only possible instance of suc a tumulus in the eastern river area – it has never been in vestigated and thus might be prehistoric – is site 37 close to the wealthy villa of Mook (site 377).[243] Sever kilometers to the southwest, in Esch, and only a few kilc metres east of our area, in Moyland, other tumuli wer excavated (see fig. 80).[244]
At least for the eastern river area the tumulus graves, they occur at all, are probably not native in the sense c being directly connected to traditional burial practice They are more comparable to other wealthy burials to b discussed below, and clearly the northernmost example of a practice which was concentrated in southern Be gium, especially west of the Meuse.[245]
Essentially, the difficulties with the concept of 'native' i the discussion of tumuli, but also of the other buria discussed so far, reflect the ongoing process of amalga mation of burial practices and thus of the religious an social concepts behind those practices. As far as the rel gious and ethnic background is concerned, we may neve be able to unravel completely exactly what the origins c observed phenomena are. On the other hand, the proces of amalgamation undoubtedly resulted fairly quickly in new set of internally consistent standards. Death an burial are too important social events in any society to b without conventions regulating the ritual and technic acts to be performed as well as their significance for th community.[246]
It is relevant to note that amalgamation is a very preci term to describe what happened. In paragraph 8.3.3 th Roman influence on native burial practices has alreac

240 A few exceptions of relatively late (2nd-century) burials with native pots are known, e.g. from site 368 (Bogaers/Morren 1954, 59) and 396 (Bogaers/Haalebos 1980a, 3).
241 A group of low barrows next to the 6 Roman cremation burials at site 543 has not been excavated. The burials under the barrows could thus also be Roman and comparable to the Moyland barrows (see note 244 and Brunsting 1948, 78, note 1), although there are other barrows in the area which are probably prehistoric. The Roman graves could also belong to site 419.
242 See Louwe Kooijmans 1973.
243 The site is not the same as the equally hypothetical tumulus at Mook mentioned in Isings 1959, 9 and note 22, of which the reported finds have been included in the catalogu under site 383.
244 Esch: Van den Hurk 1973, 1975, 1977, and 1980; Mo land: Isings 1959. The interpretation of the latter group i however, quite uncertain. The relative poverty of the grav and the large number of them suggest that the low barrows b longed more probably to an exceptionally well-preserved cen etery comparable to sites 396 or 491. Nevertheless, at least th one burial described by Isings could indeed be a tumul grave.
245 See e.g. Van Doorselaar 1967, general map.
246 See e.g. Willems 1978.

been mentioned because, for the Early-Roman Period, it is the most striking phenomenon. But it is also clear that the burial program of the society in the river area during the 1st–3rd centuries was not the result of a one-sided process. Indigenous norms and values contributed to it, as well as those of other non-Roman groups. The end-product can, therefore, not be considered the result of 'romanization', even though the entire development was caused by the Roman conquest and Roman values undoubtedly set some important standards.

The latter is, of course, the reason why, at the level of cemeteries, similar developments can be seen in widely different places in the Roman empire. There are adequate recent overviews of these developments, with particular reference to the province of Germania Inferior.[247] Although the variation in burial types is large, the cemeteries as such are difficult to analyse typologically. Apart from the 'native' cemeteries discussed above, all burial sites contain larger or smaller numbers of pits with – as a rule – the cremated remains of the deceased. The size of a cemetery obviously depends on the size of the community burying its dead there. In itself, the differentiation in large and small on Appendix 3 is not particularly relevant. It is only an impressionistic approach to size and thus not even entirely reliable. There are, however, at least a few instances where size is, or could be, relevant.

The most obvious cases are the burial sites at the lower extreme of the size scale, namely the single graves. There are quite a few of those in the river area: sites 35, 44, 82?, 153, 155?, 163, 174, 208?, 263, 327, 332, 344, 354, 372, 378?, 382, 387, 393, 431, 434, 455, 456?, 458, 462, 474, 505, 517, and 529. Some of these sites may have been part of a cemetery, together with other known burials in the vicinity (e.g., site 382) and there surely are others which were in fact part of an undiscovered or destroyed cemetery. The remaining cases of true isolated burials are thus fewer in number, but there is no reason to doubt their existence. They are definitely a separate type of burial site because they are not cemeteries: theoretically they could be considered a category of 'isolated find' (cf. fig. 5), which was defined in chapter 1 as a findspot representing a single activity. Such an interpretation, faulty as it may seem to be, could be appropriate, but it cannot be considered here because of the lack of a regional burial analysis and knowledge of the total burial program of the society involved. That program may well have included a whole series of circumstances under which the deceased should be buried in isolation, and conceivably in a specific manner.[248] Most societes do indeed have such rules and, if they pertain to the isolated burials discussed here, emphasis on the isolated find aspect would not be very fruitful.

In the absence of further analysis one might, however, also envisage simple coincidences leading to isolated burials. These would then be just another category of findspot reflecting human use of the landscape for which interpretation as isolated find is not inappropriate.

Although admittedly of little practical use, the discussion of single graves shows that a typology of burial sites can proceed on formal characteristics but its relevance will need to be substantiated further. Apart from the isolated burials, there are as yet no definite reasons to use the size of cemeteries for typological differentiation, although it could be relevant in combination with other factors. For example, the huge number of barrows in Moyland (see note 244) is one of the reasons not to interpret them as tumulus graves in the sense of the Belgian group. Conversely, rich burials in tumuli, which are probably correctly interpreted as interments of wealthy villa owners, occur only in small groups such as in Esch (or isolated!). Typologically, such family (?) cemeteries are comparable to other rich rural burials or cemeteries, such as interments in sarcophagi or square walled enclosures.

The latter have not been found in the eastern river area[249] but they do occur, possibly as a separate segment, as part of the large main cemetery (site 398) of Ulpia Noviomagus.[250] These excessively wealthy burials are presumably themselves a characteristic of a large urban cemetery. Such cemeteries are always located outside towns and alongside roads, where monumental burial fa-

247 Van Es 1981, 199–214; Bechert 1982, 239–55.

248 Examples for such a situation are provided by the four isolated burials from Rijswijk (Bloemers 1978a, 216–9) which are also unusual as inhumations instead of cremations. Moreover, two are extended, one is partially and one fully flexed.

249 For an example, see Von Petrikovits 1956, fig. 1 and 6. A possible example in the river area might be site 440. Although no structural details are known, some finds from this cemetery are very exceptional.

250 See Bogaers/Haalebos 1984. It is possible that the spatially segregated group of 6 large, ditched enclosures in the native cemetery of Oss-Ussen (see Bloemers/Louwe Kooijmans/Sarfatij 1981, 113) is in some ways comparable.

cilities were designed to have an impressive effect. Such roadside burials often led to elongated cemeteries stretching out over several kilometers. This may also be true in Nijmegen, where a 3rd-century burial in a sarcophagus (site 397) could in fact mark the southernmost extension of site 398.[251] Remains of burial monuments are also known from Nijmegen,[252] but actual foundations, apart from the recently discovered wealthy burials at site 398 mentioned above, have only been found at site 407, the western ward (*vicus*) of the *canabae legionis*. Undoubtedly intended to reach a maximum visual effect, two of such monuments were built at the very brink of the steep slope of the ice-pushed ridge.[253] The monuments at site 419, of which only possible foundations are known,[254] may have stood in a similar position and alongside the road from the fortress (site 412) to the *limes* in the east, if that should indeed be its direction (see chapter 3.4 and below, p. 301).

Discussion of the Nijmegen cemeteries automatically leads to another possible distinction between burial sites, namely, civilian and military cemeteries. It is relatively easy to differentiate the Nijmegen sites in this way, because nos. 409, 413, 414, 415, 418, and 419 (+ 543?) are clearly chronologically and spatially associated with the military installations and related sites, while no. 398 (+ 397?) belongs to the town, no. 400 being an intermediate case. It is less easy to determine whether this differentiation has any relevance as far as cemeteries are concerned. They were used for a special group of the population and thus, for example, more likely to include unusual burials or an exceptionally large proportion of male interments. In practice, however, they may not have been very different.

First, there are no indications that normal soldiers' graves are different from those of civilians, but this is again a matter that can be evaluated only after burial analysis. For example, the (compulsory) membership of burial clubs may or may not have led to special burials or a characteristic layout of the cemetery. Second, there is no reason to assume that 'military' implies that only soldiers were buried at a cemetery. Not just the deceased members of senior officers' families, who actually lived in the military establishments, but also the wives or concubines and children of other soldiers, as well as other inhabitants of the *canabae legionis*, could be buried at military cemeteries.

On the other hand, although the significance of a distinction between military and civilian cemeteries cannot be fully appreciated, it is certainly not a priori irrelevant at a regional or any other level of analysis. At the very least it is worth while to know where soldiers were buried because of the implications for the interpretation of the regional pattern of sites. The absence of special characteristics for military cemeteries, whether it is real or the result of the low level of information, does not cause great difficulties for classification. The interpretation of the Nijmegen cemeteries on the basis of their location is an example, but other clues are not lacking. The inscriptions on tombstones, such as the one erected (at site 419) for L(ucius) Cassius Clemens, soldier of the 10th legion (*CIL* XIII, 8734), are the most obvious source of information. They are, however, only very seldom found at cemeteries and more often in a secondary position. Typical military grave goods, which would have been very useful, are also rare. They are not lacking, for example, some specific types of fibulae or an occasional phalera, but weapons in military graves are almost completely absent,[255] probably because they were returned to if not reclaimed by the army.[256] The only objects of unmistakably military origin which occur more frequently are those which can be identified as such by a graffito. In view of the arguments provided in paragraph 6.5.2, a high percentage of objects, usually pottery, with owner's marks is very likely to indicate a military population. But it is not always necessary to argue by comparison in this case, because there are often some graffiti with additional information such as the function of the owner (*eques, vexillarius, signifer*) or his unit (*turma* of ..., *centurio* of ...).

251 For another interpretation, see Van Buchem 1955, 33.

252 Brunsting 1948, 78: *spolia* from the Valkhof.

253 See Bloemers 1979d, *Abb.* 50, 10 and 11, and *Abb.* 55. See also fig. 12: the monuments were excavated before the construction of the large office-building.

254 Brunsting 1948, 78.

255 Cf. Breeze a.o. 1976, 81. Note, however, that graves with weapons as such are not as exceedingly rare as is often assumed, cf. the compilation by Van Doorselaer 1965. As convincingly argued there, the weapons in this case are not indicative of a military burial but of a high status burial entirely in a civilian context. This conclusion was once again confirmed by the recent discovery of very rich burials at site 398 mentioned above. Grave 8 contained weapons (Bogaers/Haalebos 1984, 3) and together with a previously discovered rich grave at the same site (Brunsting 1937, 184) it shows that the only weapon graves from Nijmegen occur in the civilian cemetery of the town, not in the military cemetries!

Apart from Nijmegen, there are only two other cemeteries with burials of soldiers. For site 501 (Cuijk) there is little information, but it is evident that the cemetery is not only spatially but also chronologically associated with the fort (499).[257] It is thus quite likely to have initially been a military cemetery.

The other site qualifying as such is no. 39 in Kesteren. As mentioned in paragraph 8.4.1, the fort in Kesteren has not been located (site 37 is a hypothetical site) but the indications for its presence are very strong, site 39 being one of them. The evidence for military burials at site 39 is constituted by a number of graffiti, at least one of them certainly military.[258] Furthermore, the cemetery is extraordinary because of the presence of 19 horse burials. An occasional horse burial is not uncommon on both sides of the *limes*,[259] but these are usually found in isolation, in or near settlements rather than cemeteries, and may also be offerings if not simply the disposal of the carcase of a diseased animal (*kreng*) in some cases. The practice of burying horses in a cemetery seems to be exotic; the only area in Europe where it occurs more often during the Roman Period is the Danube basin (eastern Austria and Hungary), in the Noricum-Pannonian area as well as the Sarmathian area on the other side of the *limes*.[260] It is tempting to connect this observation with the fact that in Flavian and Trajan times most of the auxiliary soldiers which were not recruited in the Rhine provinces are known to have been of Danubian origin.[261] Although such an interpretation is highly speculative, it is at least probable that the horse burials do indeed indicate military activity: archaeozoological investigation of the bones has shown that the horses were significantly different from the much smaller native horses during the Roman Period and even later, and comparable to the horses from the fort at Valkenburg Z.H.[262]

Conclusions

Examples of the variability of disposal-of-the-dead practices in the Middle-Roman Period have only occasionally been mentioned in the above discussion. They are amply documented in the overviews by Van Es or Bechert (cf. note 247) and, given the present state of the evidence, only incidentally relevant to the general differentiation of cemeteries attempted here. The number of well-excavated and published sites is still too small to draw reliable conclusions, although it is clear that differences at the regional level do exist; but in view of the evidence, it is more appropriate to discuss these in relative terms rather than discrete types. Only size offers a criterion for distinguishing discrete types at the extremes: there is one very large urban cemetery, site 398, with an estimated number of 12,500 graves. At the other end are a number of isolated burials, the significance of which is largely unknown.

The other burial sites vary in size from very small groups of graves to as many as the 4000 interments estimated for site 409, but most of the somewhat larger cemeteries probably contained hundreds instead of thousands of burials (no. 39 (incomplete), 120; no. 396, c. 250; no. 491 (incomplete), 63). As far as these 'middle-range' cemeteries are concerned, there is a difference between those that are more and those that are less native, disregarding for the moment the possibility of some sites where most of the deceased were still buried according to the archaeologically hardly detectable late-Iron Age practice. Site 396 could be an example of the former; it belongs to a native settlement. The small remnant of site 305, perhaps part of one of the cemeteries belonging to the *vicus* in Wijchen (site 315), does not seem to have native characteristics. These are presumably also absent on the remaining Nijmegen cemeteries, which can be expected to resemble site 409 and other Gallo-Roman cemeteries that normally appear as dense clusters of pits

256 Possible exceptions are mentioned by Breeze a.o. 1976 (Camelon) and Gechter/Kunow 1983 (Mehrum). The grave of Mehrum, however, was definitely the burial of a native, probably a chieftain but perhaps a soldier of an irregular unit or employed at an individual basis as a scout. The weapons in question only resemble Roman weapons, and the same is true for the Camelon burial discussed by Breeze a.o. 1976. Weapons of Roman soldiers were normally bought back by their units at the time of discharge or after death (*op. cit*, 93–5). This also accounts for the multiple owner's names which sometimes appear on the same piece of equipment.

257 The finds could not be studied in detail. Cursory examination of the material in the RMO did, however, reveal that it includes a fair proportion of pre-Flavian pottery (a.o. terra sigillata) and the suggestion (Bogaers/Rüger 1974, 84) that the cemetery belongs only to the 2nd- and 3rd-century *vicus* of Cuijk (site 500) is not correct.

258 *JROB* 1974, 23.

259 Van Es 1981, 203–4.

260 See Müller-Wille 1970–71, esp. 169–76 and 187–8.

261 Alföldy 1968, 103–4. Most of them were, however, Thracians (see also chapter 11, p. 403).

262 Prummel 1983b.

with cremation burials. On the other hand, the provisional report on the military burial site 39 also mentions three graves surrounded by a ring-ditch which suggests native burial practices there as well.
The smaller cemeteries of only a few to several dozens of graves will presumably not be very different from the larger ones. The only known exceptions, the small graveyards with interments of wealthy villa owners, have not been found in the eastern river area, although they may well have been present. Sites 378 and 397 are possible instances, just as the vanished grave of M. Traianus Gumattius, of which only the tombstone remains (site 57).
In addition to the above remarks aimed at demonstrating differentiation, the limited success of these attemps already indicates that the clear differences observed for the Early-Roman Period are not so easily established for the following centuries. For a period in which processes of acculturation must have caused profound changes, such a result is hardly surprising.

8.4.5 Isolated Finds

Most of the isolated finds on Appendix 3 are simple stray finds: findspots of sherds, coins, or other Roman artefacts. To some degree, these are useful as an additional illustration of man's use of the landscape, but quite a few of them have dubious locations: a reconstructed findspot or a location in a possibly or certainly secondary context. The last category includes a few finds which are important for other reasons, such as the (military?) material from sites 18a, 135, and 546, or a few altar-stones with inscriptions (128, 422, 445), and a fragment of a milestone (427).
Some of the above, as well as several other isolated finds, have already been discussed in preceding chapters where they provided valuable information. Examples are the military graffiti and metalware from sites 120 and 125 in relation to the probable fort in Driel and the possible significance of the net-sinkers from site 199 for the dating of the river IJssel as a branch of the Rhine. There are, however, a few isolated finds which have hardly or not at all been mentioned.

Isolated (usually stray) finds are in general useful additional evidence for the reconstruction of land and water routes, if not for the actual roads (cf. paragraph 3.4). The finds from site 242 in Ewijk are a good example of a direct relation, in this case with a secondary road. The fragment of a milestone (site 427) is almost certainly re-

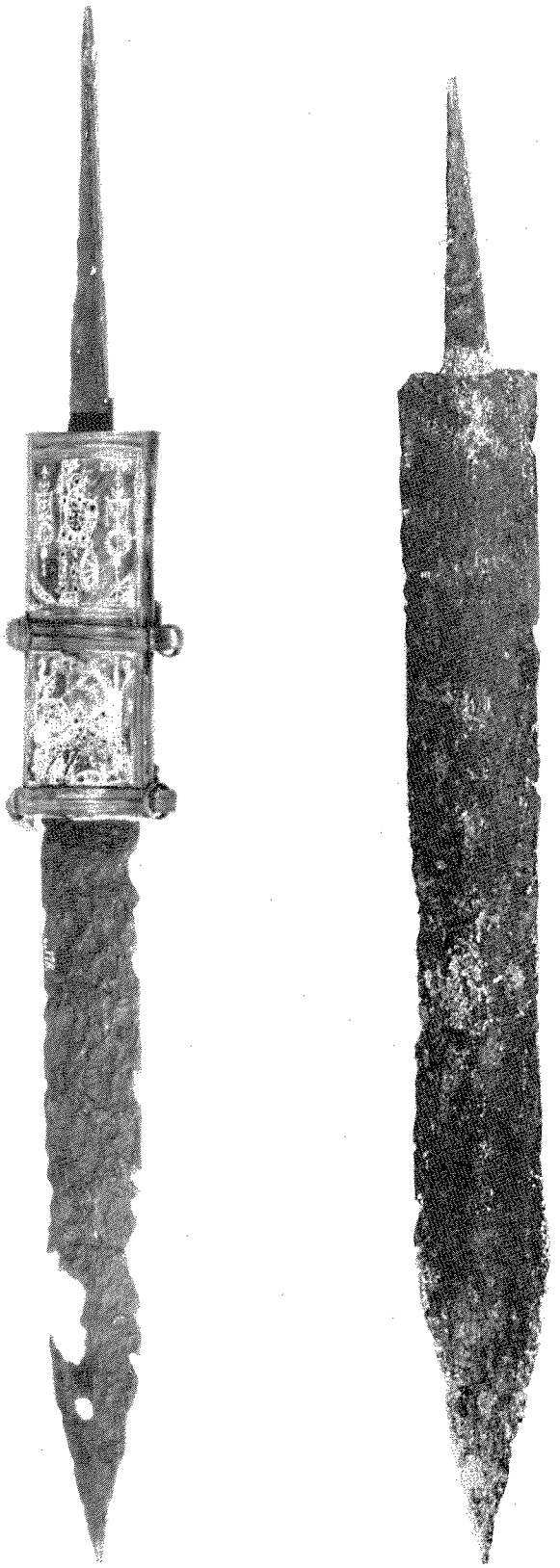


Fig. 85 *Gladii* from the Rhine at Oosterbeek, perhaps near site 23 (photo RMO, Leiden) and Arnhem (site 129). Scale 1:3.

lated to the major road from Noviomagus to Harenatium. The milestone (*CIL* XIII, 9162) was erected under Trajan, probably around AD 100, and perhaps indicates the actual road because it was found close to the early-medieval *via regia* in Beek, which is known to have been a Carolingian villa.[263] If, as is often true elsewhere, this *via regia* was originally a major Roman road, this has implications for the alternative trajectories of the road to Harenatium as outlined in paragraph 3.4 and would provide support for a direct route from Nijmegen over Beek, Zyfflich, Niel, and Mehr to Rindern.

The Roman barge from Druten (site 211) is, of course, evidence for the water route along the river Waal. Finally, the finds from site 125 should also be mentioned. In addition to the military aspects of the metalware dredged up there, it may well indicate that the Drielsche Veer existed as a Rhine crossing (ford or ferry) in Roman times.

Somewhat more to the east there are, incidentally, several other stray finds which testify to the military importance of the region around Meinerswijk. Only one of those, a *gladius* found at site 129, is included in the catalogue. The sword (fig. 85) is interesting because it has a graffito XV or, perhaps, L(egio) X, on the tang. A second find, the well-known gladius 'from the Rhine near Oosterbeek' (fig. 85) is more accurately datable because some decorated parts of the sheath were still attached to it. It has been dated to the second half of the 1st century.[264] It is quite probably related to (washed away from?) site 23 or site 126, but the find circumstances were too obscure for inclusion in the catalogue. A third stray find, an iron spearhead from Praets (c. 189.7/443.0) was reported in 1984.

An interesting and special category of isolated finds are the hoards. Two of these, sites 22 near Wolfheze and 26 in Arnhem, are indicated on Appendix 3 by a separate symbol. In the older literature there is sometimes talk of a third hoard north of the Rhine, in Quadenoord (probably site 21), but the available information, summarized by Byvanck (*ER* III, 164), only confirms that relatively many coins may have been found there.

There is, however, at least one other true hoard known from the eastern river area, namely, 50 denarii found in a pot near the slaughterhouse in Nijmegen (site 399).[265] In addition, the composition of the 17th-century coin-collection of the Smetii has been taken as evidence that it contained the coins from a hoard hidden around AD 270 and quite probably in Nijmegen; together with what was probably part of another Nijmegen hoard buried around the same time, there is thus evidence for two more hoards without an exact findspot.[266]

A collection of 121 denarii and antoniniani, allegedly found in Berg en Dal (around sites 424–433), should also be mentioned here. Daniels considered this a hoard,[267] but in view of the obscurity of the find circumstances, the composition of the collection (Republic–Postumus), and the way in which it was acquired, that interpretation is rather doubtful. Somewhat more reliable is yet another hoard from approximately the same area, namely the find of three silver saucepans discovered 'near the windmill of Beek' in June 1806.[268] The representation of the goddess Cybele on the handles has led to speculations about the possible religious nature of this hoard. In any case, the total remaining weight of 478.7 grams of fine silver (the original weight was probably more than 600 grams) indicates a considerable economic value.

Finally, three stray finds of aurei, which might also be considered a hoard by themselves because of their value, have to be included. The first is an aureus of Trajan found near Elst (site 146); the exact findspot of the second, an aureus of Nero, is unknown. It was reported to have been found near Herpen (c. 172/420), in the vicinity of site 488. A third aureus (also Nero), was recently found near Escharen.[269]

Without regard to the aurei and the saucepans, the hoards can be divided into two major chronological horizons. The two hoards from Nijmegen and the possible hoard from Berg en Dal are datable to the period around AD 270. Their interpretation in the light of historically known events is self-evident. That is not true for the

263 The *villa Bechi*; see Leupen 1977.

264 See Ulbert 1969a, 102 and 118–9, who disagrees with the conclusions reached by Braat 1967, 58–9.

265 Brunsting 1937, 175 and Daniëls 1950, 14–5.

266 Daniëls, *op. cit.*, 16.

267 Daniëls, *op. cit.*, 15–6.

268 See Daniëls 1955, 291 and Vermaseren 1956. A picture of the finds also in Van Es 1981, 260, fig. 201. The findspot is probably in square 191/426 on Appendix 3.

269 Boersma 1963, 55, no. 53d (Herpen) and Verwers 1983, 39–40 (Escharen)

hoards from sites 22, 26, and 399, which belong under the Severian dynasty. The latest coin from the hoard of (Renkum-) Wolfheze was probably minted in AD 197, the one from the Arnhem hoard between AD 222 and 235, and the one from the Nijmegen hoard in AD 208.[270] It is possible that all, but especially the first and the last, are comparable to a number of hoards dating to the same period from the northern part of the Netherlands.[271] These are regarded as the result of unrest and migrations in Germania Libera which, by the end of the 2nd century (after AD 180), also provoked unrest in the northern Netherlands and thus the hoarding of coins.

Whether or not our hoards can be attributed to the same phenomenon remains uncertain. Van Es, following Glasbergen, assumes that substantial repairs and rebuilding activities were carried out under Septimius Severus.[272] In itself this may indeed imply the presence of a threat, although others argue for an essentially peaceful period at the *limes* of Lower Germany that lasted a few decades longer.[273] This would provide a context for the Arnhem hoard but not for the other two, but perhaps the true context was a very different one.[274]

8.5 THE LATE-ROMAN PERIOD

A first glance at Appendix 4 (or fig. 63) is sufficient to confirm general expectations: compared to the previous maps, this one is virtually empty. The nearly 300 settlement sites of the Middle-Roman Period appear to have been reduced to a mere 76 inhabited settlements during the Late-Roman Period and of these not more than 8 probably and 8 only possibly.[275] The situation thus agrees with what can be found in any textbook, namely, decline and depopulation. Whatever else we can say about the Late-Roman Period in the river area, here or in later chapters, does not alter this basic fact. It is necessary to keep this in mind, because there are indeed quite a few other circumstances which need to be considered.

From a comparison of the gross numbers of middle- and late-Roman settlements, the first conclusion is that apparently their total number was reduced to 25 %, disregarding the size and nature of the settlements involved, as well as matters of continuity of occupation, geological change, and the like. Compared to other regions and notably to the 'nearly empty' western part of the Netherlands,[276] that is an extraordinary high number. For the south, no figures are available. They may be comparable or lower but certainly not higher,[277] although some regions in the loess areas in adjacent Belgium and Germany were also relatively densely settled.[278] Precise data for the northern parts of the Netherlands are also lacking, but even there the evidence for late-Roman settlements is meagre compared with the preceding centuries.[279]

Comparing the eastern river area to other regions is not without its risks. First, the chances of detecting sites in

270 Renkum-Wolfheze: Zadoks-Josephus Jitta 1960, 90; Nijmegen: Daniëls 1950, 15.

271 Van Es 1960, 33–5 and 1965–6, 63–4.

272 Van Es 1981, 46 and Glasbergen 1947, 305. Glasbergen's conclusion was based on inscriptions supposed to have been found in Katwijk (*CIL* XIII, 8828) and Roomburg (*CIL*, 8824). On the curious and complicated history of these inscriptions, see Bogaers 1976.

273 See e.g. Von Petrikovits, in Bogaers/Rüger 1974, 16, and 1978, 88–9. He does not assume any real hostilities until the reigns of Caracalla and Elagabalus and proposes two inscriptions from Vechten (*CIL* XIII, 8810 and 8811) as possible corroborating evidence. Although the conclusions in chapter 9 are necessarily provisional, it is worth noting that the substantial rebuilding in stone in Meinerswijk ('period 5') has also been dated to the same period. See also chapter 11.4.

274 See chapter 11, note 427.

275 Of the 76 settlement sites, 5 do not appear as such in Appendix 4. Sites 59 (see p. 167), 156, and 538 were definitely inhabited. No. 59 was omitted by an error, 156 and 538 because the information was not yet available when the map was printed. The same is true for the probable settlement site 536. Site 194 has been indicated as an isolated find (see below, p. 310) but there remains a (remote) possibility that it was actually occupied.

276 Bloemers 1983, 186, 191–3.

277 For the Province of North Brabant, the numbers are probably far fewer: see e.g. Boersma 1963 and Beex 1973. For Limburg, no comparable published data are available. The northern part of this province may well be comparable to North Brabant, the southern part to adjacent German (see note 278) and Belgian territory.

278 The detailed inventory from the Kreis Bergheim, for example, shows that the relative number of late-Roman settlements is certainly as high as in the eastern river area (Hinz 1969, *Taf.* 53 and 54): approximately one third seems to have survived, although this number is perhaps exaggerated in comparison to the river area because of Cüpper's rather late datings of some sorts of coarse (Niederbieber) pottery (cf. paragraph 6.2.5, p. 167). Most of the rims in *Abb.* 21 and many of those in *Abb.* 22 would not have led to a late-Roman dating by the present author.

279 See e.g. Van Es 1965–6, Van Es/Verlinde 1977, and Bloemers 1983.

general and thus also those of the Late-Roman Period are equal to those in some and much better than in most areas elsewhere. Second, there is the matter of the sensitivity of archaeological indicators: the recognition of late-Roman artefacts, and especially pottery fragments, for what they are. This depends on the availability of reliable typochronological series and these are better for wheel-turned than for hand-made wares, especially for the regions south of the Rhine where the study of late-Roman hand-made pottery is still in its infancy.[280] Unless clearly identifiable northern forms are available, there is thus an almost complete dependency on wheel-turned wares, most or all of them imports. It is evident from the data presented in chapters 5 and 6 that the eastern river area did not lack those imports. In comparison to quantities of middle-Roman wheel-turned wares, those of the Late-Roman Period are small. But compared to other regions, they are very large indeed.

In view of these arguments, the question arises whether the relatively high number of settlements in the eastern river area should be explained by a relatively large population during the Late-Roman Period or by the fact that the inhabited settlements can be better recognized as such because of the nature of the finds, or, perhaps, by both. This is a matter that will be returned to below and in chapter 12, but it is clear that different socio-political and military situations will have led to differences between areas, both in the size of the populations and the flows of goods to those areas.

The available evidence at least suggests that the eastern river area was relatively well supplied. The actual numbers of settlements must even have been higher than the maximum of the 76 mentioned earlier. The reasons for that situation are summarized in chapter 4.2 (p. 92): late-Roman sites are underrepresented because they have in fact only been recognized as such on the basis of hard evidence, the presence of recognizable late-Roman wheel-turned potsherds. In addition to what has been said there and in the discussion of the finds in chapter 6, it is also useful to repeat here that several kinds of late-Roman pottery are simply unrecognizable as such when one is dealing with small fragments. Some of the coarse ware types from the late-3rd and early-4th centuries are very difficult to distinguish from their typological predecessors (see also note 278). Late-Roman terra nigra-like wares may also not be recognized unless typical rim or base fragments (for example, of the cups Chenet 342: fig. 39) are present. Fragments of colour-coated ware, with the exception of red-brown slip ware, are yet another example.[281]

Without implying anything directly about population densities, the conclusion must be that the minimum number of late-Roman settlements is certainly one quarter of those in the Middle-Roman Period. Given the estimates for middle-Roman settlements (p. 246), this would mean that the total number of late-Roman settlements in the eastern river area can be estimated at a minimum of 90 and a maximum of 125 or slightly higher, but the uncertainties in such an estimate are considerable. It is, for example, not at all impossible that the percentage of surviving sites on Pleistocene deposits is higher than on the Holocene river deposits, where archaeological reconnaissance is much better.

On the other hand, considerations like that are somewhat superfluous because other indications, such as pollen diagrams, confirm that the total number of sites cannot have been all too large. It is worth while, however, to note that the general distribution of sites over the landscape is at least somewhat different from the preceding period, because they are more evenly spread over the Pleistocene and Holocene deposits.

This does not imply that they are more evenly spread over the entire region as well. With the exception of the sites north of the Rhine and along the evidently still existing route to the north from Randwijk/Wageningen towards Ede, there seems to be a clustering within a 15 km radius from Nijmegen. It can hardly be a coincidence that the formerly densely settled areas in the west, notably around Kesteren and Druten, have yielded no evidence for late-Roman habitation despite intensive fieldwork. Almost all late-Roman settlements are either continuously inhabited or reoccupied middle-Roman sites, which makes it even harder to believe that no late-Roman material was found on the western sites unless it indeed never existed. The general situation is in fact the same as that in the central part of the Dutch river area.

Although new regional inventories will surely modify this picture, the above conclusion is still in agreement with Modderman's findings.[282] The 4th-century habitation, or – to be more precise – the evidence for 4th-cen-

280 Compare e.g. Van Impe 1983, 88–90 and 93.

281 See e.g. Gose 1950, nos. 200–211.

282 Modderman 1949, 69–70, 79 and 1951, 46–9, a summing-up in 1955, 34. Inventories of the central part of the river area are being prepared by C. Blommesteijn and W.J. van Tent (see Van Tent 1978a, 212, esp. fig. 10, 8, and 1984)



Fig. 86 Nijmegen: late-Roman sites. For site nos., see chapter 5 (catalogue): 1 contour lines, 2 railway, 3 excavated area, 4 topographical coordinates, 5 buildings, 6 ditch around the Valkhof, 7 inhabited area; investigated and/or many finds, 8 inhabited area; not investigated and/or few finds, 9 double ditch around investigated and uninhabited area, 10 cemetery; investigated and/or many finds, 11 cemetery; not investigated and/or few finds, 12 road. Scale 1:10,000.

tury habitation, thus quickly disappears towards the west. It is barely present in the central Dutch river area and virtually absent in the western part of the Rhine-Meuse delta.

The concentration of sites in the eastern part of the eastern river area is an interesting phenomenon in itself, but it is not the subject of the present discussion, which should provide some insight in the different forms of settlement.

Although perhaps less clearly than in the preceding periods, it is still possible to separate military and civil settlements. These will be discussed in the following paragraphs.

8.5.1 Military Settlements

The organization of the Roman army was changed during the 3rd century to cope with the changing political and military circumstances. These changes[283] were, of course, reflected in the distribution as well as the structure of military establishments. The most important trends in the development were, first, an increased mobility of the troops (more cavalry) and the creation of

283 See Luttwak 1976 for a general analysis, Hoffman 1969 and Jones 1973 for historical accounts, and Von Petrikovits 1978, chapter 14 for a detailed account regarding Germania Secunda. See also chapter 12.

mobile armies normally not stationed at the frontier but in the interior. Second, the creation of heavily fortified, siege resistant, and self-contained strongholds at the frontier occupied by increasingly static forces (*limitanei, ripenses*) and of various sorts of other strongholds, as well as the fortification or abandonment of towns in the interior. Important concomitant developments were a change in offensive and defensive armament and the disappearance of differences between military and civil settlements, which had for so long been scrupulously – though not always succesfully – kept separated.[284] In addition to fairly clear-cut cases it is thus sometimes difficult to decide whether one is dealing with a fortified town with a garrison of soldiers or with a military fortification. Fortunately, such a distinction is not vital because the function of a settlement within the late-Roman system of defence-in-depth should not be measured by 1st- or 2nd-century criteria.

While the field armies of the 4th-century were either moving around or kept in garrisons inside towns in the hinterland, the forces at the frontier became increasingly immobile from the late-3rd century onwards; they were farmers with a vested interest in the land as much as soldiers. Compared to the East, this policy was apparently not as thoroughly effectuated in the western part of the Empire, and in particular not in Germania Secunda.[285] For example under the tetrarcy (Diocletian and successors), all army units were probably still stationed on the frontier. Nevertheless, in the 4th century the same policy was also practised in Germania II and *ripenses* were living with their families in heavily fortified strongholds. Their function was no longer to prevent or to counterattack, but to defend, to hold their positions, and to delay the enemy. This led to the construction of heavily fortified strongholds on the frontier as well as further inland, as hard-points in the defence-in-depth system. Incursions along major roads were prevented or delayed by road-forts, either newly built small forts (*burgi*) or fortified and contracted settlements (see fig. 143).

The development of late-Roman fortification techniques has been amply documented and need not be treated in detail here. In general, it can be said that there were various improvements, such as thicker walls, cleverly constructed gates and protruding towers, and larger and deeper ditches. Wherever possible, new strongholds were located in an easily defensible position.

In the area covered by this study, there are quite a few settlements which were, or are likely to have been, part of the late-Roman defence structure of Germania II (see also chapter 12). Presumably the largest site is that in Nijmegen.

Site 403 Nijmegen–Valkhof

In the preceding parts of this chapter, the early- and middle-Roman occupation of this site has already been discussed. During the Late-Roman Period it seems to have been the only remaining inhabited site in Nijmegen. Although a few finds indicate that some people were probably still living at site 399, the town – Ulpia Noviomagus – had ceased to exist as such before the end of the 3rd century. The inhabitants who remained in Nijmegen almost certainly moved close to the defensible Valkhof site. As pointed out earlier (p. 233), this site is the only one that combines a high, easily defensible position with a location at the river Waal: all other formerly inhabited sites were either in a more open and low-lying position or very high up the ice-pushed ridge and rather far removed from the river.

Although many questions about the late-Roman history and structure of site 403 still need to be resolved, recent excavations have finally provided some really useful data.[286] A simplified plan of site 403 and the related late-Roman cemeteries (sites 405, 410, and perhaps 413) is provided in fig. 86. It only presents the 4th-century features; although some traces of middle-Roman and late 3rd-century ditches have also been discovered, these cannot yet be interpreted.

During the first half of the 4th century, the Valkhof was protected by two systems of ditches. The largest perimeter is indicated by two parallel, more or less V-shaped ditches, each more than 4 m wide and 1.5 m deep, encompassing an area of approximately 500 m long and maximum width of approximately 300 m. These ditches, which cannot have remained open for very long, fell into disuse at the latest shortly after AD 350 as is indicated by a coin series terminating with three FEL TEMP REPARA-

284 See MacMullen 1963, esp. chapters 4 and 6; Von Petrikovits 1978, 239–40.

285 See the remarks by Luttwak 1976, 190 and Von Petrikovits 1978, 221.

286 A full publication will follow in due course. Overviews of the results until 1979 are provided by Wynia 1979 and Thijssen 1980. Provisional reports on later excavations can be found in *JROB* 1979, 49; 1980, 33–4; 1981, 33; 1982, 34; 1983, 46–7; 1984 (in press), chapter 2D, and in Bloemers 1983, 192–3.



Fig. 87 Late-Roman ditch around site 403 (Valkhof) at Nijmegen.

TIO coins from the upper part of the fill. No traces of a wall were discovered and, although not impossible, it is improbable that a substantial wall ever existed along these ditches.[287]

The inner ditch encompasses a maximum area of c. 5 ha. It is very large (see fig. 87), in one section 14–15 m wide and at the point of the V-shaped section 5.6 m deep. It contained among other material blocks of limestone from an early-Roman column (fig. 67), presumably used as *spolia* in 4th-century constructions. Unfortunately, no traces were discovered of these constructions, but in view of the very considerable post-Roman disturbance at the site that is hardly surprising. It is thus also not surprising that in the few instances where traces of the wall along the ditch could have been found none were observed, but there can be little doubt that there was indeed a wall. The coin evidence from the large ditch indicates that it, too, fell into disuse shortly after AD 350, although the coin series of the Valkhof site in general (see figs. 141 and 142) reaches to the end of the 4th century.[288]

There is as yet no clarity about the relation between the two ditch systems. The outer double ditch may represent a first line of defence (for example, to keep away siege engines and artillery), but it could also precede the inner ditch and represent either a first attempt at fortifi-

287 The inside of the inner ditch did yield some limestone, tuff, and other building material (*JROB* 1981, 33) which could – but not necessarily – point to some sort of construction.

288 See Daniëls 1921, 29–31; Haalebos 1976, 204–5.

cation of the site or a temporary safety measure while the main defences were being built. Both interpretations agree with the fact that the double ditch was open for only a short period of time and was probably not accompanied by a wall, at least not by a well-built and heavy one. In addition, clear indications of habitation between the outer and inner ditches are lacking, although the current state of the investigations does not permit definite conclusions in this respect. A fair amount of late-Roman material was salvaged from the area between the ditches before World War II.[289]

Whatever the real situation may prove to have been, it is clear that the Valkhof was an important and large fortification at least during IVA. On the basis of the cemeteries, the population has been estimated at a minimum of 600 people.[290] Although its role in the late-Roman defence system need not be doubted, there is additional evidence in the form of tile-stamps of a *numerus Ursariensium* (fig. 138) and a, presumably late-Roman, stamp of a *legio XXII*. The first unit was probably formed in IIIB and is attested from various forts, including site 460 (Qualburg). The *legio XXII*, sometimes with the title C(onstantiniana) V(ictrix), is known to have engaged in building activities at several frontier-fortifications along the Rhine under Constantine and his sons.[291] All the available evidence thus points to the late 3rd and especially the first half of the 4th centuries for the defence works discovered so far. But the occupation did not terminate afterwards, as is also indicated by the evidence from the cemeteries 405 and 410.

Site 460 Schneppenbaum-Qualburg

The evidence from Qualburg need not be discussed in detail here. The limited excavations in 1937 have provided clear evidence of a late-Roman fortification, presumably to be identified as Quadriburgium.[292] The main feature was a ditch c. 16 m wide, probably similar to the Valkhof ditch, although it could not be completely excavated. The ditch was no longer open after c. AD 300. No wall was excavated, but it evidently belonged to a late 3rd-century fortification, probably occupied by the *numerus Ursariensium*.

The occupation continued throughout the 4th century (and later) which, although no features have been excavated so far, agrees with the identification as Quadriburgium, one of the series of frontier fortifications, according to Ammianus Marcellinus reconstructed by Julian in Germania II and the northern part of Germania I.

Site 126 Meinerswijk

The evidence concerning the late-Roman fortification in Meinerswijk, also the result of limited excavations, is presented in detail in chapter 9. So far, indications for occupation during the late-3rd and early-4th centuries are lacking. The site was definitely occupied during the second half of the 4th century and later, and there is evidence for construction work in IVB, using building material from the earlier *limes* fort. No late-Roman ditch or tile-stamps have been discovered so far.

As was the case at Qualburg, the (renewed?) activities in the late 4th century have been attributed to the refortification works undertaken by Julian in AD 359. Although, as for Qualburg = Quadriburgium, incontrovertible evidence has not yet been found, the site in Meinerswijk has been identified as Castra Herculis, the westernmost fortification mentioned by Ammianus.

Site 499 Cuijk

The fourth and last of the major late-Roman fortifications in the eastern river area is Cuijk, undoubtedly the Ceuclum of the *Tabula Peutingeriana*. From north to south, it measures 110 m (inside the walls), from east to west the distance is probably similar but it cannot be established because the eastern part of the site was eroded by the Meuse.

The fort, evidently a stronghold intended to secure the Meuse-crossing of the road from Tongeren to Nijmegen, was built under Constantine and reconstructed or repaired later, presumably under Valentinian I. The first phase had an earth-and-timber wall 4–5 m wide, replaced in the final phase by a stone wall only 1.5–1.9 m thick with projecting half-round towers and buildings on the inside against it. A deep and wide ditch is absent in Cuijk.[293] Instead, the fort was surrounded by two par-

289 See Daniëls 1955, 47–101.

290 Bloemers 1983, 192.

291 See Von Petrikovits 1971, 182–4, Hoffmann 1969, part 2, 147–9, and Wynia 1979, 65, *Abb.* 72 and 73.

292 See Von Petrikovits 1937 and Horn, in Bogaers/Rüger 1974, 96–8.

293 Actually, it is not lacking but the large rounded ditch (Bogaers 1967, fig. 6.14), *which otherwise contained only (especially late-)Roman finds*, could be dated to the Middle Ages by one single find in a secure stratigraphical position (pers. comm. J.R.A.M. Thijssen).

allel V-shaped ditches only c. 2 m wide at a distance of 5–7 m and 14–15 m from the wall. The end of the occupation of the fort is unknown, but it may have been shortly after AD 400. Habitation of the site as such, however, continued (uninterruptedly?) into the Early-Middle Ages.[294]

Of the major late-Roman military sites discussed so far, two (126, 460) were frontier fortifications. A third (403) may have had a similar position at one time and have functioned as a road-fort at another. The fourth (499) was primarily a road-fort. The remaining sites are either rather uncertain as late-Roman military strongholds (123, 135, 194) or of much smaller size (391–466).

Site 466. Asperden
The *burgus* in Asperden may be interpreted as a watchtower. The published plan[295] indicates a wooden construction and later rebuilding in stone of a central tower (15.6 × 15.6 m) with massive walls, surrounded by a c. 1 m thick wall of 40 × 40 m with round, half-projecting towers and two small V-shaped ditches 2.5 m wide and nearly 2 m deep. The structure which, as a road fortlet, controlled a route along the river Niers, was constructed under Valentinian I and existed until shortly after AD 400.

Site 391 Malden-Heumensoord
The *burgus* at Heumensoord, which may have been a former *statio beneficiariorum consularis*, was probably also a watchtower. The published evidence[296] indicates two phases, a wooden construction surrounded by an earth-and-timber wall, and later perhaps a stone building and wall.
The ditch (2–5 m wide and a maximum depth of 2 m) enclosed an area of only 33 × 33, later reduced to 22.5 × 22.5 m
As a road-fortlet, the *burgus* was undoubtedly intended to control the route from Cuijk to Nijmegen. It must have existed already in the late 3rd century and functioned throughout the 4th century.

Site 123 Driel-Oldenhof
The Oldenhof is situated at a short distance to the west of the probable *limes* fort in Driel (site 117/8). In contrast to the latter site, which did not survive the 3rd century, the material from the Oldenhof indicates more or less continuous occupation, but the vast majority of the finds is datable to the 4th (especially late 4th) and 5th centuries. Much of it is described and illustrated in chapter 6.
These finds, and the location of the site, have led to the proposal that we might be dealing with a late-Roman stronghold.[297] The most obvious reasons for a shift of the centre of the occupation in Driel from site 117/8 to 123, or rather a concentration of all remaining habitation at the Oldenhof, are indeed military considerations. For example, a watchtower would be more useful if it were closer to the Rhine. However, as already indicated in the earlier publications, definite clues as to the military nature of site 123 are lacking. The finds were salvaged by amateur archaeologists when the Oldenhof was being built over in 1973 (no Roman Period features could be observed). They indicate that the inhabitants were amply supplied with wheel-turned pottery throughout the 4th and 5th centuries, but they do not include definitely late-Roman military artefacts. The presence of a late-Roman watchtower or fortlet in Driel thus remains just a possibility, with regrettably little chance for further investigations in the foreseeable future.

Site 135 Huissen
As already mentioned in chapter 8.4.1, the material from Huissen has all been found in a secondary context. It has been assumed that the finds were transported to Huissen by the river Rhine from site 194 or by human agents from sites 194 or, perhaps, 126.
There is, however, no certainty in this respect, and therefore a middle-Roman military settlement in Huissen could not be ruled out completely. That is even more true for the Late-Roman Period, because a substantial part of the finds from Huissen is late-Roman. If they have been transported to Huissen in the Middle Ages from Meinerswijk, there is no problem. If the material originated from the Loowaard, which is more likely, there is a difficulty because site 194 has not yielded clear evidence of occupation in the Late-Roman Period. An alternative explanation could, therefore, be that the

294 The coin series ends in AD 402: Boersma 1963, 33–8. See also fig. 141. Apart from some general remarks in Bogaers 1966 and 1967, the finds from the excavations have never been published.

295 Hinz/Hömberg 1968; see also Rüger, in Bogaers/Rüger 1974, 99–100.

296 See Bogaers, in Bogaers/Rüger 1974, 81–3 for a summary.

297 Willems 1980a, 343–4 and 347, 1981; Bogaers 1981a, 20.

material was originally transported to Huissen not in the Middle Ages but during the Late-Roman Period to build a *burgus* there from the remains of the fort in the Loowaard. A reason for such an enterprise could be that the shifting Rhine channel had turned Huissen into a site better located for military purposes or, less probably (see below), that the fort in the Loowaard had already been partly eroded.
All such considerations are, of course, speculation and will be hard to prove. The evidence from sites 135 and 546 testifies only to the secondary position of the Roman finds. The presence of an auxiliary fort some 3000 m upstream in the Loowaard is a good reason to consider a middle-Roman fort in Huissen very unlikely. For the Late-Roman Period, such an indirect clue is lacking, which does not strengthen the case for a late-Roman stronghold but at least it does not weaken the already feeble arguments in favour of it.

Site 194 Loowaard
As mentioned above, evidence for occupation of the fort in the Loowaard in late-Roman times is absent. The one or two fragments of pottery dating to the late-3rd or 4th centuries have been interpreted as stray finds. In view of the large quantities of material available, the possibility of a late-Roman stronghold at site 194 can be ruled out. Indications for reoccupation from the 5th century onwards are, however, present, which indicates that the site as such was probably not eroded until the Late-Middle Ages.

In addition to the sites mentioned so far, it is useful to examine some other possible fortifications. Indications at site 12, to be discussed in paragraph 8.5.3 (p. 317–8), have already led to the suggestion of a stronghold in the vicinity. In addition, one might argue for a continued existence of the hypothetical frontier fort in Randwijk (18). Although the distribution pattern on Appendix 4 could be interpreted as evidence, it should be remembered that its existence could only be deduced from the necessity for a military post there in the middle-Roman system of preclusive defence. In the late-Roman system of defence-in-depth it may have been useful, but it is not essential. Thus, a continued existence cannot be accepted without additional evidence that it was really there.
Two other possible strongholds are discussed in paragraph 8.5.2. First, there is the *vicus* in Wijchen (315) which was, relatively speaking, still thriving and surely not without defences. As a major settlement on a branch of the Meuse and at the beginning of a route to Nijmegen (cf. § 3.4.2, p. 69), a military function is not unacceptable. Second, a former villa near Ewijk (239) may have been fortified in some way and have served as an observation-post along the Waal, at the same time blocking the road over the southern levee (cf. below, p. 315 and chapter 12).
Admittedly, the above discussion shows only one thing clearly, namely, the desperate need for excavations at some of these sites. They have, nevertheless, been plotted on fig. 63. The resulting picture is not without its problems, but it shows at least a potentially fruitful explanation of the relatively dense occupation in the eastern part of the eastern river area, which so markedly contrasts with the entire area further to the west. That occupation may well have survived only because it could depend on an early-warning system and on defensible refuges in time of need.
Fig. 63 is, however, not entirely satisfactory. It gives an impression of Nijmegen lying as a spider in its web of outposts, which is probably not true. Even given the paucity of the data, the essential differences between phases from the late-3rd to the early-5th centuries should be taken into account. A modified picture is therefore presented in figs. 88a and b, which at least discriminate the phases before and after c. AD 350. They are based on the evidence from the sites discussed previously, the excavation of site 12, and the finds from sites 239 and 315 discussed below. The nature of much of this evidence does not permit exaggerated claims as far as the implications are concerned: it has, indeed, been pushed to its extreme. If the sites are indeed what they are suggested to be, then Nijmegen was at first not so much a spider in a web but itself temporarily *de facto* a frontier fortification with outposts only at the direct routes of access to it. After c. AD 350, and preferably after AD 359, the situation is then changed into a triangle of major fortifications at sites 126 (Castra Herculis), 460 (Quadriburgium), and 499 (Ceuclum), with outposts at the routes of access into the area in between.
However tentative fig. 88 may be, it is important to realize that the main elements in the models (the data on sites 126, 403, 460, and 499) are the most secure although, of course, in no way indisputable. The outposts, whether beyond all doubt (391, 466) or uncertain (12, 123, 239, 315), are important in order to detail the reconstruction but they are not crucial for a general interpretation. That interpretation should necessarily also in-

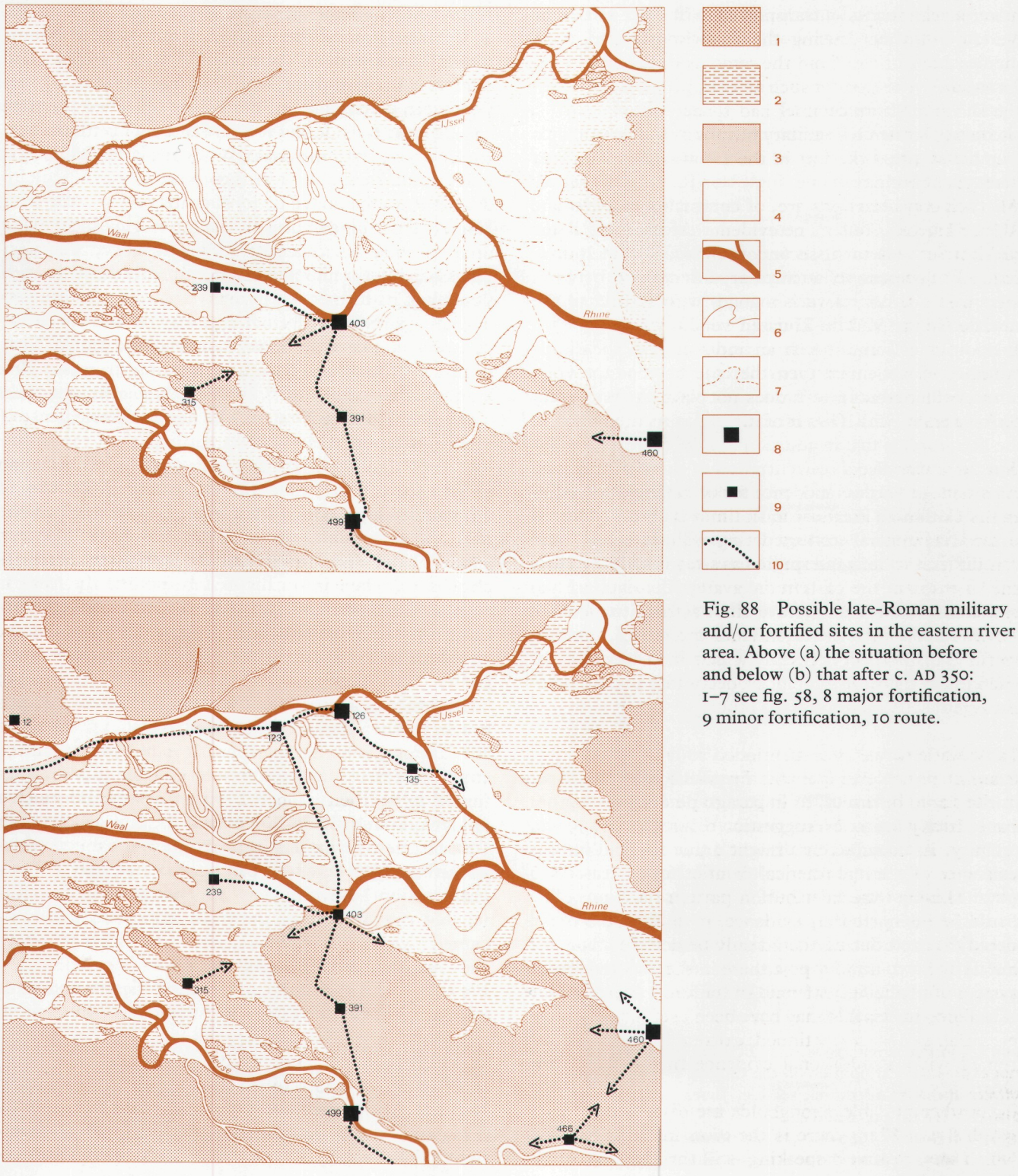


Fig. 88 Possible late-Roman military and/or fortified sites in the eastern river area. Above (a) the situation before and below (b) that after c. AD 350: 1–7 see fig. 58, 8 major fortification, 9 minor fortification, 10 route.

volve developments outside the eastern river area, and therefore further discussion will be postponed until chapter 12.

8.5.2 Civil Settlements

Compared to the civil settlements, the available evidence on military sites is very good indeed. For the entire area south of the Rhine not one single late-Roman civil settlement has ever been excavated. The situation north of the Rhine is better, but in general the information about late-Roman settlements is very meagre. This, of course, seriously hampers an attempt to differentiate between them or to detect relevant changes with respect to the situation in the Middle-Roman Period.

In addition to some specific changes, such as the disappearance of settlement types like the town or the villa, other developments may have taken place. An important consideration, which has often been commented upon in the literature, is that in addition to a decrease in population there was also a concentration of those people who remained in larger and preferably defensible settlements. Although there are indications that this occurred at the level of rural settlements as well (see below), and it is difficult to imagine single farmsteads or loose clusters of them in the eastern river area, the data are too limited to allow a clear picture of the actual situation.

In accordance with the legend on Appendix 4, it seems useful to distinguish between the largest and, because of the finds or their position in the preceding period, more important settlements and the others.

Large settlements

It is not possible to identify a regional centre without running into difficulty, or indeed to determine whether or not such a centre even existed. Although site 399 was probably not completely deserted, there are no indications of a substantial occupation and thus no reasons to assume that it retained any of its former functions. The erosion of its northern part by the river Waal may have already started during the Late-Roman Period and the rest of the town may have been more an industrial settlement, cannibalizing itself and providing a cheap source of building material for the region.

If anywhere, a regional centre should be located at site 403, on and around the Valkhof. It is logical to assume that at least initially all remaining central functions and officials converged there. It was the largest surviving settlement in the region and there is no objection (cf. note 284) against considering it a fortified civil settlement as well as a military stronghold. While the latter function is easily understandable it is, however, more difficult to discuss its function as a regional centre.

It has been assumed that a *civitas Batavorum* still existed until the mid-4th century.[298] This is indeed the most plausible interpretation, but it can only have been a shadow of its former self in many respects: western and northern parts of the former area may not, or not continuously, have been under the control of the centre. That centre itself may well have been a *civitas* in its late-Roman meaning of town,[299] although a smaller one than Tongeren, Köln, or Xanten. The new archaeological evidence as outlined above does support the proposition that the Valkhof and surroundings were indeed a centre and the site of a major fortified settlement until the mid-4th century after which the *civitas Traianensium* (Xanten) and a possible *civitas Batavorum* no longer existed.[300] The distribution of late-Roman settlements, clustered as they are in the eastern part of the eastern river area around Nijmegen, may to some extent confirm this proposal. To some extent, because it is only of limited value due to the dating evidence for individual settlements.

That evidence is usually not accurate enough to indicate precisely when they were occupied, which, for the present purpose, is a vital question. It is possible to differentiate the schematic datings of chapter 4 somewhat further by indicating the (few) sites that were almost certainly continuously occupied from the late-3rd into the 5th centuries. Those that were definitely inhabited in the late-4th and 5th centuries can also be indicated without problems but, unfortunately, it is not possible to reach reliable conclusions on the most important question, namely, which sites definitely were and which definitely were not occupied during IIId-IVA.

For the second half of the 4th century, there is no longer evidence that anything like a regional centre still existed.

298 Von Petrikovits 1978, 217. Bogaers (1960–61, 311) does not express a clear opinion on the subject. For late-Roman *civitates* and administration, see e.g. Jones 1973, 712–31. See also chapter 12, 434–6 and 442–4.

299 Cf. Bogaers 1960–61, 311 and note 264.

300 That is at least the conclusion usually drawn (e.g. Byvanck 1943, 619 and Von Petrikovits 1978, 217) from the fact that late 4th-century sources (Ammianus Marcellinus XV, 11, 7 and *Notitia Galliarum*, VIII = *ER* I, 561) only list the *civitas Agrippinensium* (Köln) and the *civitas Tungrorum* (Tongeren) and no longer mention either Nijmegen or Xanten for Germania Secunda.

Site 403 was still occupied and therefore almost certainly fortified in some way, but the excavated ditches were largely filled up and no other features have been discovered so far. Although, of course, still in the centre of the cluster of inhabited settlements which, for this period, can be more confidently identified, its importance may well have been reduced to that of a road fort. In any case, the archaeological evidence seems to agree quite well with the historical evidence: Nijmegen is not listed among the settlements refortified by Julian in AD 359, as were Quadriburgium (site 460?) and Castra Herculis (site 126?).[301]

Both these two sites may have been large settlements in addition to military strongholds, but the archaeological evidence is too limited to draw any conclusions in either case. They may also have functioned as local – and, under the circumstances, not necessarily secondary – centres for the areas around them. For site 126 there is, however, an alternative in site 123. Especially if that should turn out not to have been a military establishment, it must have been a fairly important civil settlement although it may not have been very large. The size of the ancient settlement soil and the distribution of the finds (excluding late-medieval material) indicate a settlement of perhaps 4 ha there, but the part inhabited during the Late-Roman Period was probably smaller than that. The total lack of late-Roman finds from Elst, while not excluding some hitherto undiscovered form of continued occupation, indicates that this former *vicus* and secondary centre for the Overbetuwe (the region between Rhine and Waal) no longer functioned as such.

The same is not true for the former *vicus* and secondary centre for the region between Waal and Meuse, site 315 in Wijchen. The quite considerable amounts of late-Roman pottery, coins, and other artefacts, indicate continued habitation over a fairly large area. Although findspots like the 'Tienakker' and 'Oude Ravensteinseweg' have, unfortunately, never been excavated, there is ample evidence that the settlement continued, perhaps only in a relatively modestly reduced form. The intensive, but necessarily incidental, efforts of local amateurs yielded no convincing traces of a wall, but there is every reason to believe that several stone buildings were still used. A golden crossbow-fibula from site 316 (fig. 89) is another indication for the status of the settlement, even though it is a stray find and perhaps to be interpreted as a hoard. Roman crossbow-fibulae (*drieknoppenfibulae, Zwiebelknopffibeln*) are an indication of office (*Amtstracht*) and golden specimens are restricted to very high officials indeed.[302] The Wijchen fibula, however, is not Roman but a Germanic, so-called *Stützarmfibel*.[303] The close relationship between the two fibula-types[304] may point to a similar symbolic function.

Like Wijchen, the former centre for the area south of the Meuse, in Cuijk (site 499/500), probably continued to function as such. The fortified site 499 alone may have measured some 1.2 ha, but there was also habitation outside its walls in (the remains of) the former *vicus* of Cuijk (site 500).

In addition to the settlements mentioned so far, there is yet another large settlement, namely, Ede–Veldhuizen (site 1) located north of the Rhine. It has not been indicated as a large settlement on Appendix 4, because such an indication would suggest that site 1 is also some sort of central place like Wijchen, which is possible but not at all certain.

Ede–Veldhuizen is a large example of the nucleated villages with a considerable degree of internal structuring discussed earlier (p. 283–4). The total excavated area amounts to 7.5 ha and, although publication of the excavations and an analysis of the various phases still has to be awaited, the main period of occupation and therefore presumably the largest is the late-Roman settlement. It is possible that Ede–Veldhuizen shows a development towards a hierarchically organized community such as comparable settlements in Wijster, Flögeln, and Feddersen Wierde. These settlements, to which perhaps also belongs the only partially excavated settlement of Bennekom–Achterstraat (site 7), present a problem of interpretation.

On the one hand, they may be the central places of the increasingly complex society developing beyond or at

301 As already observed by Haalebos (1976, 205), it is almost inconceivable that Julian should not have devoted attention to Nijmegen if Castra Herculis had been located on the Waal (as Bogaers 1968: Castra Herculis = Druten?). Nijmegen would then be located *between* Quadriburgium and Castra Herculis. The identification of the latter place with site 126 removes these difficulties (see also chapter 9).

302 See Van Buchem 1966 and Keller 1971, esp. 171–3 for an overview and further references.

303 The find is discussed in detail by Haalebos, in press.

304 Böhme 1974, 51–2 and 191.

the perimeter of the Roman empire. On the other, it is possible that the unstable political and economic situation in the Late-Roman Period led to an increased need for self-sufficiency and concentration (and thus to the need for a more hierarchically organized society), which led to such large settlements being the normal settlement type instead of the exception at the top of a settlement hierarchy.

At the moment, it is still difficult to decide which of the two situations conforms best to the actual situation. Research over the past decades in the northern part of the Netherlands and Germany has, however, yielded more arguments in favour of the last interpretation. A settlement like Wijster, for example, shows a trend towards self-sufficiency, especially in Period II (c. AD 225-300),[305] and of considerable growth during the late-3rd and 4th centuries which has at least partially been attributed to the arrival of immigrants rather than population growth only.[306] In addition, there seems to be a lack of really small settlements in the same period, but this is quite possibly misleading.[307] More important, however, are the findings for the subsequent Early-Middle Ages, which indicate again less centrally organized villages[308] while smaller settlements are not lacking.

There are, thus, reasons to believe that there was a general trend towards large, nucleated villages with a visible hierarchical social structure, starting in the 3rd and diminishing again in the 5th centuries.[309] This does not necessarily imply that they disappeared completely nor that we may expect to find only those types of settlements. Consequently, it does not imply that a village such as Wijster and perhaps others, such as Ede–Veldhuizen, are nothing special. It only means they were perhaps less extraordinary than might be assumed, being the most pregnant examples of a general trend that lasted for about two centuries.

In this way, the village of Ede–Veldhuizen can be evaluated against the background of the (especially Late-) Roman Period settlement on the 'free-Germanic' sandy soil of the northeastern Netherlands.[310] With the poss ble, though not very probable, exception of site 123 Driel, it does not seem to be paralleled by any of t large settlements south of the Rhine. The excavated se tlements there do not even look similar. As far as o limited knowledge allows us to say (see below), the cha acteristic large farms with house and byre under one ro and situated on separate yards within the settlement (fig. 81) do not even occur there. Whatever further hyp theses one may seek to prove, they should thus take in account that perhaps the Rhine continued to be a rath sharp divide between types of habitation. In one way, was even sharper than before: it is hardly imaginab that such large settlements as Ede–Veldhuizen and pe haps some of the other sites could have existed c rather, would have been allowed, so close to the Rhine the preceding period.

Other settlements

Some of the settlements north of the Rhine were partia ly excavated, but only site 7 (Bennekom), which is con parable to Ede–Veldhuizen, was at least adequately i vestigated. The excavation of site 17 was a by-product the excavation of the cemetery (site 16) in Wageninge and yielded only some uninterpretable pits and pos holes. Similarly, the small excavation at site 21 (Quade oord) produced some Roman Period finds and feature but the investigated area appeared to be mainly late-m dieval.

The same dismal picture is offered by the results of exc vations south of the Rhine, in those settlements whi are considered to have been inhabited during the Lat Roman Period. There are only 5 of those, namely, nc 75, 109, 143, 150, and 399. The excavation at site 1(was so fragmentary that it has never even been pul lished,[311] and the work at site 399 has so far only pr duced late-Roman finds, not features.[312] That leav sites 75 (Zetten–De Hoge Hof), 143 (Bemmel–De He vel), and 150 (Ressen–Kerkenhof), none of whi

305 Van Es 1967, 406–7; Willems/Brandt 1977, 78–80; Bloemers 1983, 191.

306 E.g., Van Es 1967, 538–9.

307 The settlements of Denekamp and Dalfsen in the Province of Overijssel, for example, do not seem to be 'very extensive settlements in the style of Wijster' (Van Es/Verlinde 1977, 41). See also Waterbolk 1979, 5 for similar remarks on the settlements of Peelo, Noordbarge, and Rhee in the Province of Drenthe.

308 Waterbolk 1980, 199.

309 The latter development undoubtedly being related to general discontinuity of settlements (though *not* of habitati in general) shortly after AD 400. See Van Es/Verlinde 197 38–41 and Waterbolk 1979, 12–3.

310 Cf. Van Es 1973.

311 Braat 1937, 23.

312 The same is true for the largely excavated settlement Wijk bij Duurstede- De Horden (pers. comm. W.A. van Es).

yielded interpretable late-Roman house-plans, although a renewed analysis, especially of site 75, might still produce some clues. As mentioned above, large houses of the type common in the area north of the Rhine have not been found.

It is clear that under these circumstances a discussion of late-Roman settlements can hardly offer any useful new data. None of them is likely to have been very large, but even the question whether they might represent a more concentrated form of habitation cannot be answered. For all we know, the habitation may also have had a rather temporary and makeshift character, with people living there to till the soil but always ready to move to a nearby larger and defensible centre at the first sign of trouble. Such people are, of course, reluctant to invest in well-built homes.

The same situation may not only apply to the continuously or reoccupied native villages but also to former villae. For the same reasons that they were considered to have existed in the river area during the Middle-Roman Period, villae had undoubtedly ceased to function by the end of the 3rd century. Even in the south of Germania II, on the loess areas of the *civitas Tungrorum* and the *civitas Agrippinensium*, where quite a few villae survived,[313] their owners must have had a hard time.

Nevertheless, some of the sites interpreted as villae in the Middle-Roman Period, were still or reoccupied in the Late-Roman Period. Of course, this does not necessarily imply they were organized and functioned in the same way as before, but they still may represent a different type of settlement. Of the sites in question, nos. 80, 81, 85, 109, and 298 need not be discussed because they are not definitely identified as villae. The villa from Overasselt was excavated but still produced only a few late-Roman sherds; it was, although in view of the very limited excavation in retrospect not on reliable grounds, considered to have been uninhabited (cf. Appendix 4: isolated finds). That is certainly not true for the remaining sites, nos. 59, 234, 239, 250, 291, and 508, which have not been excavated but where sometimes considerable numbers of late-Roman artefacts were collected at the surface.

Site 59 is different from the others because it was already deserted in around the end of the 3rd or the beginning of the 4th centuries at the latest. Perhaps significantly, the others are all located south of the river Waal but, in view of the distribution of villae in the 2nd-3rd centuries and the five possible villae mentioned above (4 in the Overbetuwe) that may not really mean anything. All five sites were inhabited in the late 4th century, but only for sites 234, 239, and 508 are there indications for occupation during the late-3rd and early-4th centuries as well.

The most interesting settlement is Ewijk–De Grote Aalst (site 239), where the surface finds indicated – in the context of the eastern river area – an extraordinary luxurious establishment (cf. p. 279). It may not be a coincidence that an equally extraordinary series of late-Roman coins has been found there (see figs. 141 and 142). For a few years after its reputation had been established, the site enjoyed intense and not entirely wholesome attention paid by coin hunters with metal detectors, which surely is part of the explanation. But other sites have been examined by the same people, in all but one case (no. 244, Beuningen–De Heuve) with much less result,[314] and it cannot (yet?) be assumed that comparable quantities were normally lost at all other sites.

Within the context of the eastern river area, comparable amounts of currency have only been found in Cuijk (somewhat less, but found before the age of the detector) and Nijmegen. This may well indicate some sort of function comparable to those two sites, the precise nature of which remains unknown for the moment. Instead of attributing the finds to continued use by a wealthy villa owner, such an interpretation is much more realistic. The site may have been a defensible refuge for the people living there and in the surroundings (sites 226, 234, 244, 250), perhaps with central and protected storage facilities and (at the same time?) a watchtower. It may have been selected both because of its comparatively luxurious nature and its position close to the river Waal.

All this does, of course, in no way mean that the people living at site 239 did not also work the land or that they may not have tried to uphold some standards of villa life. The same may be true for the other villa sites as well. Two of these (234, 250) are close to site 239, a third (291) is close to Wijchen (315), and the fourth (508) directly opposite Cuijk (499). For all of them, we must assume that a convenient refuge was close at hand, but this is again an observation whose significance cannot be evaluated. Perhaps there were even in our area people foolhardy enough to invest in a villa in a relatively peaceful period and the coin evidence from a site such as De Heuve (244), which did not even have stone buildings, may also be indicative of economic activity. But except for

313 See, e.g., Von Petrikovits 1978, 244.

314 See below, figs. 141 and 142 and p. 446–9.

site 239 there are no reasons to assume that settlements at the site of a former villa were any different from those at other sites. It is not even certain they still lived in a stone house, as long as archaeological proof is lacking.

8.5.3 Cemeteries

Just as in the case of the settlements, the number of known late-Roman cemeteries is far smaller than in the preceding period. Relatively speaking, it is even smaller than that of the settlements, because the maximum number is only 10, which is nearly 13% of the number during the preceding period. Compared to the settlements (25% surviving), the number of burial sites is definitely too low. There is a convincing explanation for this phenomenon, namely, the change in burial practices. Cemeteries in general are not as easily discovered as settlements, but late-Roman cemeteries are even more difficult to find than middle-Roman ones. Those are almost invariably cremation burials in relatively shallow graves which are frequently disturbed and thus discovered during modern agricultural activities and the like. Most of the late-Roman interments, however, are inhumations in comparatively deep pits (shaft graves) which would normally require a much deeper disturbance of the soil before they are discovered. They are, in fact, very often found during the construction of buildings which provides an additional disadvantage, and not only because the possibilities for archaeological investigations are often limited in such a situation: the aversion of contractors to reporting accidental finds is too well known!

Convincing as the above explanation may be, the relative scarcity of late-Roman burial sites is nevertheless remarkable. It is not uncommon for late-Roman interments to lack grave goods,[315] which could mean that substantial numbers may have disappeared without the slightest attention being paid to them, especially during construction work. But that nothing at all is found and, in the end, comes to the attention of some professional or amateur archaeologist, is not probable. After all, a cemetery will normally have at least some graves with complete vessels or other objects which are not likely to be thrown away as often happens to sherds from a settlement. In addition, the presence of inhumations is bound to attract attention (cf. site 386) and, as far as archaeological interpretation is concerned, attention to the fact that one might be dealing with a late-Roman grave. Especially when Roman cemeteries contain late-Roman graves as well, the possibility that they are not identified is small.

This last observation leads to what is most remarkable about the lack of late-Roman burials. As mentioned above (p. 303), virtually all late-Roman settlements are either continuously inhabited or reoccupied middle-Roman sites. If the same should apply to cemeteries, many more should have been known. Therefore, while the data clearly point to a prolonged use of the same sites for habitation (which is not the same as continuity of occupation!), they indicate equally clearly a discontinuation in the use of burial sites. This conclusion is also supported by the known late-Roman cemeteries. Only sites 292 and 410 were definitely used in both periods. The burials at site 413, and certainly those at sites 418 and 419 are doubtful (see below) and the (small) spatial overlap between sites 409 and 410 has been shown by the excavations to be completely accidental. The same could be true for the middle- and late-Roman graves at site 292, but there might also be real continuity in this case.

It is difficult to interpret this change. It is entirely conceivable that it means nothing, as might be concluded from the best documented example, the Nijmegen cemeteries. The new cemeteries at sites 405 and 410 may be merely a result of people being practical: they were living at site 403 and buried their dead around its perimeter, the other sites being too far away (e.g. 398) or by then already long fallen into disuse. On the other hand, Nijmegen may be an exception because the other settlements did not move and yet their cemeteries were no longer used. Admittedly the entire archaeological data base leaves much to be desired, but one (292) more certain case of continued use out of 78 is too little to be a coincidence. It is concluded, therefore, that deliberate socially or ideologically motivated choices must have played a part, and it could be significant that the possible instances of continuity (292, 413, 418, 419) reach only to the mid-4th century.[316]

To move from these general considerations to particular burial sites, only 6 of these are definitely late-Roman. Site 386 is an inhumation burial covered with seven tegulae and is probably but not definitely late-Roman. The material of the cemeteries at sites 413, 418, and 419,

315 See, e.g, Böhner 1958, 268, Böhme 1974, 185, or Pirling 1985, 136.

316 See further chapter 12, 444.

which were looted rather than excavated, includes a minimal amount of late pottery, whose origin, however, is not beyond all doubt.[317]

The two largest cemeteries are those around Mariënburg (405) and Hugo de Grootstraat (410; also known as cemetery 'OO' or 'Margriet'), substantial parts of which were excavated after the last war.[318] The former is estimated to have consisted of approximately 1500 interments (c. 750 excavated), the latter presumably held around 800 burials (c. 350 excavated). Both presumably started in about AD 300, but the Mariënburg cemetery survived into the Merovingian Period while interments at the smaller graveyard must have stopped in about or shortly after AD 400.

The very careful excavation of the Margriet cemetery and new methods such as the biochemical analysis of bone, which permits a differentiation of age and sex even when most of the skeleton has decayed,[319] will hopefully provide a detailed insight into the social and biological composition of the local population. It has already been established that site 410 contains a group of Germanic and, in all probability, military burials.

The population indicated by the two cemeteries has been estimated at c. 460 and over 200, but these figures may need further specification. The provisional reports indicate that, at least at the largest cemetery, the majority of the burials might belong to the period before AD 350. Nevertheless, the number of late 4th- and early-5th-century graves from both cemeteries is not inconsiderable, which surely means that the remaining population at site 403 was not too severely reduced or, to be more exact, that the number of people living at site 403 after c. AD 350 was not too severely reduced. After all, we know that the population's ethnic identity may have undergone considerable change (see chapter 12).

The two graves around Wijchen (292, 321) are accidental discoveries and can contribute little to our understanding of the population. They contained quite a few grave goods, a characteristic that also seems to be especially common in early-4th-century graves in Nijmegen. No further investigations followed the original discovery.

The two cemeteries north of the Rhine (12, 16) are different from the others because the late-Roman phases at both sites only constitute the limited beginnings of what were to become large Merovingian cemeteries. The oldest burials in both cases date to the late-4th and early-5th centuries. In Wageningen (16) these are only a few, grouped around a Neolithic barrow.[320] Interestingly enough, some interments are still cremation burials. These were also present (but exceedingly rare) in Nijmegen, which may illustrate a difference in burial practices known to exist between the Roman empire and the Germania Libera, where cremation was still quite common in the Late-Roman Period.

One or a few late-Roman cremation burials are also attested to in Rhenen (12), where the majority of the 34 graves from the first phase is a clear group of inhumation burials.[321] In fact, one might as well consider these a separate cemetery at a short distance from the main Merovingian graveyard starting in VB.

Although a coherent publication of the Rhenen cemetery is still lacking, various important bits and pieces can be found scattered in the recent literature. The most important contribution for our purpose is Böhme's analysis.[322] In his opinion, the cemetery, with 62% of the male graves containing weapons, is clearly military *...und hat sicherlich zu einem spätrömischen Militärposten nördlich des Rheins gehört...* Böhme's conclusion is not, of course, as absolutely certain as presented: the relation between Germanic warrior graves and late-Roman military strongholds is not an entirely unambiguous one and such a site near Rhenen remains to be found. On the other hand, while perhaps stated too confidently, the interpretation arose from a thorough comparative study and is surely more than a mere possibility. By itself, the evidence indicates that a late-4th-century military post in Rhenen is not improbable although the character of such a facility at that location remains an open question.

An additional argument is provided by the geological situation. A fortified watchtower at the site of present-day Rhenen or on the Grebbeberg would be situated on the high Pleistocene ice-pushed ridge. That would not

317 See Stuart 1963, 25, 48, 109, and 111 for the pottery. The findspots of the pottery are usually considered to be reliable (e.g. Brunsting 1937, 6), but there was no control whatsoever over the find-circumstances and errors are not impossible.

318 Provisional reports in Wynia 1979, 66–8 (site 405) and Bloemers 1983, 193–8 (site 410). Complete publication is currently being prepared by P.A.M. Zoetbrood.

319 Lengyel 1980.

320 Van Es 1964, 295–7.

321 Ypey 1978, fig. 3.

322 Böhme 1974, 185, 268–72. See also *Taf.* 59–69.

only provide an easily defensible position but also an excellent look-out post and opportunities for signalling. Such a station would also be conceivable in the context of what is known about the eastern river area and against the general background of the late-4th-century situation in Germania II.[323] A rejection of Böhme's conclusion for the sole reason that Rhenen is situated on the Rhine is quite untenable.[324]

8.5.4 Isolated Finds

Most of the isolated finds from the Late-Roman Period are stray sherds, some of which have been mentioned in previous paragraphs. One stray find, the bronze hairpin recently discovered at site 199 along the river IJssel (see chapter 7), should be mentioned here because it is not indicated on Appendix 4.

Only one category of late-Roman isolated finds, the hoards, deserve special attention. Not more than three of these could be indicated as such on Appendix 4, but there are several other finds which might also be considered hoards. In some cases that seems to be an acceptable interpretation, such as the relatively valuable gold coins (stray finds), but there are also finds reported in the literature which are rather dubious and a few which have to be rejected. To sum up, the following finds are concerned:

A The area north of the Rhine

1 Site 9, Rhenen (Achterberg–Friesesteeg). Hoard of two faceted gold torcs of the so-called 'type Velp' and a fragment of a third torc of different form, decorated with five glass paste stones. See fig. 145. The finds, discovered in 1938, were published by Roes (1947). The total gold weight amounts to 212 grams. The hoard has been dated to IVB, preferably in IVd, and may have been buried in about AD 400.

2 Renkum–De Beken (probably not far from site 21). One gold coin of Gratianus (*ER* III, 164), found before 1826. No other data are known. There is a possibility that there is yet another hoard from the area Wageningen–Renkum, consisting of a few gold coins (latest specimen: Magnentius), but there is no really tangible evidence in this case.[325]

3 Site 30, Velp–Hervormde Kerk. Large hoard, consisting of six faceted gold torcs which led to the designation 'type Velp', a fragment of a torc of plain round gold wire and a fragment of a similar but decorated torc, as well as three finger-rings. The finds were discovered in 1851 and, fortunately, published immediately afterwards by Janssen. The total gold weight is 530.4 grams.[326] The dating of this hoard conforms to that of Rhenen and it can be assumed to have been buried in about AD 400.

4 Site 32, Velp–Grote en kleine Durk. The extraordinary collection of finds from this site as described by Pleyte (see catalogue) might be considered a hoard. In view of the artefacts involved and the absence of data on the find circumstances, such an interpretation is not very likely to be correct.[327]

5 Emmerich–Elterberg (c. 209/341). One gold coin of Honorius. No other data are known.[328]

B The area south of the Rhine

6 Ressen–Bemmel. The aureus of Constantinus I reported from this site (site 150) was not included in the catalogue because it was almost certainly not found there and quite possibly not even in the river area.[329]

7 Site 244, Beuningen–De Heuve. One solidus of Arcadius, stray find.

8 Site 316, Wijchen–Wijchens Meer. The small golden fibula from this site (see p. 313 and fig. 89), weight 5.5 grams, could be interpreted as part of a (washed-down) hoard: it was found in soil from the former branch of the Meuse. Obviously, other interpretations are also possible. It is datable in the late 4th or the first half of the 5th century.

323 See below, chapter 12. Note that, in contrast to what has been said earlier about middle-Roman frontier-forts (chapter 3, p. 57–8), the location of a late-Roman heavily fortified installation north of the river is unusual but not unthinkable. In addition, and perhaps more likely, the soldiers involved and thus their 'station' may have been very irregular indeed.

324 See Ypey 1978, 57, who otherwise agrees with Böhme on the basic data provided by the finds. His only argument, cf. his notes 6–8, is based on the no longer tenable hypothetical construction of a late-Roman 'Waal-*limes*' by Bogaers 1967 and 1968.

325 De Boone 1954, 220, sub A.

326 Janssen 1852, 164 indicates the weight as 53 *lood* and 4 *korrel*.

327 See also chapter 3, p. 59–60 and notes 155–6.

328 De Boone 1954, 222, sub C.

329 The coin was included by De Boone 1954, 218, but almost all of the extraordinary objects from the collection Van Olst were in fact bought or traded finds from far afield.

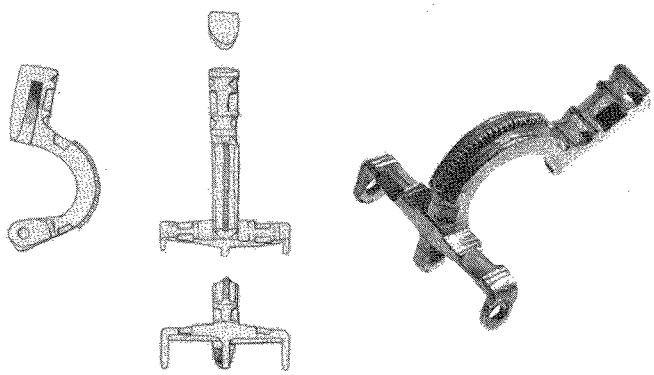


Fig. 89 Golden Germanic *Stützarmfibel* from the Wijchense Meer (site 316). Drawing scale 1:1.

9 Site 399, Nijmegen–Fort Krayenhof. Probably a hoard, consisting of one damaged gold bracelet decorated in the style of the torcs of the Velp type, found in or before 1901 near the former fort Krayenhof,[330] and thus in the area of Ulpia Noviomagus. The weight is 18.25 grams and the dating and estimated period of hiding are identical to the hoards of Rhenen and Velp–Hervormde Kerk.

10 Site 403, Nijmegen–Valkhof. One gold coin of Valentinianus, found in 1786.[331]

11 Megen (c. 167/425). One gold coin of Arcadius. No other data are known.[332]

12 Site 499, Cuijk. One gold coin of Valentinianus. No other data are known.[333]

13 Site 467, Kessel–Niers. Hoard, consisting of three solidi of Constantinus I, Constantius II, and Magnentius, as well as a golden ring with a stone of rock-crystal. It was found in 1935 and is probably a washed-down hoard.[334] The gold weight of the coins is 15.7 grams.

In addition to the above-mentioned finds there are two other gold coins from Nijmegen and, most notably, the first hoard of Velp (site 28, Velp–Het Laar), found in 1715. There are several viewpoints from which these finds should be discussed together with those mentioned so far. Nevertheless, there is also a difference because the hoard from site 28 was buried in the mid-5th century and thus de facto in a post- rather than a late-Roman context. It has been indicated on Appendix 5 for that reason, at the same time symbolising the absence of abrupt changes between 'late-' and 'post-'Roman, as already outlined in chapter 2.4. These are, after all, no more than analytical historical concepts which may obscure issues rather than clarify them. However, it is difficult to believe that the owner of the wealth stored in the gold artefacts hidden at site 28 felt compelled to do so for reasons which were very different from those of the owners of the other hoards a few decades earlier.

From the available evidence it is quite clear that the eastern river area has its fair share of the enormous amounts of gold that were hidden in the late-4th and early-5th centuries both inside and beyond the imperial frontier.[335] A treasure such as the second hoard from Velp (site 30) is not surpassed by many other hoards and in itself is an indication that its owner must have been a high-placed an powerful individual. It is entirely conceivable that the torcs such as those from Rhenen and Velp were status symbols of very high-ranking individuals. It is generally accepted that they were manufactured from melted down solidi, but it is rather improbable that they should be '*nichts anderes als eine Art der Aufbewahrung einer grösseren Menge Gold bei Völkern, wo das Münzwesen kaum oder nicht von praktischer Bedeutung war*'.[336] The fact that the torcs in some of the hoards show hardly any traces of wear does not justify such a conclusion. It only means that they were not regularly worn trinkets, which is hardly surprising for such valuable objects, and it is not incompatible with an interpretation as status symbols. Such symbols are often reserved for display on very special occasions.

Furthermore, it is difficult to understand why the solidi were melted down in the first place, if the purpose was only to preserve the gold. In addition, the practical use of gold coinage inside the empire was very limited as well,[337] and yet torcs – or equivalent objects – are practically absent there. With some exceptions, such as the

330 Braat 1954, 3.

331 *ER* III, 89. De Boone 1954, 220, sub B indicates Valens instead of Valentinianus, possibly by mistake (pers. comm. 1984).

332 De Boone 1954, 220, sub B.

333 Boersma 1963, 37 (with partially wrong references).

334 *BJ* 142, 1937, 196, 202; 143/4, 1938–9, 292.

335 See De Boone 1954, 220–5; Waterbolk/Glasbergen 1955, 98–9; Bloemers 1983, 198–9. See also chapter 12.

336 Waterbolk/Glasbergen 1955, 100.

337 See e.g. Reece 1977, who suggests that it was important only in dealings by and with the state. Cf. also Frere 1978, 415–6.

one bracelet from Nijmegen (which is not even definitely a hoard), hoards from inside the empire appear to have consisted entirely or almost entirely of coins and normally do not contain the equivalent in solidi of even one torc (some 12 to 19 coins of c. 4.5 grams).[338]

This is quite possibly due to the fact that the gold expended by the state in political payments outside its frontiers could not be retrieved as was most of the precious coin used to pay the imperial military and civil organization which returned into the hands of the state (cf. Reece's model of gold circulation inside the empire).

Perhaps the above reasoning can also be employed to use the distribution of large gold hoards to establish a divide between the area where the Roman state was still able to retrieve its gold succesfully and the area where that was not possible. Because that divide evidently coincides with the Rhine it might be concluded that the river also marks the line inside which gold was paid out in retrievable form such as salaries and beyond which it was used for political payments to people outside the cycle of gold circulation and thus outside direct imperial control.

Such a reasoning is not without its risks, but, as is indicated by the hoards from Rhenen and Velp and also other evidence discussed previously in this chapter, the line was rather sharply defined around AD 400, at least from a supra-regional point of view. A hoard such as that from Menzelen shows that the Rhine as a divide should not be overaccentuated at a regional level even though the same hoard may also be interpreted as a chronological divide which illustrates (rather than marks) the end of the cycle of gold circulation. In any case, while incorporating a rather dangerous element of reasoning *ex nihilo* the distribution and size of hoards do support a picture of fairly effective Roman control in the eastern river area up to the Rhine until the early-5th century.

8.6 THE MEROVINGIAN PERIOD

The entire post-Roman period to be discussed here, labelled 'Merovingian' for convenience, was defined in chapter 2 as lasting from c. Vb to VIIIa. It thus spans almost exactly three centuries, approximately twice as long as the preceding late-Roman phase. This long duration is undoubtedly the major reason why the tota number of known settlements is larger, although it i still much smaller than during any of the phases befor the late-3rd century, including the Late-Iron Age. Thi observation is in accordance with the picture presente by the numerous available pollen diagrams, which shov a maximum of tree pollen and a minimum of culture in dicators in the 5th and 6th centuries.

The total number of known settlements between Vb an VIIIa is 104, to which can be added the sites in Ooster hout (545) and Escharen (548) mentioned in chapter 7 Of this total number, 10 sites are only probably inhabit ed settlements and 6 are mere possibilities. There is, o course, no evidence that all these sites were inhabite during the entire period under discussion. The archaeo logical material does not, however, allow too mucl chronological differentiation because the most frequen finds, sherds of the Merovingian coarse ware, notabl the steep-walled cooking-pots discussed in paragrapl 6.2.7 (fig. 48), are hardly suitable for that purpose.

Only the settlements which were definitely inhabite during the 5th century can be reliably distinguishe from the others because of the presence of some shorter lived pottery forms, such as the 'type 3' cooking-pot discussed in chapter 6.2.5 (p. 168–170). There are 2 such sites, including the doubtful settlement in Huisse but, more important, without even one Nijmegen settle ment. In Nijmegen, reliable finds indicating habitatio throughout the 5th century are still lacking, althougl continued use of the Mariënburg cemetery (site 405) ha been assumed.

The figure of 25 settlements is, of course, only minimum number because there are other sites wher late-Roman as well as Merovingian habitation has bee demonstrated (such as Nijmegen!) and which are at leas likely to have been inhabited during the 5th century. I other cases, such as Ede–Veldhuizen (site 1), the fina desertion of the settlement may well have taken plac only towards AD 500. On the other hand, not all of th 106 Merovingian settlements are likely to turn out t have been inhabited during the 5th century, and thus a increase in the number of settlements throughout th Merovingian Period is a logical assumption.

It is, in fact, more than just an assumption because th

338 See Bloemers 1983, 198–200. A notable exception is the hoard of Menzelen near Xanten (Kaiser-Raiß/Klüßendorf 1984) which contained at least 207 solidi and was buried very shortly after AD 412/13. The other famous Xanten hoard of c. 400 solidi (*BJ* 151, 250–1 and *op. cit.*) may be less relevant because it was buried in the mid-5th century, at least after A 425.

evidence from the pollen diagrams mentioned above very clearly leads to the same conclusion. The values for culture indicators show an increase before the end of the Merovingian Period (such as some *Hordeum*, the appearance of *Secale*, and the spreading of *Plantago* and *Rumex*[339]). A third source of information in this respect are the cemeteries (see below). Certainly on sites such as Rhenen (12), Wageningen (16), or Lent (159), there is an increase in the number of burials from the 5th or the 6th into the 7th centuries which again points to a general process of population growth in the river area as a whole. Finally, the distribution of the sites compared to the preceding period should also be mentioned (see Appendices 4 and 5). A concentration of habitation in the eastern part of the eastern river area such as in the Late-Roman Period is no longer observable, which might be interpreted as evidence for the resettling of virtually deserted areas in the west.

On the basis of the available evidence it is clearly a very risky undertaking to provide estimates for the total number of settlements in the area during the three centuries after the end of the Roman occupation. The archaeological and pollen-analytical data indicate that the maximum number cannot have been anywhere near that of the five centuries before the late-3rd century. If the estimated numbers of 250–300 for the Late-Iron Age and 350–500 for the Middle-Roman Period are indeed in the correct order of magnitude, then the maximum number of inhabited sites at the very end of the Merovingian Period, in the early-8th century, may have reached 200. Given the number of settlements known to date, the probability of a substantial number of undiscovered sites on the Pleistocene soils, and the probability that the dependence on relatively scarce wheel-turned pottery caused the Merovingian occupation on a number of known sites to remain unnoticed, a figure of 200 does indeed seem to be a reasonable estimate. Comparable arguments for the 5th century lead to an estimate of some 50 settlements, although the relatively very limited means for dating a site in the 5th century may also allow a higher estimate: 50–100? On the other hand, the near absence of culture indicators in the pollen diagrams can hardly be reconciled with higher figures even though the return of the forest undoubtedly prevented the wide dispersion of pollen, and most if not all of the settlements may have been very small indeed. Unfortunately, little is known about this last subject or, for that matter, about most aspects of Merovingian settlements.

8.6.1 Settlements

Although there have been a number of excavations at settlements which were also inhabited during the three centuries after the Roman Period, there are only a few sites where at least some coherent features from that time were also included in the investigated part. The excavations at sites 21, 105, 109, 126, 143, 150, 403, 461, 473, and 479 are of no use in the present discussion. That leaves only the investigations at sites 75, 246, 444, 466, 500, 536, 548, and perhaps those at site 1.

The latter site (Ede–Veldhuizen) may, as mentioned above, have existed until well into or the end of the 5th century. Although the site does not appear on Appendix 5 because of the conclusions on its chronology in the preliminary reports, the datable wheel-turned pottery certainly allows a different conclusion. In addition, the settlement of Wijster, comparable in many ways, is nowadays also considered to have been inhabited throughout the 5th and even into the 6th centuries.[340] The latest phase of Ede–Veldhuizen, as a nucleated village with a considerable degree of internal structuring,[341] may thus represent one form of 5th-century habitation. Unfortunately, a final publication and plans of the successive phases are still lacking, which prevents a more detailed discussion of the structure and size of the final phase.

The small excavation at De Hoge Hof in Zetten[342] produced finds which are datable, and a series of features which could be datable, between the Roman v. 'Carolingian and later' traces. The excavation remained too small to yield interpretable plans although, as was also mentioned for the late-Roman occupation here, a reanalysis of the original documentation may provide useful information. The excavations in Millingen (site 444) and Angerlo (site 536) are similar to Zetten in that the excavated area is too small.[343] For both sites, Merovingian habitation has been demonstrated and a few features were identified, but the core of the occupation from that period was either not excavated or only very partially, and thus uninterpretably, touched upon.

339 See e.g. Teunissen/Teunissen van Oorschot 1980, 263 and especially the overview in Teunissen 1982, 27, fig. 11. See also fig. 137.

340 Waterbolk 1984, 22.

341 See above, p. 283–4 and 313–4.

342 Braat 1937.

343 Millingen: Hulst 1975; Angerlo: *JROB* 1982, 33.



Fig. 90 Early–Medieval settlement unit from Kootwijk, after Heidinga 1984, fig. I, 1:5. Location of the site indicated on fig. 80. Drawing IPP.

The excavations in Cuijk (site 500), where a continuous habitation in a probably continuously important settlement can be assumed, only yielded a few features between the late-Roman fort and that of the (early-?) Middle Ages which was destroyed in AD 1132.[344] The summary publications only mention one or probably two sunken huts, one of which was presumably used for weaving and datable to the 7th century. The presence of other possibly intermediate but undatable features is not recorded. If these were indeed absent, this may well have been due to the extensive later disturbance which could have erased all but the deepest features. Of course, the continued use of late-Roman buildings is, in Cuijk, also a phenomenon to be reckoned with.

Although not entirely relevant in the present context, the late-Merovingian or early-Carolingian temporary occupation of site 466 (Asperden) should also be mentioned here.[345] It is not a settlement in the sense of the concept as employed here, but a single activity site (and thus essentially an isolated find) where people remained for a short time to rob (at least) the iron from the late-Roman *burgus* and to rework it on the spot. The latter has left traces of activities which are usually difficult to identify but which must already have been regular occurrences during the Late-Roman Period.

The only two sites where, with respect to the early-medieval occupation, more successful excavations were carried out in our area are Beuningen (site 246) and Escharen (site 548).[346] Although both excavations have only very recently been completed and the analysis of the results has yet to begin, it is clear on both sites that the Merovingian settlement is only a small part of the total settled area, which is datable to later periods. The settlements as such probably consisted of only one or two contemporaneous houses. What these houses looked like, and the way in which they were organized into a settlement are, obviously, questions which cannot be treated here.

Unfortunately, evidence on these aspects from outside the eastern river area is also limited, especially as far as accessible published information is concerned. Only for the northern Netherlands has the work of the BAI re-

344 Bogaers 1966, 71 and 1967, *afb.* 9; Van Es 1973a, 285–6.

345 Hinz/Hömberg 1968, 190–1. The site was not indicated on Appendix 5 because it is most likely to have been early-Carolingian.

346 Beuningen: *JROB* 1982, 32–3; 1983, 31; Escharen *JROB* 1983, 60–2.

sulted in a more or less coherent picture with excavations such as those of Wijster, Odoorn, Eursinge, and Peeloo.[347] A report is also available for the settlements of Kootwijk (see fig. 80) and Hoog Buurlo on the Veluwe.[348] Apart from a few other published excavations such as those in Rijnsburg and Grubbenvorst,[349] there are only two large excavations for which partial and preliminary reports are available: Den Burg on the island of Texel[350] and Dommelen in Northern Brabant.[351]

Leaving aside the details of the intricacies of the typological and functional development of houses, it appears that there is gradual and uninterrupted evolution from the Roman Period onwards. For the northern provinces that has already been conclusively demonstrated, and for the Veluwe the same phenomenon can be recognized.[352] Although most Merovingian houses appear to be fairly small, large structures with a clearly recognizable byre have been found in the north and in the settlements of Rijnsburg, Ede–Veldhoven, and Kootwijk-5, but are so far lacking in the southern Netherlands.

In general, the houses form settlement units together with accompanying structures such as barns, a well, and, as a very characteristic element, sunken huts. Sometimes these units are clearly marked yards, differentiated from their surroundings by ditches or palisades (fig. 90). Occasionally, burials are also associated with the settlement units (Dommelen, Escharen). Most of the excavated settlements appear to be similar to those at sites 246 and 548 in that they were composed of only one or a few of such units, indicating a rather dispersed form of habitation.[353] The large nucleated villages such as Ede–Veldhoven and Wijster (see fig. 80) disappeared during the 5th century, to return again in basically the same form after the Merovingian Period. For the eastern river area, virtually all sites may well have been more or less similar, small settlements.

On the other hand, not all settlements were necessarily the same. The poverty of available data indicates that direct archaeological clues for differentiation are virtually absent. There are, however, still a few sites which may have been of special importance and perhaps also larger, although the evidence is circumstantial and indirect.

The first of these is, of course, Nijmegen, the Noita of the Anonymus Ravennas (see fig. 140), which was presumably never outside Frankish control. It is assumed to have been the base from which Dagobert I conquered the central river area in VII b, and which was later used by Charlemagne as one of his residences.[354] Leupen postulates the oldest parish church of Nijmegen on site 403 (Valkhof) as early as c. 600 AD.[355]

Archaeological evidence for the post-Roman habitation in Nijmegen is, however, meagre. Occupation of the Valkhof is probable but, apart from the 7th century cemetery (site 404) there, without direct evidence. The cemetery may have belonged to the church.[356] Other traces of Merovingian occupation were discovered around the Korte Brouwersstraat (site 401) and in the 1983 and later ROB-excavations at the Waalkade. Some habitation in this low-lying area along the Waal can thus be considered proven, but its character and extent are as yet unknown. An elongated settlement along the bank of the Waal, perhaps even a fairly large one connected to the Valkhof, may, however, easily escape attention: it may have been largely eroded by the river. Nevertheless, its existence as a relatively large site needs to be documented by more finds; even material in secondary position is limited so far.

The historical information is thus more indicative of a centre (though not necessarily in an administrative sense) of some importance in Nijmegen than the archaeological data, which only indicate habitation. Indirect archaeological evidence for the status of Nijmegen is also provided by the NIOMAGO coins (see fig. 92) in the hoard from Escharen (site 514), which were probably minted in Nijmegen in the late-6th century.[357] The activity of a monetarius presupposes economic activities of some importance[358] and thus the presence of a mint in Nijmegen testifies to its central function as well as to the economic

347 Van Es 1967 (Wijster), Waterbolk 1973 (Odoorn), Lanting 1977 (Eursinge), Bardet/Kooi/Waterbolk/Wieringa 1983 (Peelo). For overviews, see also Waterbolk 1979, 1980, and especially 1982.

348 Heidinga 1984, 13–6 (Kootwijk 4 and 5) and 16–19 (Hoog Buurlo).

349 Van Es 1973a, Sarfatij 1977

350 Woltering 1975, 28–33. Location on fig. 80.

351 Van Regteren Altena, in: Slofstra a.o. 1982, 114–124. Location on fig. 80.

352 See Heidinga 1984, 41–9.

353 Cf. Heidinga 1984, 87.

354 Blok 1979, 35–7 and 74.

355 Leupen 1979.

356 Thijssen 1980, 13. The position of site 404 on Appendix 5 is incorrect!

357 See p. 328, fig. 92, and note 397.

358 Ewig 1980, 44–6.

revival in the region around it: the central importance of the river area as a whole can be deduced by various means.[359]

Apart from Nijmegen, the secondary centre and military stronghold in Cuijk from the Roman Period may have remained important in the Merovingian Period. Continued use of the Roman road to Nijmegen and its strategic position at the Meuse-crossing, continuity in the name Ceuclum–Cuijk, and the presence of the later castle (destroyed again in the 12th century) are all indications for such an interpretation.[360] Another settlement which may have remained some sort of centre is Wijchen (site 315), with evidence for the 5th to 8th-century (and later) occupation over the entire area of the Roman *vicus*. Corroborating historical data do, however, seem to be lacking. In addition, the Wijchense Maasje started silting up in the 5th century which should have negatively influenced the development of the site. To consider site 315 as a relatively large settlement with some central functions is thus rather doubtful for the period at hand.

The same is to some extent true for site 123 in Driel. The 5th to 8th-century (and later) finds are plentiful and some are even extraordinary, but the site as far as it is known today was not large. The first source naming Driel (*Dryele in Betua*) dates only from 1262, but the name itself may be older: if it should be a *-lo*-name, a dating in at least the end of Merovingian Period is necessary.[361] On the other hand, the Rhine crossing at the Drielsche Veer was a vital link in the very important early-Medieval north-south route from the Veluwe to Nijmegen and the location of the Hunneschans or Duno, a ring-fort from before AD 1000, on the other side of the Rhine, can hardly be a coincidence.[362]

In addition to the above-mentioned sites, there is enough archaeological evidence at sites 23 and 126 in Meinerswijk to assume a direct relationship to the *Meginhardiswich* (AD 814) or *vicus Meginhardi* which was looted by Vikings in AD 847. It seems logical to assume that the evidence for continuous occupation here is evidence for the development from the late-Roman strong hold Castra Herculis into the early-Medieval *emporiun* The available archaeological information is discussed i chapter 9.

There are a number of other places known from histori cal sources which may have been important settlement but for which corroborating archaeological evidence i usually not available. In contrast to place-names i classical sources, those mentioned in early-Medieva sources do not also almost automatically imply that th sites were of at least regional significance. Furthermore in most cases what is known about these settlement seems to refer to the 7th and 8th centuries or later. I might be more relevant in the context of the late-Mero vingian and Carolingian (or even later) development than in the present context, which is primarily con cerned with what remained after the Roman Period an not so much with what was built again on and from thos remains. Nevertheless, there are a number of sites whic should at least be mentioned here.

North of the Rhine, these are Rhenen, Wageninger Arnhem, and Elten. The evidence for Rhenen (AD 855 *Hrenhem*) as an important place is summarized b Heidinga, who mentions, for example, an exceptionall large ring-fort at the Grebbeberg.[363] In chapter 8.5 (p 310 and 317), the possible presence of a small late-Ro man stronghold in Rhenen was already referred to an with the additional evidence of the Merovingian ceme teries here (below, chapter 8.6.2) one might conside Rhenen an important site during the 5th-8th centuries.

The possible importance of Wageningen (possibly A 838: *villa Uuaganuuega*),[364] opposite the presumed Ro man fort in Randwijk and on the north-south rout crossing the Rhine at the old Lexkesveer, is indicated b its geographical position. The large Merovingian ceme tery (site 18) on the only part of this route where the ac tual road can be identified with certainty (the Dieden weg) could provide archaeological support for a post Roman settlement of some size.[365]

359 E.g. Heidinga 1984, part III, chapter 2, who considers it as a nuclear region or *kerngewest* (see esp. p. 201, fig. III, 2:2) and Ewig 1980, 45, who called attention to the presence of mints in Nijmegen and Dorestad on the one hand and Köln and Maastricht on the other, but also to their absence along the Rhine and Meuse in the area between these regions.

360 Cf. note 344.

361 Cf. the comparable arguments for (Maas)Driel in Blok 1980, 35. Another toponymical derivation is, however, possible.

362 See Heidinga 1984, 232–4.

363 Heidinga 1984, 207. See also Blommesteijn a.o. 1977 37–8.

364 The identification is not considered secure by some, e.g Gijsseling 1960 (Waganwega: unknown) and Heidinga 1984 170 (unknown and *not* Wageningen).

365 This is possible, but not necessarily so. See below, p. 32 and note 376.

The historical evidence for Arnhem (AD 893: *Arneheym, Arneym*) was recently discussed by Verkerk.[366] Although there is some evidence for a Merovingian cemetery (site 25) there and the situation across the Rhine from Meinerswijk is significant, there are no definite data to support an interpretation as an important settlement before the 8th century.

An administrative centre during the 9th and 10th centuries was situated on the Elterberg, once the seat of Wichman, Count of Hamaland, who also owned the *praedium Arnhem* and a *praedium* and *curtis* in Renkum (perhaps the above-mentioned Hunneschans or Duno).[367] There is, however, no firm archaeological evidence for any habitation before the late-9th century. Definitely Merovingian finds are lacking and the Roman material was probably transported to the site from elsewhere.[368]

South of the Rhine, Elst (AD 726: *villa Eliste*) is, of course, also a settlement to be considered here. Although there is evidence for Merovingian habitation at site 105, there are no indications for a continuous occupation and not even for a continuity between the Roman temple and the first church which could 'not impossibly' date to the 8th century.[369] The fact that *villa Eliste* belonged to the domains of Everhard, confiscated before AD 711 because of his disloyalty to Childebert III,[370] does not necessarily imply that it was of special importance. On the other hand Elst, which must have had important ruins – if not actual stone buildings – surviving from the Roman Period and thus have been a desirable property, is also located in the geographical centre of the Betuwe and very favourably situated in relation to overland routes. In any case, if the current assumptions on the later developments are correct,[371] a church was built on the site of the Roman temple only after AD 726 when Elst was given to Bishop Willibrord in Utrecht.

It is possible that close to Nijmegen the village of Beek (AD 814: *villa Bechi*) was of special importance. The finds at site 420 show that it may have been an industrial centre (pottery production). In addition, it is known that in AD 826 Louis the Pious and his son Lotharius traded a large estate in northern Italy to a count Boso for his relatively modest possesions (eight *mansi* and a chapel) in Beek. As Leupen observed in his study on Beek,[372] this exchange implies that Boso's property must have been of considerable value to them. This may, however, as Leupen suggests, not have been the intrinsic value of the villa itself and there is no definite connection between the archeological and historical data which are at least 100–150 years apart.

Further to the east, the definitely continuously occupied Qualburg, and perhaps also Rindern, may also have been of some significance. Rindern is, for example, mentioned as the place near which the Viking chief Godefrid was slain by Count Everhard of Hamaland.[373] Finally, the town of Kleve seems to have become important only in later times.[374]

8.6.2 Cemeteries

Although Merovingian graves are usually as deep and as difficult to detect as late-Roman burials, the total number discovered so far is surprisingly high. The maximum number of known cemeteries in the area covered by Appendix 5 is 32 and, while still deficient in many ways, the information about them is better than that on settlements from the same period. After all, excavations of cemeteries were (and are) more readily started and complete pots or other objects are not as easily discarded as a few sherds.

There are, of course, great differences in the level of knowledge about the various sites. The most important excavated cemeteries are those in Rhenen (site 12), Wageningen (site 16), and Lent (site 159). Burials were also excavated in Huissen (site 134), Nijmegen (site 404 and 405), Donsbrüggen (site 454), and Escharen (site 549), while the grave goods of several graves (parts of cemeteries) were salvaged from Beuningen (site 247), Hernen (site 287), Wyler (site 435), Deursen (site 484), and Wijchen (site 519). In addition, there are older reports on finds from cemeteries where the available information leaves much to be desired, such as those on the Grebbeberg (not in the catalogue),[375] near Heelsum (see chapter 5 under site 21 and note 13), in Millingen (site 441), and near Klein-Linden (site 497).

The remaining sites so far all represent either definitely

366 Verkerk 1983; see also Heidinga 1984.

367 See Binding a.o. 1970 and also Verkerk 1983, 12–5 and Heidinga 1984, 215–7.

368 See Binding/Janssen/Jungklaaß 1970, esp. 1–14.

369 Bogaers 1955, 195–206.

370 Verkerk 1983, 6.

371 Bogaers 1955, 198–229, Verkerk 1983, 6–9.

372 Leupen 1977, 387.

373 Ewig 1980, 185–6. On Rindern see further Gorissen 1975.

374 Gorissen 1952.

375 On the Heimerberg, square (169/441). See Ypey 1959, 110–111.

or presumably only one grave. In many cases they are only the findspots of one undamaged vessel, usually a biconical pot. With some exceptions it can safely be deduced that these pots must have originated from accidentally disturbed graves, for which sometimes additional proof exists.

The largest cemeteries, those in Rhenen and Wageningen with c. 1100 and c. 800 (originally probably also over 1000) burials, were used from the Late-Roman Period onwards. They are, in fact, a separate category of cemetery because of their size as well as their chronology. It is not inconceivable that such cemeteries were central burial grounds for a population from several villages instead of belonging to one large settlement.[376] There is as yet no proof for such an assumption, but even if there were that would not necessarily invalidate what was said above about the possible significance of Rhenen and Wageningen in the Early–Middle Ages.

Neither of the cemeteries has yet been fully published,[377] which hampers a discussion of their interpretation beyond the two distinguishing features already mentioned. A sensible framework for such a discussion has recently been provided by Steuer, together with an overview and critique of older interpretations,[378] but it cannot be applied here. For the moment, we must be content with the observation that the two cemeteries represent a much larger burial community than all others. It is also noteworthy that, while the Rhenen cemetery generally speaking is the richer of the two,[379] exceptionally wealthy interments have not been recorded at either site. This does not mean that rich burials are lacking, for example, the male burial 775 from Rhenen recently published by Ypey, with such grave goods as an umbo, snaffle bit, bucket, the sheath of a (disappeared?) sax, part of a (disappeared?) bronze bowl and two sets of gold-covered silver fittings with central discs in the shape of D-bracteates.[380] With a few other burials this interment surely belongs to the relatively rare *Qualitätsgruppe C* as defined by Christlein,[381] which represents people in a relatively high social position. It is, however, not comparable to the really rich burials of group D,[382] which are absent in the eastern river area. There is only one (remote) possibility or such a grave, namely, the presumably female burial from Beuningen (site 247), which is in fact probably also a 'type C' grave and was uncovered by a contractor under somewhat obscure circumstances. It must have contained, among others, a splendid necklace (fig. 91) composed of 13 amethyst, 1 amber, and 10 glass beads with no less than 13 gold pendants and a gold tremissis from Dorestad, monetarius Madelinus (total gold weight 10,2 grams). Comparatively speaking, the value of the objects is certainly not excessive,[383] but the



Fig. 91 Reconstruction of the necklace of Beuningen (site 247), without the glass beads. Scale 1:2.

376 See the comparable arguments forwarded in the *JROB* 1981, 76 (on Wageningen) and by Heidinga 1984, 178–80.

377 For the recent excavations in Wageningen see *JROB* 1981, 76. For Rhenen, see Ypey 1973a, Böhme 1974, Ypey 1978 and 1983.

378 See Steuer 1982, esp. 362–92, 436–44, 458–71, and 498–501.

379 This may not be such a clear difference after the new excavations in Wageningen have been published. In addition, part of the central area of this cemetery was destroyed.

380 Ypey 1983. The fact that a spatha is missing in the inventory further reinforces the idea that the grave have may beer robbed.

381 See Christlein 1973.

382 Christlein 1978, 83–111.

383 See e.g. the data assembled by Steuer 1982, 448–54. The value of a full armour in the 6th-7th centuries is estimated at c 10 solidi (45 g gold or 675 g silver).

grave goods from Beuningen are quite possibly not complete.[384]

From a chronological point of view, the only cemetery comparable to those of Rhenen and Wageningen is site 405 in Nijmegen which was also in use in the Late-Roman Period. Compared to its former size, this cemetery, as far as it is known to date, seems to have been rather small in Merovingian times,[385] a situation which is exactly the reverse for the other two. There are several other sites which were also used for burials during the Middle-Roman Period, namely, nos. 134, 217, 241, 454, and 474, but there are no cases where continued use can be demonstrated. The size of the chronological gap, which may amount to four centuries, is too large to allow any hasty speculation on this point.

As has already been mentioned, the evidence from most Merovingian cemeteries, as far as they can be evaluated, points to a start in the 6th century while the majority of the burials dates to the 7th century. It is probable that the majority will prove to have been of medium size, comparable to the graveyard of Lent, which contained some 140 interments and may be ascribed to a community of only a few households.[386] It belongs to Steuer's type 2 and at least the smaller of its two groups of graves contained burials with a snaffle bit belonging to Christlein's group C and thus the interments of people of relatively high rank.

The presence of small cemeteries from a single household can also be expected, as well as the isolated and very small graveyards of high-ranking individuals sometimes referred to as *Adelsgräberfelder*.[387] Site 247 may be a case in point. Whether or not isolated graves occur is rather uncertain. With the exception of a few *Fürstengräber*, genuine isolated graves are apparently unknown.[388] There is no evidence that any of the single graves recorded in the eastern river area so far was indeed isolated and in view of the lack of evidence from elsewhere there are no reasons to assume they were present.

From the available evidence on Merovingian cemeteries it is not yet possible to present far-reaching conclusions. That they come in different sizes is obvious, and so is the fact that there is a substantial amount of discontinuity in the use of burial sites from the previous period. In fact, it might be significant that there are eight cases (25%) where Roman cemeteries were used, even though real continuity exists for only three of them.

It is also worth noting that the number of wealthy people identifiable by 'group C' grave goods seems to be quite considerable. There is a group of those in Lent and the number in Rhenen will undoubtedly prove to be rather large. They are even present in the part of the 'poor' cemetery from Wageningen that has been published.[389] Together with other data, such as the burial from Beuningen, the probable activities of a monetarius in Nijmegen, and the presence of hoards and stray gold coins (to be discussed in the next paragraph), they testify to an economic revival and the relative importance of the eastern river area. Evidence for very high-ranking officials and nobility is, however, absent after the mid-5th century. Only the owners of the two Velp hoards and perhaps that of the one from Rhenen may be classified as such.

8.6.3 Isolated Finds

Most of the isolated finds indicated on Appendix 5 are stray sherds and a few other objects. Only the hoards and the stray gold coins, some of which might be hoards as well, merit a separate discussion. There are, in fact, quite a few of those in addition to the two known hoards of which only one is Merovingian. The following finds are concerned:

1 Site 28, Velp–Het Laar. This hoard, which was discovered in 1715, has already been mentioned in paragraph 8.5.4 because it can also be considered 'late-Roman'. The hoard is only partially known because most of the finds, a large but unknown[390] amount of gold coins, rings or torcs, and five 5th-century gold medallions originally attached to a chain, have been dispersed. Only three of the medallions, with portraits of Honorius and

384 Ypey 1973b, 454.
385 Wynia 1979, 68.
386 Van Es/Hulst, in prep.
387 E.g. Ament 1970, 131–51.
388 Steuer 1982, 460–1.
389 Van Es 1964: at least the almost entirely disturbed grave no. 123, with a snaffle bit; presumably also the (robbed?) grave no. 144 with part of a spatha, an ango, and (fragments of) a shield.
390 An impression of the size is given by a letter of Dec. 17, 1715, from G. Cuper (see Zadoks-Josephus Jitta 1950), who estimated the total value at *thien duisent of veele meer duysent gulden* (ten thousand or many more thousands of guilders), at the same time giving the values of 3 medallions as 70–75, 80–85, and 140 guilders, respectively, and that of two coins as 7 guilders each (although he paid 19–20 guilders for them). If this estimate is at least in the correct order of magnitude (Janssen 1852, 168 mentions a value of *f* 10,000 to *f* 12,000), the hoard must have had a value of some 500 solidi at least!

Galla Placida, have survived. Of the coins, specimens are mentioned from Constantinus I and his sons, Valens, Gratianus, and Johannes, as well as a Valentinianus. The hoard can thus be dated to after AD 425 and was presumably buried around the mid-5th century.

2 Site 97. Homoet–De Hoge Woerd. Tremissis from Metz, monetarius Garoaldus, AD 630–640 (660).

3 Site 171. Flieren-dorp. Tremissis Anastasius, probably Visigothic imitation, AD 491–518.

4 Site 403(?). Nijmegen–Valkhof(?). In his discussion of the coins from Nijmegen collections, Daniëls mentions two aurei of Valentinianus III (AD 425–455).[391] The findspot 'Valkhof', or indeed 'Nijmegen', is not absolutely certain.

5 Site 468(?). Dieden. Tremissis Justinus II (AD 565–578). Gallic imitation. The exact findspot is unknown.[392]

6 Demen (c. 171/425). Tremissis Justinus I (AD 518–527). The exact findspot is unknown.[393]

7 Beers (c. 186/415). Medallion, cast of aureus of Antoninus Pius, dated to c. AD 630.[394] Found in 1802 between Cuijk and Beers.

8 Site 445. Millingen-kerk. Gold coin Justinianus (AD 527–565), found in 1847 close to the church of Millingen.[395]

9 Site 514. Escharen–Graafsche Raam. Hoard of 66 solidi and tremisses, found in 1897. Total gold weight c. 108 grams. The hoard must have been buried around AD 600, the latest coins having been minted during the reign of the emperor Maurice (AD 582–602).[396] The hoard contained two solidi and five tremisses of Magnia vico, minted in Niomago (fig. 92). Although not entirely certain, this Niomago can probably be identified as Noviomagus–Nijmegen,[397] and contribute to the evaluation of the importance of sites 401/403 in post Roman times.

10 Escharen. At a very short distance from the large hoard, a tremissis of Justinianus (AD 527–565) found in 1983, and a small hoard of nine, presumably early-8th-century, sceattas.[398]

In addition to the above-mentioned finds, two other stray coins should be mentioned here. They are both gold coins of Maioranus (AD 457–461) and were found in



Fig. 92 Gold tremissis and solidus from the Escharen hoard (site 514), probably minted in Niomago-Nijmegen. Scale 2:1. Photo Koninklijk Penningkabinet, Den Haag.

Lienden in the Betuwe, 2 km west of the area covered by Appendix 5 (c. square 164/439) and 's Heerenberg in the Montferland, only 4 km east of our area (c. square 214/432).[399] Together with the tremissis of Anastastius from site 171, these coins more or less bridge the gap between the perhaps still 'late-Roman' gold finds from sites 28 and 403 and those of the 6th to early-8th centuries.

In fact, the available evidence indicates that gold coinage continued to be imported into the river area after the Roman period. The quantities are less than during late-Roman times, but that is primarily due to the extremely rich hoards which were buried in a very short period from the end of the 4th to the early-5th-centuries, culminating in the first hoard from Velp (site 28). Apparently,

391 Daniëls 1950, 3. The aurei of Valentinianus III mentioned in *ER* III, 97 and by De Boone 1954, 224, sub B, are probably the same.

392 Boersma 1963, 55, sub 53c.

393 Hermans 1865, 27; Boersma 1963, 55, sub 53a.

394 Boersma 1963, 30, sub 8a.

395 Spann 1967, 15.

396 Lafaurie 1960.

397 The other possibility is Noviomagus-Neumagen (Blok 1979, 25–7), but Nijmegen is generally accepted (also e.g. Ewig 1980, 45).

398 *JROB* 1983, 151 (tremissis) and Verwers 1983, 42–3 (sceattas).

399 De Boone 1954, 140.

storing wealth in gold remained possible and, if the evidence from the Escharen hoard is interpreted correctly, gold was already minted locally in the 6th century.
The list of finds is perhaps large enough to conclude that a system whereby gold was retrieved by a central authority, such as may have been the case in the late-Roman period, did not exist or function effectively.[400] That is consistent with, for example, the information provided by the *Historia Francorum* of Gregorius, Bishop of Tours, on the large royal hoards which were continually depleted by costly gifts which subsequently must have spread widely, a substantial number eventually ending up in burials.[401] The original royal hoards seem to have been acquired from outside sources by such means as conquest, confiscation, or ransom rather than by taxation, and most of the gold moved from the top downwards or between local elites, in a horizontal direction, and not upwards again.[402] In any case, the number of stray Merovingian gold coins is larger than those of the Late-Roman and considerably larger than those of the Middle-Roman Period. Of course, this phenomenon can also be attributed to the insecurity of the area and the unstable political and economic environment from the late-3rd century onwards. In that case, the argumentation implies, in fact, an interpretation of the stray gold coins as hoards but that is not exactly a foolhardy assumption. The relative value of gold coins was enormous, as was so pointedly illustrated by Zadoks, who concluded that the late-Roman hoard of Hapert (Province of North Brabant), which consisted of no less than 2598 small bronze coins, must not have been worth much more than half a solidus![403]

400 Which does, of course, not mean that there were no taxes and other 'internal' means of income. See Weidemann 1982, 327–38.
401 A process leading to the edict of Theoderic (AD 483–526) prohibiting excessively valuable grave goods and even sanctioning the robbing of graves in some cases (cited by Steuer 1982, 500).
402 The mechanisms involved in the circulation of gold are, of course, more complex than suggested here and urgently need further study, cf. Van Regteren Altena/Theuws 1984.
403 Zadoks-Josephus Jitta 1955, 108. One solidus was sufficient to buy provisions for a whole year (Steuer 1982, 451).

9 THE ROMAN FORT AT ARNHEM-MEINERSWIJK

9.1 INTRODUCTION

The site of the fort in the polder Meinerswijk, across the Rhine from Arnhem, was discovered in the course of an inspection of previously ignored ancient settlement soils recorded by a soil survey of the Betuwe.[1] A small excavation, intended to provide information on the suspected nature and on the chronology of this new site, was carried out in September 1979. The other circumstances leading to this enterprise have been outlined in chapter 8 and in the preliminary reports.[2] The geological situation, the interpretation of which proved to be of much wider relevance and first led to new insights in the Roman Period fluvial system in the northeastern part of the river area, was discussed in chapter 3.[3]
The size of the site as indicated by the ancient settlement soil is give in fig. 18. From profiles AB and CD, it appears that the easternmost extension of the soil is virtually identical to that of the original settlement, while only a small part of the formerly settled area in the south was eroded by a late-Medieval or even later channel coming from the east.[4] Precise information on the western boundary is lacking. Part of the area has been dug out, presumably for the clay, but that is likely to have occurred even after it had been eroded by the river. Data on the possibly still surviving part of the site to the north, the entire area behind the road north of the excavation trench (fig. 93), are completely lacking. It is covered by brick factories and inaccessible for any observations (see fig. 11). At least the finds from site 23 (fig. 93) give some idea about the possible maximum extension of the original site which may well have suffered greatly from erosion by the Rhine which, in Roman times, presumably followed a course north of site 23.
The excavation trench, although near the centre of the area which still survives, is thus probably located in the southern part of the Roman Period settlement, or rather settlements, considering, as mentioned in the previous

1 Zegers/Zandbergen 1958. See also chapter 3, pp. 71–2.
2 Willems 1980a and b; chapter 8, pp. 255–6.
3 Chapter 3, pp. 54–5 and fig. 18.
4 This is the so-called *groene rivier* (green river), still used as an additional outlet for the Rhine during times of very high water-levels (Kuper a.o. 1978, 3–4).

chapter, that it is likely to have consisted of a military fortification with associated civilian habitation.
The trench itself, due to limitations of time and money as well as the objective of the excavation, measured only 5 × 40 m, later extended by an even smaller trench of some 2 × 11 m to the south. Due to the complicated and very deep stratification of the site, only part of this area was excavated. The northern half of the trench, with the foundation trenches of stone walls, was left intact after removal of two levels, and only the southern part was investigated to deeper levels although, unfortunately, not to virgin soil. Due to unfavourable weather conditions but especially the proximity to the river, normal pumping became ineffective at 2.5 m below the surface and futile at a depth of 2.8 m when a sterile level was still not reached. From the geological profile CD in fig. 18, (chapter 3, p. 54), the northern part of which coincides with the west profile of the excavation trench, it could be deduced that virgin soil may only be found at 3.1 m to as deep as 3.5 m below the present-day surface. It cannot, however, be taken for granted that the first occupation of the site coincides with the top of the sand and gravel of the pre-Roman channel deposits. At least the lowest parts of the sandy clay on top of these deposits may also predate the first human occupation, although the pollen analysis indicates that at the very least an inhabited site cannot have been very far away.[5]

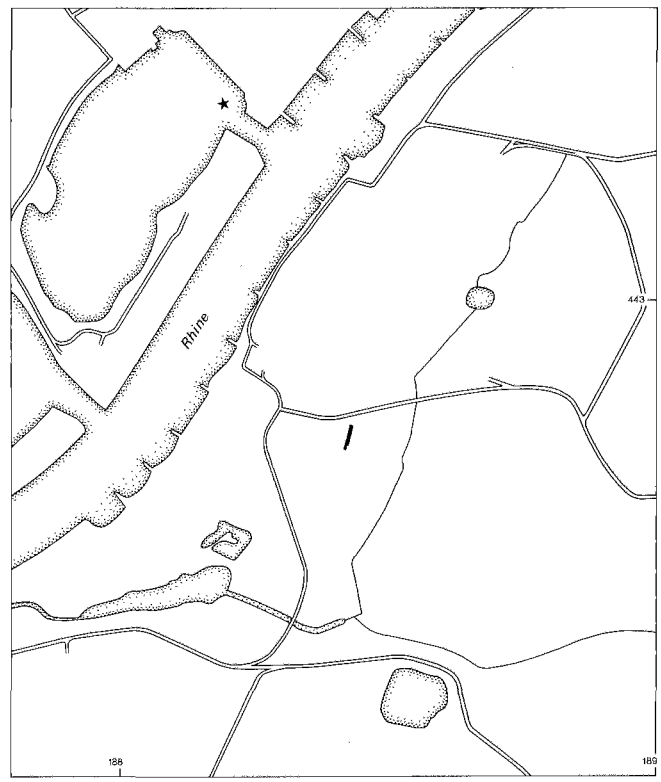


Fig. 93 Arnhem-Meinerswijk. Situation of the excavation trench and findspot (star) of the dredged-up finds from site 23.

9.2 THE EXCAVATION

An overview of the excavated features is presented in fig. 94.[6] It shows only one complete plan, at the second excavation level, some 40–50 cm below the surface. Because the circumstances did not allow complete excavation, it was decided to leave the robber trenches intact and to concentrate further efforts on the southern part of the trench with a dark-black fill in which some features, notably a possible ditch, were nevertheless visible.
The robber trenches clearly indicate a large stone building with a wall some 6 m to the south of it. As shown by the material in the robber trenches, the walls must have been built of tuff. The width of the trenches is 1.50–1.40 m for the outer wall and 1 m for the inner wall. As shown by a section through the latter (fig. 94, profile AB), this probably corresponds fairly closely to the original width of the walls. There is no evidence to date the removal of the stone, but it is at least clear that it was done effectively with a minimum of effort, and only to obtain the tuff. The actual foundation was left intact. It consisted of a 40-cm-thick bed of large blocks of greenish sandstone with relatively little quartz, which might be described as *grauwacke*. In their turn, these were founded on (probably six) rows of closely spaced, rammed in oak posts, a common method of construction on an unstable or damp subsoil and also used for the nearby temples of Elst (site 105).[7]
There are no indications about the size or purpose of the building. Even though the robber trenches reach virtually to the present-day surface, they were already clearly visible at the first excavation level at 15 cm below

5 See Teunissen a.o. 1985, 22.
6 On a separate map: Appendix 6.
7 Bogaers 1955, 47 (see also Haalebos 1977, 37, note 59). Specimens of wood from this and other features were submitted to Dr W.A. Casparie (BAI Groningen). All constructional timber proved to be oak.

the surface, it proved to be almost impossible to trace them. Several attempts by an electrical resistivity survey during and after the excavation produced no results.[8] Air-photography in 1979 and 1981 also remained without success, although one attempt by the National Aerospace Laboratory in 1979, with an infrared line-scanner (fig. 95), showed a probably more or less rectangular or square structure some 60 m southwest of the excavation trench. Borings at that spot have shown that the features do not represent walls, but they might be ditches of some sort. Other borings, around the excavation trench, showed that the southern wall of the building extended for at least 10 m to the east.

Some 6 m south of the building, another robber trench is indicated on fig. 94. It reaches only half-way into the excavated plane and in the preliminary publication it has been assumed that there must have been some sort of opening here. However, at the first excavation level the robber trench went straight across, as indicated by the interrupted line. In view of the efficiency of the methods used to remove the stone walls, it is not impossible but rather unlikely that a trench would have been dug where there was no stone. Unless this is true after all, there should thus have been a continuous wall but the lack of a foundation indicates that it may originally have had an opening which was later closed. If there was such an opening, it may correspond with an interruption in one of the ditches, if that feature is indeed a ditch.

It is also possible that the wall is not contemporaneous with the building because there is a slight difference in orientation between the two. Because there is no direct stratigraphical relation between both features, a discussion of the possible significance of this will be returned to after the presentation of the stratigraphical data.

These data were obtained in the southern part of the excavation trench, which showed a very complex picture of natural deposits and layers due to the artificial raising of the soil as well as features due to both natural and human agents. The stratigraphy could not be properly interpreted from the various plans during the excavation, but only after the entire profiles became available. Especially because of the limited width of the excavation trench, this did not interfere with the attribution of finds to relevant stratigraphical units in later analysis, with the exception of some material from the lower levels: from c. 2 m below the surface layers of brown, overlying greyish-blue, clay were encountered and finds kept apart. Only after studying the profiles in the field did it become clear that the border between the two was crossed by archaeologically relevant levels and was merely a natural phenomenon, representing reduction of the clay (from brown to greyish-blue).

In the following interpretation of the stratigraphy, the attempt has been made to obtain a more or less general archaeological periodization for the site. The excavated area is of course much too small to guarantee that this



Fig. 95 Arnhem-Meinerswijk. Air photograph with infra-red line-scanner from a height of 500 m showing a rectangular (?) structure (corners indicated by arrows). The black line in the lighter area on the right indicates the position of the excavation trench, the black-and-white dots are cattle (Photo National Aerospace Laboratory, Amsterdam).

8 Two attempts during and shortly after the excavation by Ir R.S. Sporry, of the Adviesbureau Arnhem BV (Heidemij Group), and one in 1980 with equipment provided by Dr I. Scollar (Rheinisches Landesmuseum Bonn) failed, presumably because the subsoil in Meinerswijk is too wet.

periodization will long outlive more extensive excavations in the future, but this is all the more reason not to attempt a very detailed periodization which might just be accidental and relevant only to this part of the site.

9.2.1 Period 1

As mentioned earlier, it is not certain that the phase of the occupation history of the site designated here as Period 1 is indeed the first. Earlier features and, more significantly, earlier finds are, however, lacking. The trajectory in profile CD belonging to Period 1 is indicated in fig. 96. It consists of an up to 1-m-thick deposit of greyish-blue and brown sandy clay which is only very slightly polluted (fig. 96, H). It does contain, however, a quite considerable number of artefacts throughout the fill.

At the moment, it is not quite certain whether the deposition of this clay was due to human or to natural agents, or to both, although the possibility of natural deposition is certainly the least probable one. In view of the period of time involved and the thickness of the deposit, completely natural sedimentation can almost be ruled out.[9]

The original top of this deposit has been preserved in the southern part of profile CD, where it is covered by c. 10-cm thick layers of gravel reaching to 8.90 m NAP, with a maximum width of 4.5 m (fig. 96, G). There can be little doubt that this zone of gravel, which has the same orientation as most of the other (later) features in the excavation trench, represents (the remains of) a metalled road.

Immediately north of this road the profile shows two depressions, reaching below the deepest excavation level (fig. 96, B and E). As already indicated in the preliminary report, it is possible that these represent the upper part of two parallel ditches. If this is true, the ditches should belong to Period 2, although it cannot be excluded that they in fact still belong to Period 1. The original surface of the deposit in which they were dug has almost completely been eroded by a later gully (fig. 96, D), which is an additional argument for the ditches because at times of high water gullies follow the easiest path along depressions such as ditches. The southern slope of the gully shows bands of washed-in gravel, originating from an eroded part of the road (fig. 96, F).

The northern slope of the northern depression is more interesting, because it is covered by an up to 20-cm-thick layer of humic brown sand (fig. 96, A). The same layer is also present on the southern slope of this depression although more vaguely and perhaps in an eroded and redeposited state (fig. 96, C). The pollen analysis of this same layer in profile CG (see Teunissen a.o. 1985, figs. 3 and 4) has shown that it, as well as the fill of the gully directly above, contains an unusual and very high amount of *Calluna* pollen. This indicates that the surface of the slope and perhaps of the entire inhabited area north of the road has been deliberately covered with sand and heather, presumably harvested on the Veluwe north of the Rhine. There are no definite archaeological indications that sods were involved. Their transport to the site would have required an enormous labour investment but that is in itself not at all impossible. The sandy nature of the layer may well be an indication for sods, even though they could not be seen in the profile. In addition, the pollen diagram shows rather important deviations, limited to the trajectory through this layer, notably the minimum values for *Compositae, Cyperaceae, Dryopteris t*, and allochtone pollen in addition to the high value for *Calluna*, which at least leave open the possibility of sods.

Undoubtedly, this cover of the surface was applied to gain a surer and drier footing, perhaps also to prevent too much erosion causing a rapid silting up of the ditches (if there were any). Comparable measures, such as covering the slope of ditches with sods, are known for example from Nijmegen. Covering the surface of low-lying and wet ground with a carpet of harvested plants is also known from other sites, such as the bracken covers found in Vindolanda.[10] The pollen analysis has, by the way, also provided evidence for the use of bracken (*Pteridium aquilinum*) from the deepest (unexcavated) level up to the beginning of the levels ascribed to Period 3.[11] It is quite likely, therefore, that bracken was harvested on the Veluwe as well, and for a longer time.

The 'heather' layer, whether belonging to this period or the next (it did not contain any finds), was in any case due to artificial raising of the soil. The genesis of the underlying deposit of greyish-blue and brown sandy clay is more difficult to interpret. It contains a chronologically rather homogeneous group of pottery throughout the

9 See also Teunissen a.o. 1985.

10 Birley 1977, 122 and 130–1. Perhaps also relevant in this context is the evidence for the use of a specially imported (because it grows only in a salt-rich environment) variety of rush (*Juncus gerardii*) in the fort at Woerden (Bogaers/Haalebos 1983, 309).

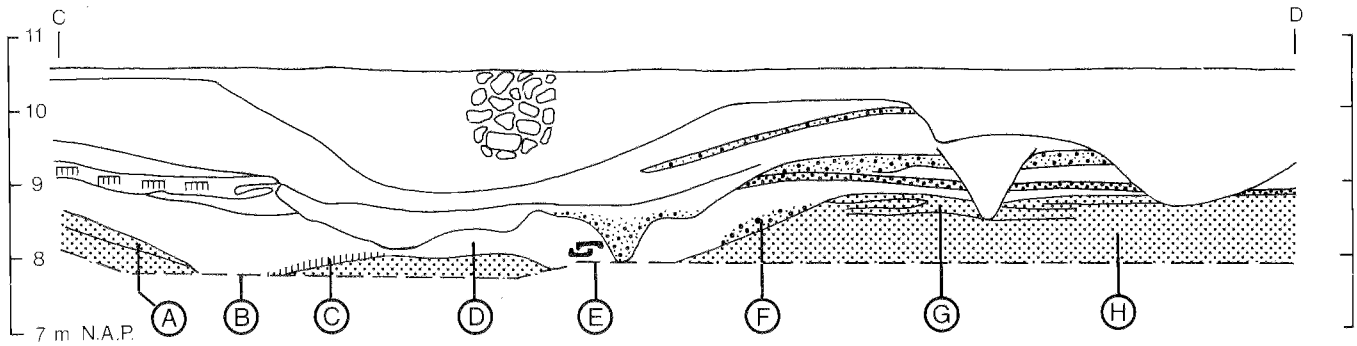


Fig. 96 Meinerswijk, profile CD, Period 1. Relevant deposits are shaded. A-H, see the text.

entire stratum which at least proves that it does not predate the occupation and that its formation took place in a relatively short period of time. As mentioned earlier, this makes a completely natural deposition process rather unlikely. On the other hand, artificial deposition would have left some traces and the pollen diagram shows no indications of disturbances. For the moment, the most reasonable interpretation seems to be that the process of deposition was natural in principe but greatly accelerated by human influence: clay from the immediate surroundings (and thus occasionally artefacts) being added.

The finds

With the exception of one find from the gravel of the road, all material originates from the thick deposit of sandy clay indicated in fig. 96. These are finds collected after the correct interpretation of the profile and the recognition of the sharp border between the greyish-blue and the brown clay as a natural phenomenon as mentioned above. The result of its earlier misinterpretation as an archaeologically relevant border is that there are two groups of finds collected before then, which cannot be attributed with certainty to one archaeological stratum. For the present discussion only one of these (no. MW 1-0-23) is relevant. It may contain some material from levels ascribed to Period 1, although the vast majority can only belong to Period 2 and a small fraction might be from Period 3.[12] It will be discussed in the next paragraph. Relevant material from the definitely Period 1 deposits concerns the following finds:

11 See Teunissen a.o. 1985, 18, 21–2.

12 The other group (no. MW 1-0-24) is only very small and will be omitted from the discussion because it contains no relevant finds.

Terra sigillata

1 Fragments of an almost complete plate Haltern 2 (fig. 97, 1) with missing foot-stand with a central stamp VTILIS (see chapter 6.2.1, p. 157 and fig. 33, no. 51).

2 Wall-fragment of a plate Haltern 2, probably from the same plate as no. 1.

3 Bottom fragment of a dish, presumably Haltern 2 (fig. 97, 2), with a central stamp CLA (see chapter 6.2.1, p. 153 and fig. 33, no. 10).

4 Wall-fragment of a cup, possibly Haltern 8.

5 Wall-fragment of a dish Haltern 2.

Gallo-Belgic ware

1 Two orange-to-brown coloured base-fragments, presumably of butt-beakers Haltern 84 (fig. 36, nos. 2–3).

2 Two wall-fragments, at least one of a chocolate-brown butt-beaker Haltern 85 (fig. 36, no. 4) decorated with wavy lines.

3 Two wall-fragments of cork-urns Haltern 58/91.

4 Base-fragment of a terra nigra cup with foot-stand, probably form Haltern 78–80 (fig. 97, 3).

Smooth ware

1 Neck and rim of two flagons Haltern 47, the neck-fragment with a 4-ribbed handle (fig. 97, 4–5). Another 4-ribbed handle (fig. 97, 7) might be from the same flagon as the rim-fragment.

2 Rim of a flagon Hofheim 50 (fig. 97, 6).

3 Neck of a two-handled flagon with neck-rings and 3-ribbed handles, Hofheim 58 (fig. 97, 8).

4 Fragments of five two-handled flagons Haltern 52, four of which have 3-ribbed handles which is an early

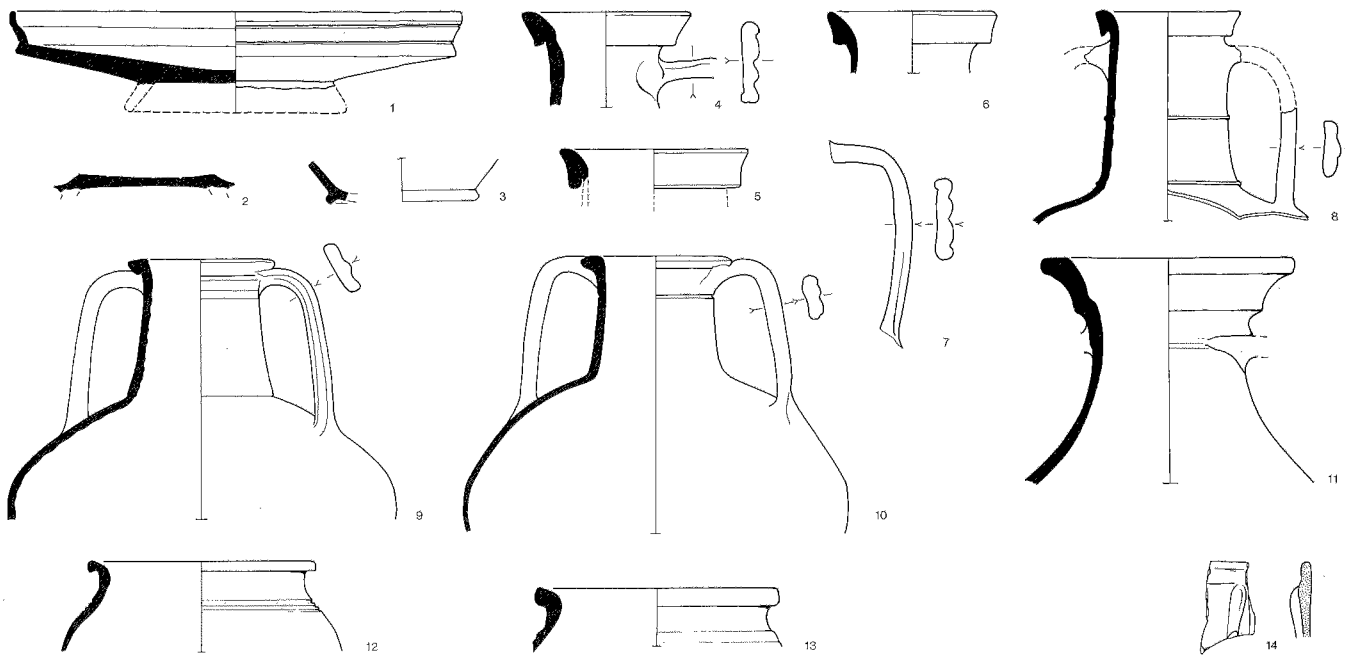


Fig. 97 Meinerswijk, finds from Period 1 deposits, 1–13 pottery, 14 glass.

characteristic (figs. 97, 9–10).[13] The only find from the road-gravel, a 3-ribbed handle, belongs to this group.

5 Neck of large two-handled flagon with a flat rim and very slight neck-ring (fig. 97, 11). Typologically, the specimen may be compared to Oberaden 52 (Stuart 133), but the jugs with a neck-ring (but a very different rim) Oberaden 53/Haltern 53 may also be related.[14] The shape of the neck does not, however, suggest a very early date.

6 Pointed amphora-base and wall-fragments of oil-amphorae Dressel 20.[15]

7 Two wall-fragments of flagons with graffiti on the shoulder (fig. 98). One is an unreadable fragment. The other is probably a military inscription which could be read as > LIICV, and interpreted as: (centurio) LEG (ionis) V [Alaudae].[16] The *legio V* was stationed in nearby Xan-

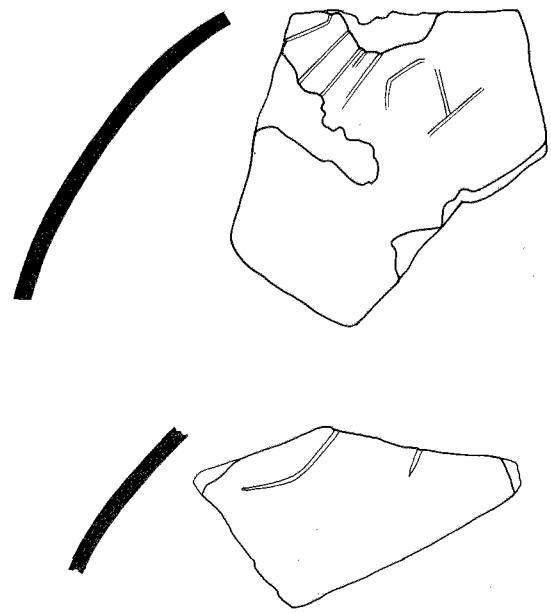


Fig. 98 Meinerswijk, graffiti on flagons from Period 1. Scale 1:2.

13 Cf. Stuart 1977, 56.

14 See also the discussion in Schönberger/Simon 1976, 95–6 (types 37–38).

15 I am grateful to Dr J. van der Werff (Amersfoort) for his help in the determination of the amphorae.

16 Reading by J.E. Bogaers (Nijmegen).

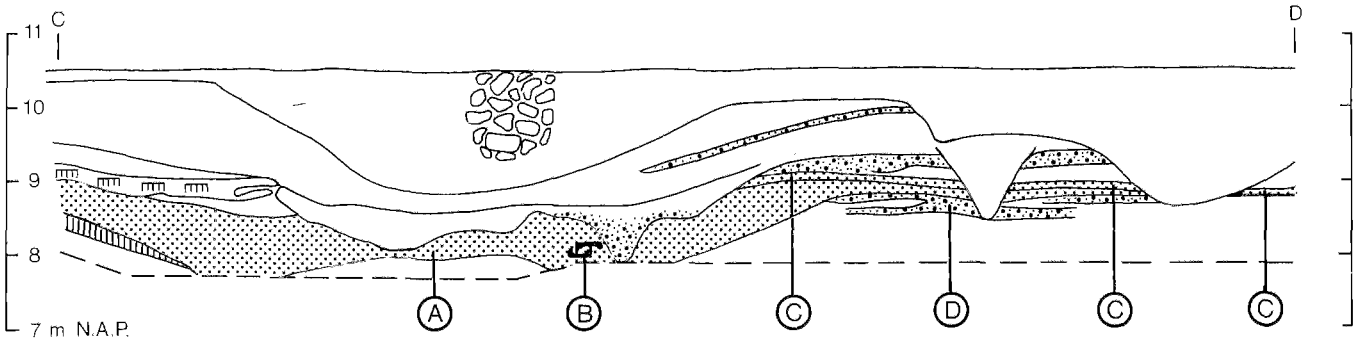


Fig. 99 Meinerswijk, profile CD, Period 2. Relevant deposits are shaded. A-D, see the text.

ten from AD 9-69/70. An alternative reading might be L III C Y, L(egionis) III CY(renaicae). Although perhaps not entirely impossible,[17] the normal abbreviation of Cyrenaica seems to be CYR instead of CY and the interpretation of the first sign as an L is not very plausible. In contrast, the second sign may well have been an L and there are no real objections against a reading of the last as V instead of Y. Nevertheless, the first reading of the inscription cannot be taken as definite.

Coarse ware

1 Rim-fragments of two cooking-pots with everted rim (fig. 97, 12–13) Stuart 201.

2 Fragment of the neck of a jug, no rim, presumably Haltern 54/Hofheim 86A.

Glass

Rim-fragment of a pillar-moulded bowl of monochrome, bluish green glass, Isings 1957, type 3 (fig. 97, 14).

Metal

Bronze terminal for a strap with a rivet still in its place (fig. 104, 1).

Dating

The dating of Period 1 is primarily determined by the terra sigillata. The forms (service 2) and the stamps indicate that we are dealing with a late phase in the production of Arretine ware. On the other hand, not a single fragment of South-Gaulish ware is present. Although that may not be interpreted as hard evidence, in view of the relatively small total numbers of pottery, the most probable dating is surely the second decade AD. This is confirmed by the available smooth ware, which is dominated by Haltern forms 52 (with 3-ribbed handles) and 47. But there are also some flagon-necks which are more correctly identified as a Hofheim-type. The combination of these forms in a deposit which can hardly be later than c. AD 20 and is also very likely to post-date the occupation of the Haltern camp (ended in AD 9) is, in fact, exactly what was to be expected. It agrees well with Stuart's conclusions on the period between Haltern and Hofheim.[18]

As far as the other pottery and glass is concerned, the wares and forms fit into the proposed dating. No fragments of fine wares, which could have contributed to the dating, are available.

9.2.2 Period 2

With the exception of the two possible ditches mentioned in the previous paragraph, there are no features which can be ascribed to Period 2. From the profiles (see figs. 94 and 99) it appears that the surface of the previous settlement at the site of the excavation has been largely eroded by a natural gully, following the course of the presumed ditches (fig. 99, A). Part of the gravel of the road (fig. 99, D), the top of which may also belong to Period 2, was washed into this gully (fig. 96, F), as well as a wooden conduit-pipe or drain (figs. 99, B and 101). The channel of the gully seems to have been filled in fairly quickly and probably by natural means: except for the washed-in gravel on the southern slope the fill is homogeneous.

The fill of the gully is sealed by a partly preserved and up to-20-cm thick strongly polluted dark layer, contain-

17 See RE XII2, 1508, s.v. *Legio* [Ritterling] and especially Schillinger-Häfele 1977, 531–2 (no. 152).

18 Stuart 1977, 15–6.

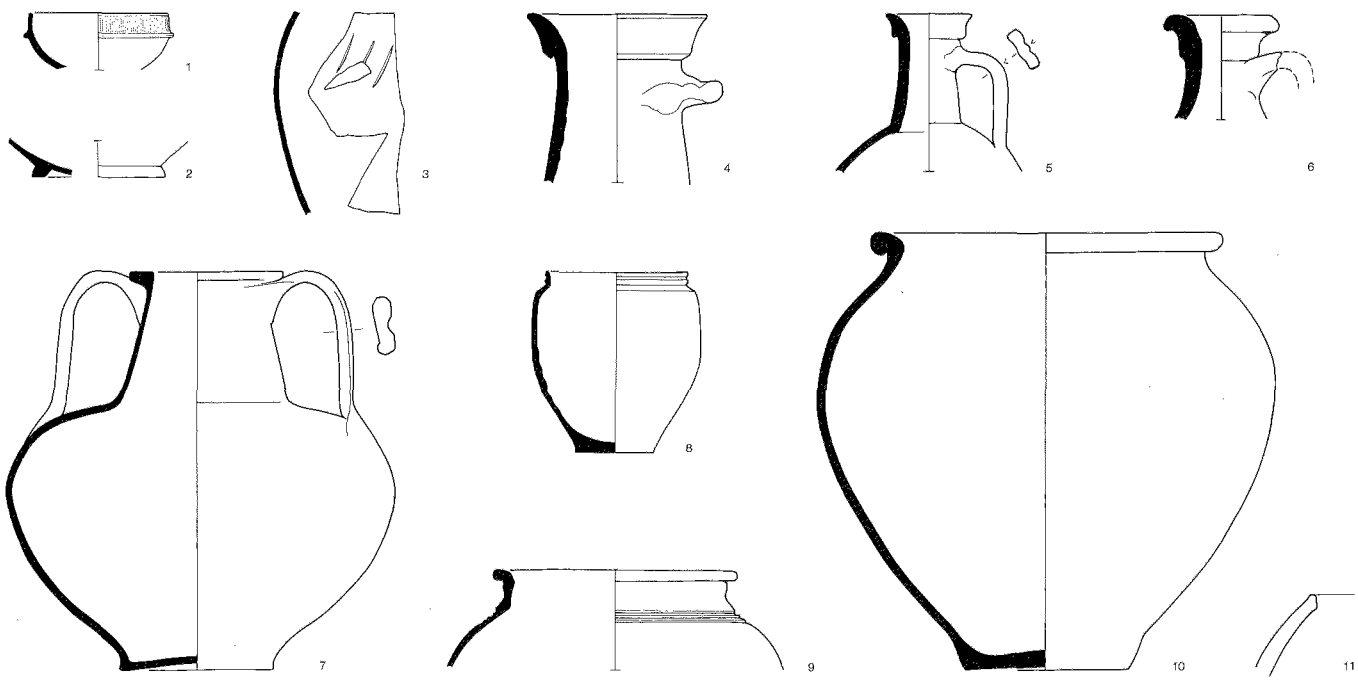


Fig. 100 Meinerswijk, pottery from Period 2 deposits.

ing much charcoal (fig. 99, C). As visible on profile EF (fig. 94), it gently slopes down to the south. It seems as if Period 2, at least at the site of the excavation, is characterised by two calamities: it starts with a flood and ends with a fire. The pollen analysis of the trajectory through the fill of the gully indicates more or less a return to the relative frequencies observed underneath the 'heather' layer.

The finds

All except one of the relevant finds originate from the fill of the gully. Unfortunately, as explained in the previous paragraph, the finds probably contain a few sherds from the underlying Period 1 deposit and also from the gully-fill above, ascribed to Period 3.

Terra sigillata

1 Large fragment of a bowl Drag 29 (see chapter 6.2.1, p. 143 and fig. 26, no. 11), Neronian or early-Vespasian and presumably a product of Passenus. This is the only sherd from the layer with charcoal overlying the gully-fill.

2 Rim and base fragment of two early South-Gaulish cups Drag 24/25 (fig. 100, 1–2).

3 Base- and wall-fragment of two South-Gaulish cups Drag 27, the base with an unreadable stamp, and a base fragment of a dish.

Gallo-Belgic ware

1 Wall-fragments of a butt-beaker decorated with apparently only one band of applied ribs and manufactured in a well-fired smooth white ware (fig. 100, 3). The beaker is possibly a further development from the ribbed beaker (*Grätenbecher*), Oberaden type 103.

2 Base/wall-fragments of a globular beaker with barbotine spikes (*Stachelbecher*) in terra nigra, Hofheim type 106.

3 Wall-fragment of an ovoid beaker decorated in barbotine (*Perlurne*), Hofheim type 118.

Smooth ware

1 Neck of a flagon Hofheim 50 (fig. 90, 4).

2 Neck of a small flagon Hofheim 51 (fig. 100, 5).

3 Neck of a flagon which seems to be of the type Stuart 110B (fig. 100, 6). Although this mid-2nd-century type does clearly differ from the other finds, it is *not* from find no MW 1-0-23, but from a secure stratigraphical posi-

tion. Even though the inside of the rim is only very slightly concave, it may be that we are dealing with a variant of Hofheim 53.[19]
4 Complete flagon Haltern 52/Hofheim 57 with two-ribbed handles (fig. 100, 7).
5 Wall- and base-fragments of oil-amphorae Dressel 20, neck-fragment of a wine-amphora Pélichet (1946) 47, and wall-fragment of a fish sauce-amphora Dressel 7-11/Pélichet 46.

Coarse ware
1 Complete beaker Hofheim 85 A/C (Stuart 204) (fig. 100, 8) and one rim-fragment of an identical specimen.
2 One complete and one fragmentary cooking-pot with everted rim Stuart 201 (fig. 100, 9–10).

Native ware
Rim-fragment of a cooking-pot with inturned, faceted rim (fig. 100, 11).

Building material
1 Two fragments of tuff.
2 One fragment of limestone.
All three fragments are from find no. MW 1-0-23, which means they originate from the level of reduced, greyish-blue clay. Because not even the tiniest crumb of building material was observed in the layers ascribed to Period 2 in the profiles, the (fairly large) stone blocks probably belong to Period 3. Only the deepest part of the gully-fill from that phase (see fig. 102, B) consisted of the greyish-blue clay and that is, in fact, the most logical place for such fairly heavy objects to be deposited.

Organic material[20]
1 Parts of washed-down oak posts.
2 Part of a wooden conduit-pipe, consisting of a hollowed-out trunk covered by a lid (fig. 101). In contrast to all other wood (constructional timber), the pipe and the lid were both manufactured from alder which is not very durable but easy to work. Comparable pipes in oak are known from other sites.[21]

Dating
Apart from one or two exceptions, the datable material presents a fairly coherent picture. The finds from the gully-fill are no later than Vespasian and more probably pre-Flavian. The 'charcoal' layer above it is dated by the Neronian or early-Vespasian bowl Drag 29. The combination of these facts leads almost inescapably to the conclusion that we must be dealing with the ubiquitous 'layer of AD 69' and that Meinerswijk suffered the same fate as so many other forts along the Rhine which were destroyed during the Batavian revolt.
As far as the beginning of Period 2 is concerned, the situation is more difficult to evaluate. If any finds actually originating from the Period 1 deposit are among the finds, these cannot be identified with certainty. In principle, the available evidence allows the conclusion that there was continuous occupation over Periods 1 and 2. The flooding and the gully may in addition have been very local phenomena and of no consequence for the site as a whole. For the moment, however, this has to remain an assumption.
With the exception of two fragments of tuff and one of limestone, no building material except wood was found. The fragments belong to find no. MW 1-0-23 and thus very likely originate from the deposit above the gully-fill. The total absence of other stone, even in the charcoal layer, support this conclusion.

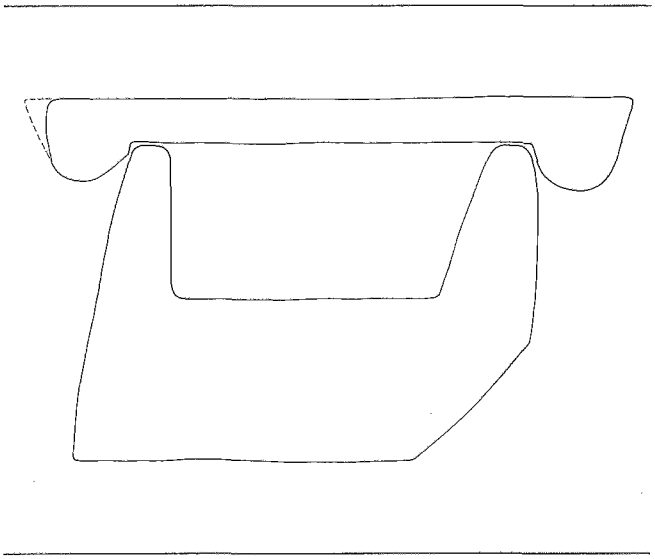


Fig. 101 Meinerswijk, section across a conduit-pipe and lid from alder wood. For the original position in profile CD, see fig. 99,B. Scale 1:4.

19 See esp. Ritterling 1912, 283, fig. 64, 1–7.
20 The zoological material from Arnhem-Meinerswijk has been examined by R. Lauwerier (BAI) and will be published separately.
21 See Jacobi 1934, 52–7 and fig. 14, 8.

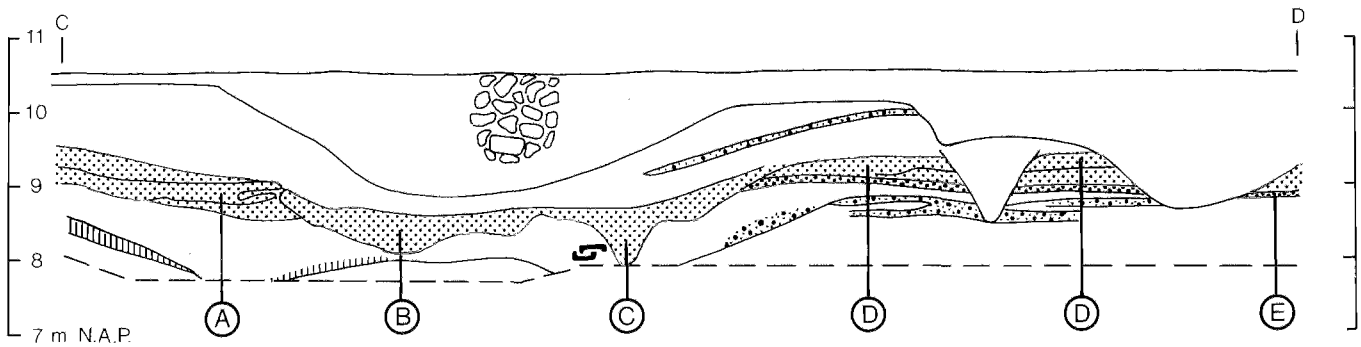


Fig. 102 Meinerswijk, profile CD, Period 3. Relevant deposits are shaded. A-E, see the text.

9.2.3 Period 3

From the profiles (figs. 94 and 102) it is evident that the surface of the site was raised after the events which led to the formation of the charcoal layer (fig. 102, E). In the southern part of the trench this layer is covered by an up to 30-cm-thick deposit of greyish-brown clay (fig. 94, profile EF). In the northern part, the remaining depression of the gully was filled with the same material and covered with a 20–30-cm-thick layer of sods (fig. 102, A). As appears from the pollen diagram these sods were probably cut locally, in the immediate surroundings of the site. There are no obvious and sudden changes in the trajectory through the sods.

At exactly the same spot as the previous one(s), a new road was built, with a metalling of gravel with a maximum remaining thickness of 30 cm (fig. 102, D). North of and apparently parallel to this road, a V-shaped ditch was dug which originally must have been between 1.50 and 2 m wide and 1.25 to 1.50 m deep (fig. 102, C). The function of this ditch is unknown. It is perhaps more likely to have been a road ditch than part of the defence works, but the relation to the road and raised surface is lost because the ditch must have attracted water during a high-water period and once more a gully was formed (fig. 102, B) which eroded part of the previous gully-fill and the layer of sods above it, as well as the northern side of the road, most of the gravel landing in the ditch and completely filling the lower (50–60 cm) part of it. The lowest part of this gully silted up or, less likely, was filled in again fairly quickly after the flooding.

22 See Bogaers/Haalebos 1976, 159 and Haalebos 1977, 238 and notes 12 and 13.

The finds

The finds from these various deposits belonging to Period 3 can thus be divided into two groups.

1 Finds from the sod layer overlying the gully-fill of Period 2 and from the clay deposit plus the road above the charcoal layer (fig. 102, A and D).

2 Finds from the – stratigraphically later – lower fill of the second gully and from the ditch (fig. 102, B and C).

These two groups are indicated separately.

Terra sigillata

1.1 Wall-fragment of a cylindrical bowl Drag 30 (see chapter 6.2.1, p. 143 and fig. 26, no. 12), Flavian, and a base-fragment of such a bowl.

1.2 Rim-fragments of two dishes Drag 15/17 (fig. 103, 1a–b). South-Gaulish ware.

1.3 Rim-fragments of two dishes Drag 18. South-Gaulish ware.

1.4 Wall-fragment of a cup Drag 27. South-Gaulish ware.

2.1 Wall-fragment of a bowl Drag 37 from La Madeleine (see chapter 6.2.1, p. 145 and fig. 28, no. 47).

2.2 Base-fragment of an early South-Gaulish dish with a graffito X.

2.3 Two rim-fragments of cups Drag 33.

2.4 One rim- and one wall-fragment of two plates Drag 31.

Fine wares

1.1 Wall/base-fragment of a colour-coated cup decorated in barbotine, Hofheim 22 (fig. 103, 2).

1.2 Rim-fragment of a dish of orange-brown 'fine Nijmegen' ware, Holwerda 1944, type 37b (fig. 103, 3).

2.1 Large fragment of a wine-sieve of 'fine Nijmegen' ware (fig. 103, 4). A similar vessel in the same ware has been found in the legionary fort in Nijmegen. Other specimens in different wares are known from Arentsburg, Zwammerdam, and Nijmegen.[22]

Gallo-Belgic ware
1.1 Fragments of one or two dishes of well-polished terra nigra with a metallic lustre, Hofheim 99.
1.2 Wall-fragment of an ovoid beaker decorated in barbotine (*Perlurne*), Hofheim 118.

Smooth ware
1.1 Rim of a flagon Stuart 106 (fig. 103, 5). A three-ribbed handled is probably from the same flagon.
1.2 Neck of a two-handled flagon Stuart 131.
1.3 Rim of a cylindrical jug with wall decorated with feather-rouletting, presumably the rare form Stuart 128 (fig. 103, 6).
1.4 Rims of mortaria with level bead-rim Stuart 149 and a wall-sided rim with narrow spout, Haltern 59/Hofheim 79 (fig. 103, 7).
1.5 Fragment of a plate with a very low rim and a low, double foot-ring (fig. 103, 9). Traces of wear on the foot-rings indicate that it was probably not a lid.
1.6 Fragments of oil-amphorae Dressel 20 and of fish sauce-amphorae Dressel 7–11/Pélichet 46.
2.1 Rim of mortarium with level bead-rim Stuart 149.
2.2 Pedestal of a tazza (incense cup).
2.3 Wall-fragment of a flagon with part of a graffito]vs (fig. 103, 8).
2.4 Wall/rim-fragment of a wall-sided mortarium Haltern 59/Hofheim 79.
2.5 Wall-fragments of a wine-amphora Pélichet 47.

Coarse ware
1.1 Rim-fragments of three cooking-pots Stuart 201.
1.2 Rim-fragments of three cooking-pots Stuart 210.
1.3 Neck of a jug Stuart 214B.
2.1 Rim-fragments of two jars Stuart 213A.
2.2 Rim-fragment of a dish Hofheim 94A.

Building material
1.1 Fragments of tegulae and one fragment of opus signinum.
1.2 Fragment of tuff.[23]
1.3 Fragment of slate.
1.4 Piece of burnt wattle-and-daub.
2.1 Two fragments of tegulae with cement.

Glass
1.1 Two fragments of blue and green glass of bottles Isings 1957, form 50.

23 The two blocks of tuff and one limestone fragment from find no. MW 1-0-23 should probably be added to this.

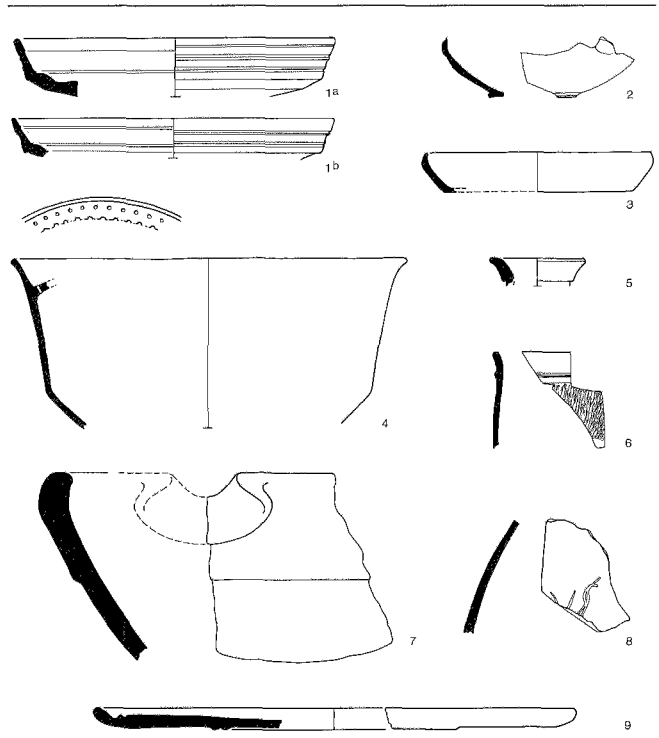


Fig. 103 Meinerswijk, pottery from Period 3 deposits.

1.2 Fragment of bluish glass of a cup or bowl Isings 12.
1.3 Fragment of blue-green glass of a bottle Isings 50 or 51.
1.4 Fragment of window-glass.
2.1 Fragment of blue glass, probably square bottle Isings 50.

Metal
1.1 Bronze coin, As of Nero, rev. Victoria, AD 64–68 cf. *RIC* 329 (fig. 104, 4).
1.2 Two bronze rings, diam. 23 and 35 mm (fig. 104, 2–3).
1.3 Iron L-shaped tumbler-lock lift key (fig. 104, 5).
1.4 Iron butt-end of a lance, still containing the wood and a nail (fig. 104, 6). Compare Ulbert 1970, *Taf.* 30, 497–9.
1.5 Iron dagger (*pugio*) with point broken off, remaining total length 28 cm, length of the tang 10 cm (fig. 104, 7). The leaf-shaped blade has no prominent midrib and is decorated by four parallel grooves. Although different from the 1st-century daggers with narrow blades and a

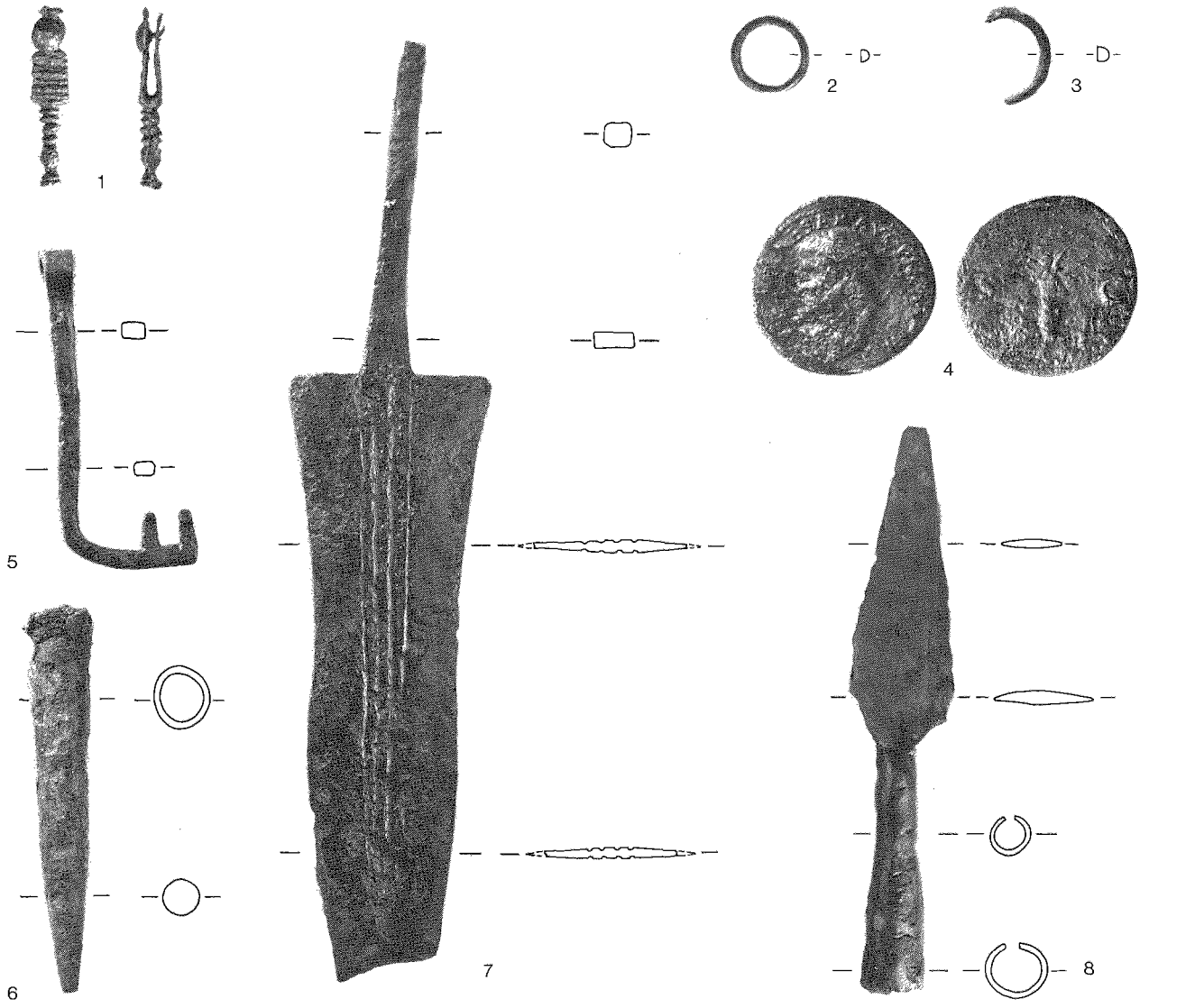


Fig. 104 Meinerswijk, metal objects from Periods 1 (no. 1) and 3 (nos. 2–8). Scale 1:2 (coin 1:1).

midrib, the form of the dagger is not necessarily indicative of a much later date.[24]

1.6 Leaf-shaped iron spearhead with missing point, remaining length 16.9 cm, with a circular-sectioned and split socket (fig. 104, 8).

Organic material

1.1 Part of a wooden post (oak).

1.2 Various oyster-shells.

1.3 Pointed end of an oak pole (fig. 105).

24 Cf. Herrmann 1969, 133. See also *Abb.* 3, 1–3.

Dating

The evidence from the previous phase implies that Period 3 can be dated from c. AD 70 onwards. Some material, such as the early wall-sided mortaria, the colour-coated cup Hofheim 22, and the base of an early South-Gaulish terra sigillata dish, is probably residual from earlier layers (*opspit*). Much of the terra sigillata (1.1–4) as well as the fine Nijmegen and Gallo-Belgic ware are evidence for a dating in the Flavian period.

The finds from the lower fill of the gully and the ditch contain, however, also material that is probably post-Flavian, notably some terra sigillata sherds (2.1 and 2.3–4). This indicates that the flooding which created the gully must have occurred in the first half of the 2nd century and probably well before AD 150. The lower gully-

fill is ascribed to Period 3 because major changes must have taken place only after it had been deposited and the youngest finds should date the end of that phase.
The available building material and small particles of brick and some of stone in the various layers indicate that at least some structures must have been built in stone and that there were tile-covered roofs.

9.2.4 Period 4

Stratigraphically, the phase which constitutes the fourth period of occupation of the site can be conveniently described as all those layers and features which overlie or cut those of Period 3 and which are beneath the upper dark fill ascribed to Periods 5 and 6 (fig. 106). The attribution of finds to this phase as a whole does not cause any problems. The interpretation and sequence of the various features belonging to Period 4 is, however, rather problematical in some respects.
It is clear that after the second flooding the surface of the site was raised again, and this time quite considerably, to almost 1 m above the surface of the sod layer and the road from Period 3 (fig. 106, A). A thin layer of stone and tile debris above the southern slope of the former gulley (fig. 106, B) and of some gravel above the sod

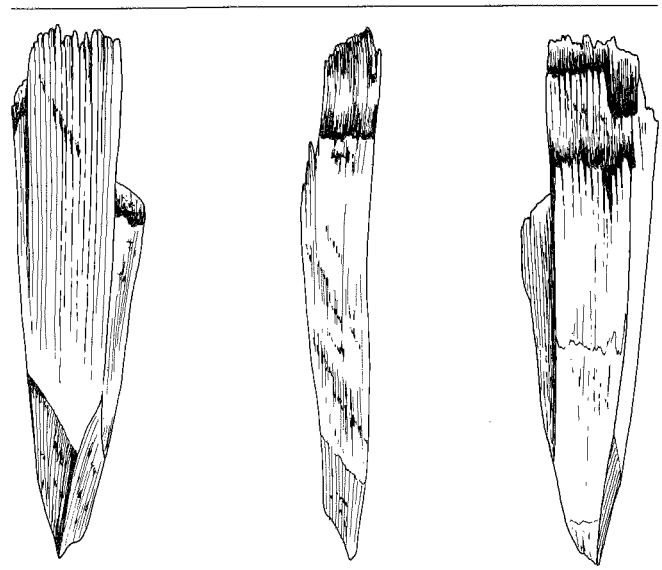
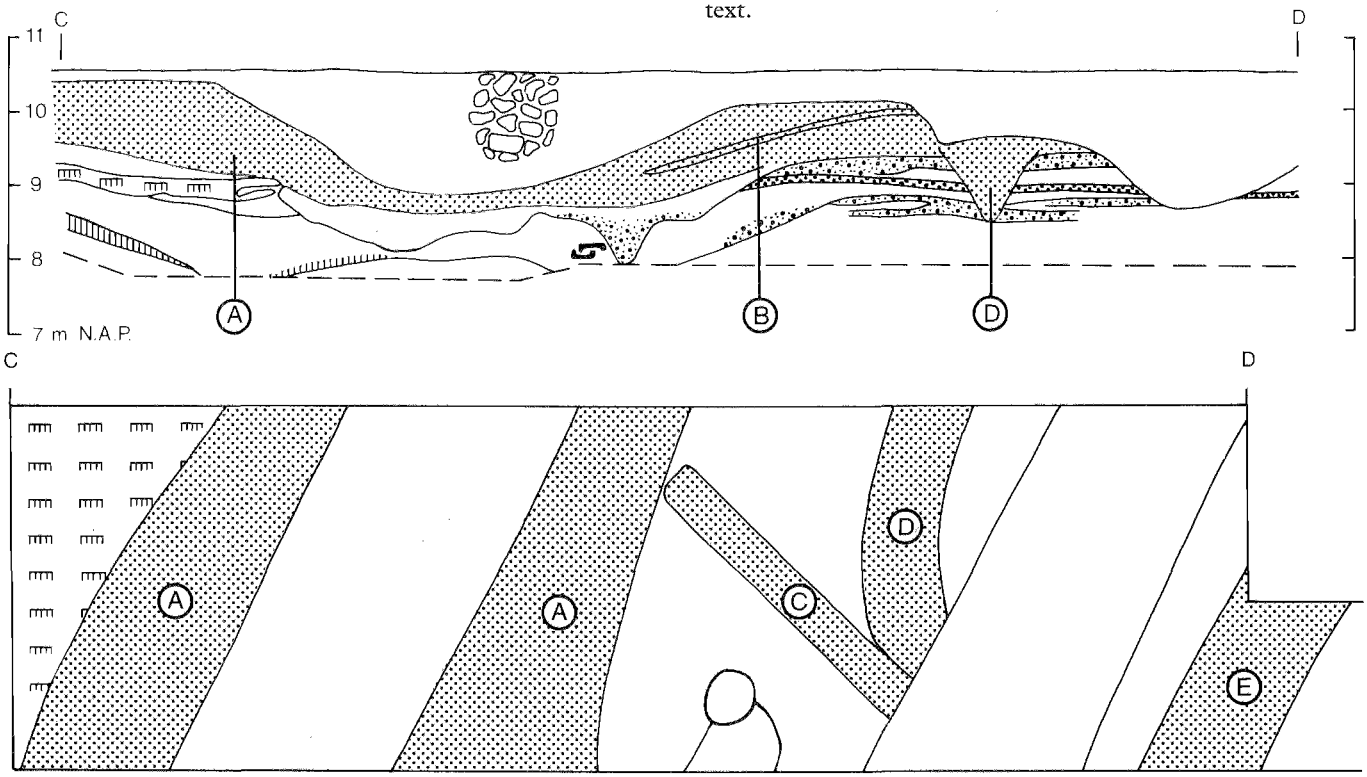


Fig. 105 Meinerswijk, pointed end of oak pole, Period 3. Scale 1:2.

Fig. 106 Meinerswijk, profile CD and plan at c. 9 m NAP, Period 4. Relevant deposits and features are shaded. A-E, see the text.



layer in the profile GC, as well as two or three different layers in the southern part of the trench (profile EF) are indications that this raising may not have been a single enterprise, and certainly not in the area beyond the southernmost ditch.
A raising in phases is all the more probable because the profiles show two V-shaped ditches which must belong to Period 4: the southernmost V-shaped ditch (figs. 106, E and 94, profile EF) and a V-shaped ditch with flat bottom dug through the gravel of the former roads (fig. 106, D). Both ditches have definitely been dug from a level above those of Period 3 and definitely not from the dark upper fill but neither can be related to any specific level, despite intensive efforts during the excavation. It is quite well possible that this lack of a clear stratigraphy can be attributed to yet another flooding of the ditches, with erosion and redeposition of material. Some proof for this assumption is provided by the northern slope of the southernmost ditch (fig. 94, profile EF), which has partly disappeared. Fortunately, the southern slope is still preserved to a level high enough to conclude that it must have been dug after Period 3. This circumstance plus the fact that the fill contains no gravel to speak of indicate that the ditch cannot have been the southern road-ditch from Period 3, although it has approximately the same depth as the gravel-filled ditch north of the road.
It is not clear which of the two ditches from Period 4 is the older, but the southern one reaches c. 50 cm deeper than the ditch cut through the road and is thus likely to be somewhat earlier. Both ditches have been preserved to approximately the same level at c. 9.50 m NAP. The ditch through the road is the only one which does not run straight but curves southward, where it is cut by a small north-south trench (fig. 106, C) at the same spot where both features are cut by one of the large ditches from period 5. This indicates at least that the two ditches can hardly have been contemporaneous.
The precise nature of these features and, in part, also the sequence of events remain highly elusive. The finds from the various features can contribute little to solving this problem. On the whole, the stratigraphic units attributed to Period 4 differ from previous ones because they contain much more building material, such as fragments of tiles, tuff, limestone, and cement. They do not, however, provide any clear arguments for a chronological differentiation of the various features.

The finds
Two groups of finds can be differentiated:

1 Finds from the (upper) fill of the gully and from the northern part of the excavation trench (the top of the layer into which the foundations of the stone walls and some refuse pits have been dug; fig. 106, A/B).
2 Finds from the southernmost ditch (fig. 106, E). The other two trenches or ditches (fig. 106, C, D) are without significant finds.

Terra sigillata
1.1 Rim-fragment of a bowl Drag 37 from La Madeleine (see chapter 6.2.1, p. 145 and fig. 27, no. 45).
1.2 Wall-fragment of a bowl Drag 37 from the Argonne (see chapter 6.2.1, p. 146 and fig. 28, no. 59).
1.3 Wall-fragment of a bowl Drag 37 from Trier (see chapter 6.2.1, p. 148 and fig. 30, no. 96).
1.4 Base-fragment of a bowl Drag 29 with a stamp [OF I]VCVNDI, from La Graufesenque (see chapter 6.2.1, p. 155 and fig. 33, no. 21).
1.5 Rim-fragments of a dish Drag 18, a dish Drag 18/31, 3 plates Drag 31, and one plate Drag 32 (Lud Tp).
1.6 Base-fragment of a dish Drag 18/31 with a stamp SECVN[DINI] from Lezoux (see chapter 6.2.1, p. 156–7 and fig. 33, no. 42).
1.7 Rim-fragments of a cup Drag 27 and 3 cups Drag 33.
1.8 Base-fragment of a cup Hofheim 9 with a stamp [OF] MASO from La Graufesenque (see chapter 6.2.1, p. 156 and fig. 33, no. 28).
1.9 Rim-fragments of 3 bowls Drag 38, a bowl Drag 44 (form Lud Sn) and one mortarium Drag 45.
2.1 Wall-fragment of a bowl Drag 37 from La Madeleine (see chapter 6.2.1, p. 145 and fig. 28, no. 48).
2.2 Large part of a bowl Drag 37 from the Argonne (see chapter 6.2.1, p. 146 and fig. 28, no. 60) with a graffito X on the base.
2.3 Rim-fragment of a cylindrical bowl Drag 30.
2.4 Fragment of a cup Drag 33.

Fine Wares
1.1 Rim-fragment of a cup Stuart 1.
1.2 Rim-fragment of a cup Stuart 2.
1.3 Rim-fragments of plates Stuart 10.
1.4 Wall-fragment of fine Nijmegen ware.

Smooth ware
1.1 Neck of a flagon Stuart 110B (fig. 107, 1).
1.2 Rims of two-handled flagons Stuart 129B and 132B.
1.3 Rim of mortarium with level bead-rim Stuart 149.
1.4 Rims of 3 mortaria with vertical flange cf. Brunst-

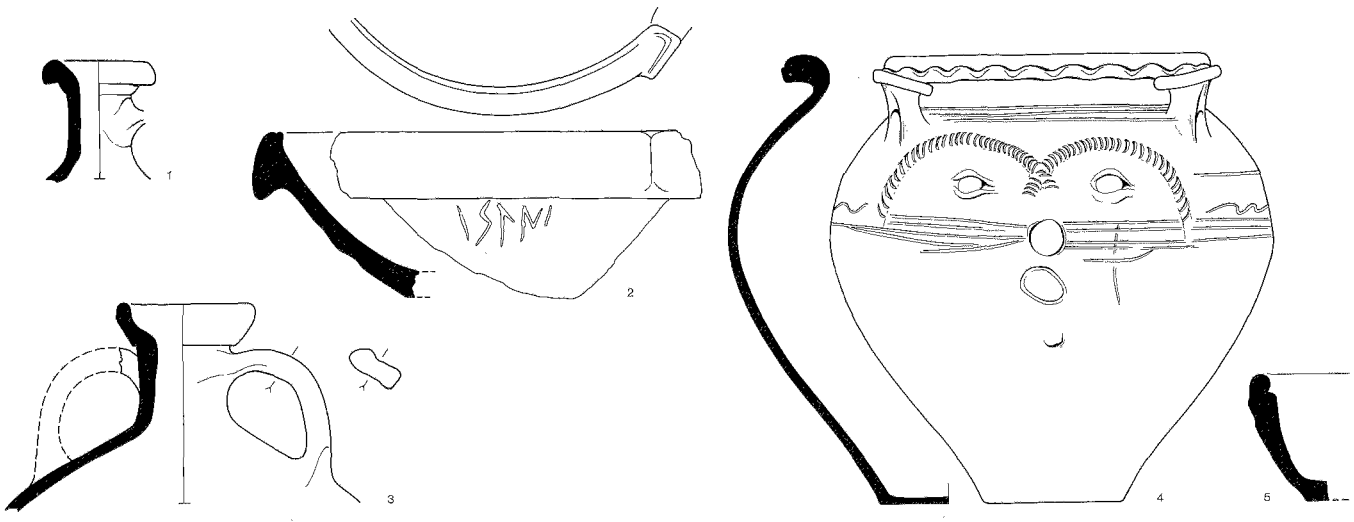


Fig. 107 Meinerswijk, pottery from Period 4 deposits.

ing 1937, type 37. One of these (fig. 107, 2) with a graffito ISANI on the wall. Probably an unknown cognomen. See Holder 1962, 72: *Isannus*.
1.5 Wall-fragments of oil-amphora Dressel 20.
2.1 Neck of a two-handled flagon Stuart 129A (fig. 107, 3).
2.2 Wall-fragment of a wine-amphora Pélichet 47.

Coarse ware
1.1 Complete face-urn with frilled rim and three ornamental (unpierced) applied spouts (fig. 107, 4), cf. Niederbieber 80.

1.2 Rims of 3 plates Niederbieber 112B, one fragment (fig. 107, 5) with traces of cement attached.
1.3 Rims of one vessel each of the following forms: Stuart 201, 215, and 217; Niederbieber 89, 104, and 111; and the neck of a jug Niederbieber 100.
2.1 Rims of two cooking pots, Stuart 201 and Niederbieber 89.

Building material
1.1 Numerous fragments of tegulae, imbrices, and lateres; one fragment with the circular stamp with two 'tails' (fig. 108) EX GER INF: *CIL* XIII/6, p. 124–5, type $19+1^4$ = Holwerda/Braat 1946, Pl. 29, 30.
1.2 Fragments of wall-painting (white and pink with red dots) and cement.
1.3 Fragment of opus signinum.
2.1 Fragments of slate, *grauwacke*, and quartz.
2.2 Fragments of cement.

Glass
1.1 Fragment of window-glass
1.2 Base-fragment of a bottle with a pontil-mark.

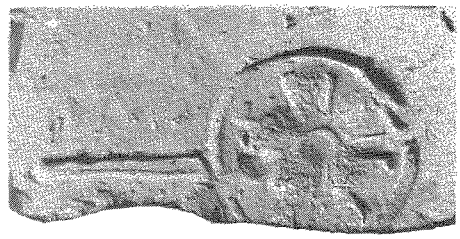
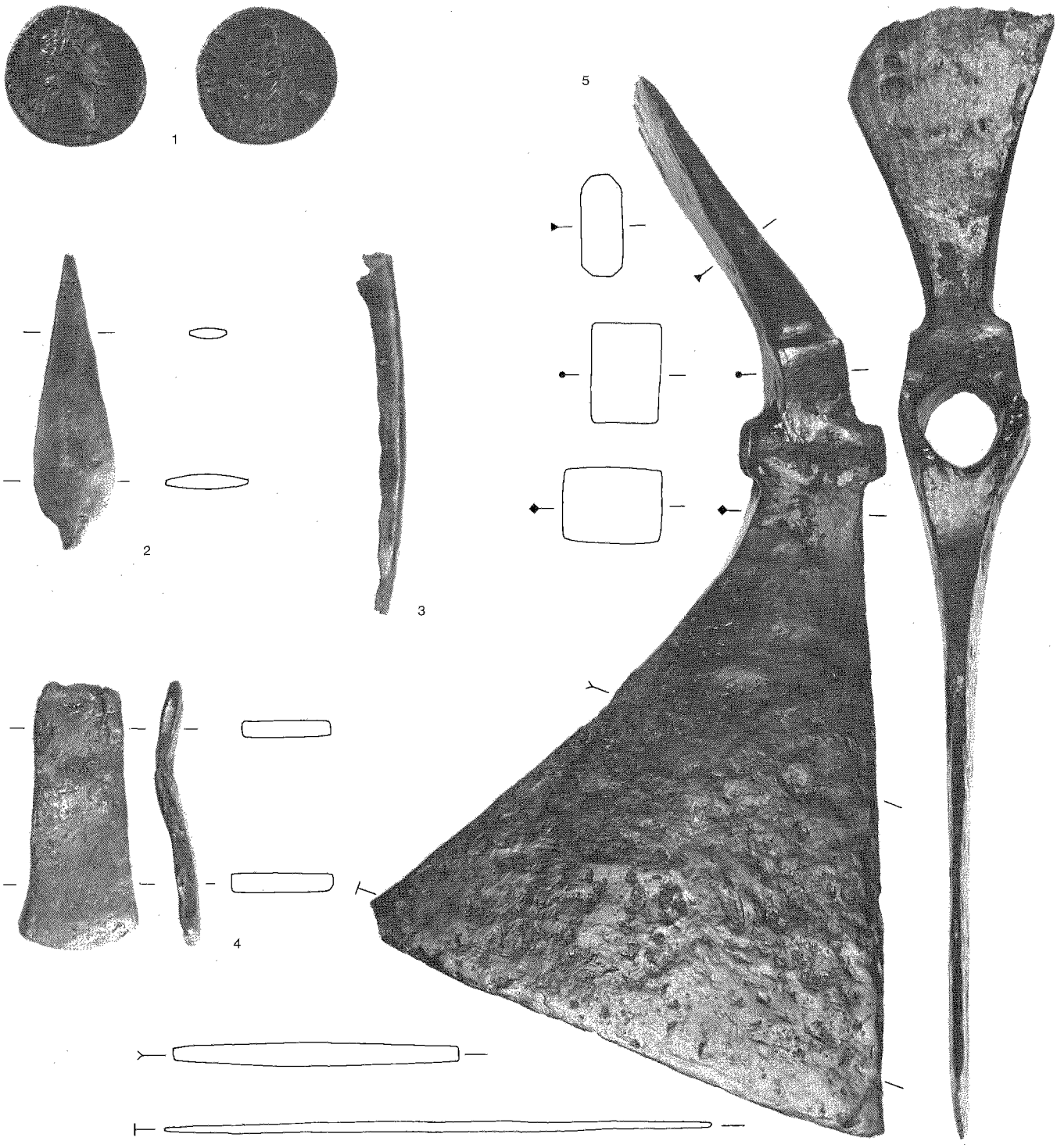


Fig. 108 Meinerswijk, tile-stamp EX GER INF from Period 4 deposit. Scale 1:2.

Metal
1.1 Bronze coin, As of Domitian, AD 86, cf. *RIC* 335 (fig. 109, 1).
1.2 Leaf-shaped iron spearhead. Most of the socket is missing, remaining length 9.4 cm (fig. 109, 2).
1.3 Curved iron pick (fig. 109, 4) rear end of a pioneer axe as no. 1.4.
1.4 Large and well-preserved iron pioneer axe (*dolabra*) with broad blade and relatively small curved pick, length 36,3 cm, weight 1.228 kg (fig. 109, 5). Compare e.g. Ulbert 1970, no. 287.
1.5 Fragment of bronze binding from an oval wooden shield (fig. 109, 3). The binding has a U-shaped section and on one end still has part of the roughly semicircular lugs with a hole for a rivet surviving.

Organic material
1.1 Fragment of corroded hobnails with some leather; part of a sole.

Dating
From the lower (natural) fill of the gully, ascribed to Period 3, it is clear that the major changes which led to a


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2
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◁ Fig. 109 Meinerswijk, metal objects from Period 4 deposits. Scale 1:2 (coin 1:1).

raising of the site and the abandonment or at least relocation of the road must have started some time before AD 150. Apart from some clearly residual finds from earlier layers, the material covers the entire 2nd and the early-3rd centuries. The military tile-stamp of the *excercitus Germanicus inferior*, produced at site 433 (De Holdeurn), indicates the Period 4 lasts at least till after AD 175. Late terra sigillata forms such as Drag 32 and especially Drag 45 point in the same direction, as does a fragment of the coarse-ware dish Niederbieber 112B which must have landed in the opus caementicium of some wall which was demolished again in time for the fragment to end up in a Period 4 layer.

On the other hand, for Period 4 to have lasted far into the 3rd century the number of late coarse-ware pots Niederbieber 89 is rather low and the absence of sigillata from Rheinzabern may also be significant. The total quantities of datable material are, however, too small to draw any firm conclusions. The end of Period 4 should probably be dated to around AD 200.

9.2.5 Period 5

The main features of Period 5 are two or perhaps three large parallel ditches with a very dark and polluted fill and merging without noticeable transitions into the dark upper layer covering the entire southern part of the excavation trench (fig. 94). In its turn, this layer is indistinguishable from the upper layer of dark soil which covers the entire site (fig. 110, A). It is only 10 cm thick, occasionally at the most c. 20 cm, and cannot be identified as a ploughzone. This could be verified in the northern part of the excavated area where the foundation trenches (robber trenches) of the stone walls became clearly visible immediately after the sods were removed! It is not impossible, and for reasons to be discussed below and in the next paragraph even likely, that the surface at the highest point of the site, where the excavation trench is situated, has to some degree been levelled in the past. Definite proof is, however, lacking.

This state of affairs implies that there is no definite relation between the stone walls and any of the features in the southern part of the trench. In theory, the walls could belong to the latest phase of Period 4 but, as suggested by profile AD (fig. 94), a relation to the features of Period 5 (or 6?) is more likely: in any case the foundation trenches were dug into the layer that was raised during Period 4.

It is possible that there are actually two phases in the building activities. As mentioned at the beginning of this chapter, the foundation trench of the southernmost wall covers only the western part of the trench but the robber trench of the same feature at the first plane (immediately under the surface) runs straight through. In addition, the foundation trench shows a slight but nevertheless clear difference in orientation from the other foundations (and, for that matter, from virtually all other features from all periods) while the robber trench has the same direction. It is possible, as mentioned before, that during the removal of stone from the foundations a robber trench continued where no foundation existed. But it is more likely that there was indeed a foundation. This implies two things. First, the original surface of the site was somewhat higher than it is today. Second, that an opening was closed perhaps in a more or less provisional way.

These findings may correspond with the results of the excavation in the southern part of the trench. Apart from the two large ditches (fig. 110, B and E) there is a depression in profile EF (fig. 94) with a lower fill that has a somewhat lighter colour than the normal 'Period 5' fill. It has a flat bottom, c. 50 cm wide. This feature extends only c. 1 m into the excavation trench (fig. 110, C) where it is cut by a pit dug from the Period 5 level (fig. 110, D). Although the black layers in the profile did not permit a stratigraphical differentiation between this and other features, the less-polluted lower fill and the stratigraphically later pit show that it must be a relatively early phenomenon in Period 5. It is not impossible that this feature is some elongated refuse pit, but it may in fact be the end of a flat-bottomed ditch. In that case, the opening corresponds well with the opening in the southernmost wall.[25]

The two large and probably contemporaneous ditches may thus have been dug after the interrupted ditch was filled in again, and perhaps at the same time as the wall was closed. If this southern wall is the wall around the fort, the distance between it and the northern side of the inner ditch is no more than 2.5 m, which would seem to be a very narrow space and, moreover, the wall and the ditch do not appear to run parallel. It is, therefore, also

25 Note that the small trench which seems to run through this opening (fig. 106, C) definitely does not belong to the same phase!

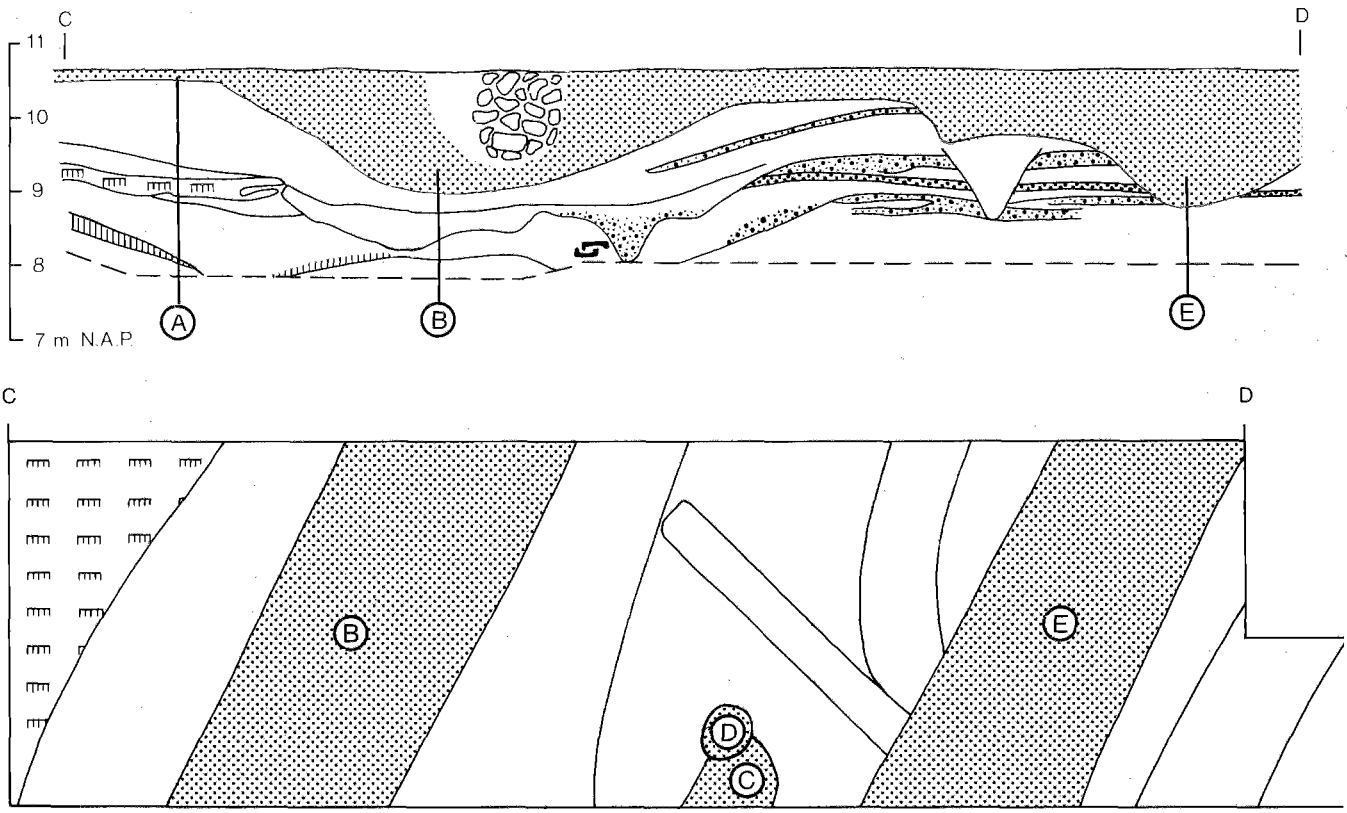


Fig. 110 Meinerswijk, profile CD and plan at c. 9 m NAP, Period 5. Relevant deposits and features are shaded. A-E, see the text.

conceivable that the wall was demolished and replaced by a more northern wall, in which case the building in the northern part of the trench could be a gate or, less probably, a tower.

Whatever the true interpretation of the features and the real sequence of events may prove to be, it is obvious that the available data are too limited to allow a completely satisfactory interpretation. It is clear, however, that the two large ditches belong to a fort which was probably built entirely in stone. The entire dark upper fill of the excavation trench, including the ditches, contains large amounts of brick and various kinds of stones. The origin of this dark layer can probably again be attributed to natural causes. In section, the ditches are more or less U- or bowl-shaped. Only the section through the southern ditch (and, perhaps, the interrupted ditch) in profile EF (fig. 94) still show a more or less V-shaped profile with a flat bottom. For the southern ditch it could be observed that the 50-cm-wide base of the ditch was in fact a sort of cleaning channel, lined with sods. Such a construction has also been observed in the ditche around the fort in Zwammerdam and elsewhere.[26] Th origin of the dark layer as well as the final shape of th ditches were probably caused by (repeated?) flooding and the width of especially the northern (inner) ditch some 6 m, may well far exceed its original width. If it i assumed that the surface during Period 5 is more or les the same as the present-day surface of the site, the north ern ditch (fig. 110, B) was 1.70 m deep, and the southern (E) and the interrupted (C) ditch 2 m deep.

26 Haalebos 1977, 34 and note 51.

The Finds

In the discussion of the finds, five groups are differen tiated. The foundation trenches are without significan finds, but there is some material from the various pits These are included here, although there is, of course, n guarantee that they are indeed all features from Period 5 The following groups are concerned:

1 Finds from pits (fig. 110, D and on the first excava tion plane, fig. 94).
2 Finds from the dark layer in the southern part of th excavation trench (fig. 94, profile EF).
3 Finds from the northern ditch (fig. 110, B).
4 Finds from the southern ditch (fig. 110, E)
5 Finds from the possible, interrupted, ditch (fig. 110 C).

Terra sigillata
2.1 Rim-fragment of a bowl Drag 37 from Blickweiler/Eschweilerhof (see chapter 6.2.1, p. 145 and fig. 27, no. 38).
2.2 Fragments of three dishes Drag 18/31.
3.1 Fragments of three bowls Drag 37 from Trier (chapter 6.2.1, p. 148 and fig. 30, nos. 95, 97, and 98).
3.2 Fragments of three plates Drag 31, a cup Drag 33 and five fragments of mortaria Drag 45.
4.1 Fragments of bowls Drag 37 from Trier (chapter 6.2.1, p. 148 and fig. 30, nos. 92 and 93) and Rheinzabern (see chapter 6.2.1, p. 151 and figs. 31, no. 125 and 32, nos. 126–129).
4.2 Fragments of two South-Gaulish cups: Drag 24/25 and Drag 27 with a graffito XX[.
4.3 Fragments of one cup Drag 27, one Drag 40, one Drag 46 (Lud Bb), and seven specimens of Drag 33; five plates Drag 31, one bowl Drag 36, one Drag 38, and one Niederbieber 11B; three mortaria Drag 45.
5.1 Fragment of a bowl Drag 37 from Trier (see chapter 6.2.1, p. 148 and fig. 30, no. 94).
5.2 Fragments of a plate Drag 31 and a cup Drag 33.

Fine wares
1.1 Rim of a colour-coated beaker Niederbieber 32c.
3.1 Fragments of three beakers Niederbieber 32c and one beaker Stuart 2.
4.1 Rim of a colour-coated plate Stuart 10.
4.2 Fragments of two beakers Niederbieber 32c and one Niederbieber 32d.

Gallo-Belgic ware
3.1 Rim of a terra nigra plate Hofheim 99.

Smooth ware
2.1 Part of a lid.
2.2 Rim of a mortarium with horizontal flange Stuart 149.
2.3 Fragments of oil-amphora(e) Dressel 20.
4.1 Fragments of a flagon with brown 'marbled' colour-coating.
4.2 Fragments of two mortaria, one with horizontal and one with vertical flange.
4.3 Fragments of oil-amphora(e) Dressel 20 and the handle of a wine-amphora Pélichet 47.
5.1 Fragments of a mortarium with horizontal flange.

Coarse ware
1.1 Rim of a cooking-pot Niederbieber 89.
2.1 Fragments of two bowls Stuart 210 and one cooking-pot Niederbieber 89.
3.1 Fragments of one bowl with inturned thickened rim Niederbieber 104, two plates Niederbieber 111, and nine cooking-pots Niederbieber 89.
4.1 Fragments of two bowls Stuart 210, one c. Gose 1950, no. 505, and one Niederbieber 104; one cooking-pot Stuart 201 and seven Niederbieber 89; three plates Niederbieber 112b and one Niederbieber 111.
4.2 Neck of a flagon Niederbieber 98.
4.3 Wall-fragment of a face-urn.
5.1 Rim of a cooking-pot Stuart 201.

Building material
Brick, cement, and stone fragments occur in all features from Period 5. The following items have special relevance:
A Brick[27]
1.1 From the refuse pit cutting the possible (interrupted) ditch: fragment of a tegula with one half of a round stamp (fig. 111, 1), diam. 5.1 cm, with traces of positive letter imprints: v[ ] f, possibly VEX EX GER F.
2.1 Fragment of a tegula with a round stamp (fig. 111, 2), diam. 5.3 cm, with traces of positive letter imprints, presumably VEX ER GER (INF).
2.2 Stray find, probably from the topsoil: fragment of a tegula with a square stamp (fig. 111, 3) EXGERINF: *CIL* XIII/6, p. 125, type $32+\zeta$ = Holwerda/Braat 1946, Pl. 28, 28. The stamp is known from many frontier fronts.[28]
2.3 Stray find, probably from the top soil: fragment of a tegula with an illegible round stamp with traces of positive letter imprints (fig. 111, 4), probably VEX ER GER (INF).
4.1 Fragment of a tegula with a rectangular stamp (fig. 111, 5) EXG[ERINF]. Compare Holwerda/Braat 1946, Pl. 28, 7.
5.1 Fragment of a tegula with a rectangular stamp (fig. 111, 6) EXGE[RINF]. Compare Haalebos 1977, *Taf* 28, 48 and Holwerda/Braat 1946, Pl. 28, 25.
5.2 Fragment of a tegula with a rectangular stamp (fig. 111, 7) [E]XGERINF: *CIL* XIII/6, p. 124–5, type $19+\gamma^1$. Compare Haalebos 1977, *Taf.* 27, 41.
5.3 Fragment of a tegula with part of an illegible, rectangular stamp (fig. 111, 8) with traces of negative letter imprints, presumably EXGERINF.

27 I thank J.E. Bogaers (Nijmegen) for his help in the reading of several tile-stamps.
28 For references, see Haalebos 1977, 184, nos. 60–61.

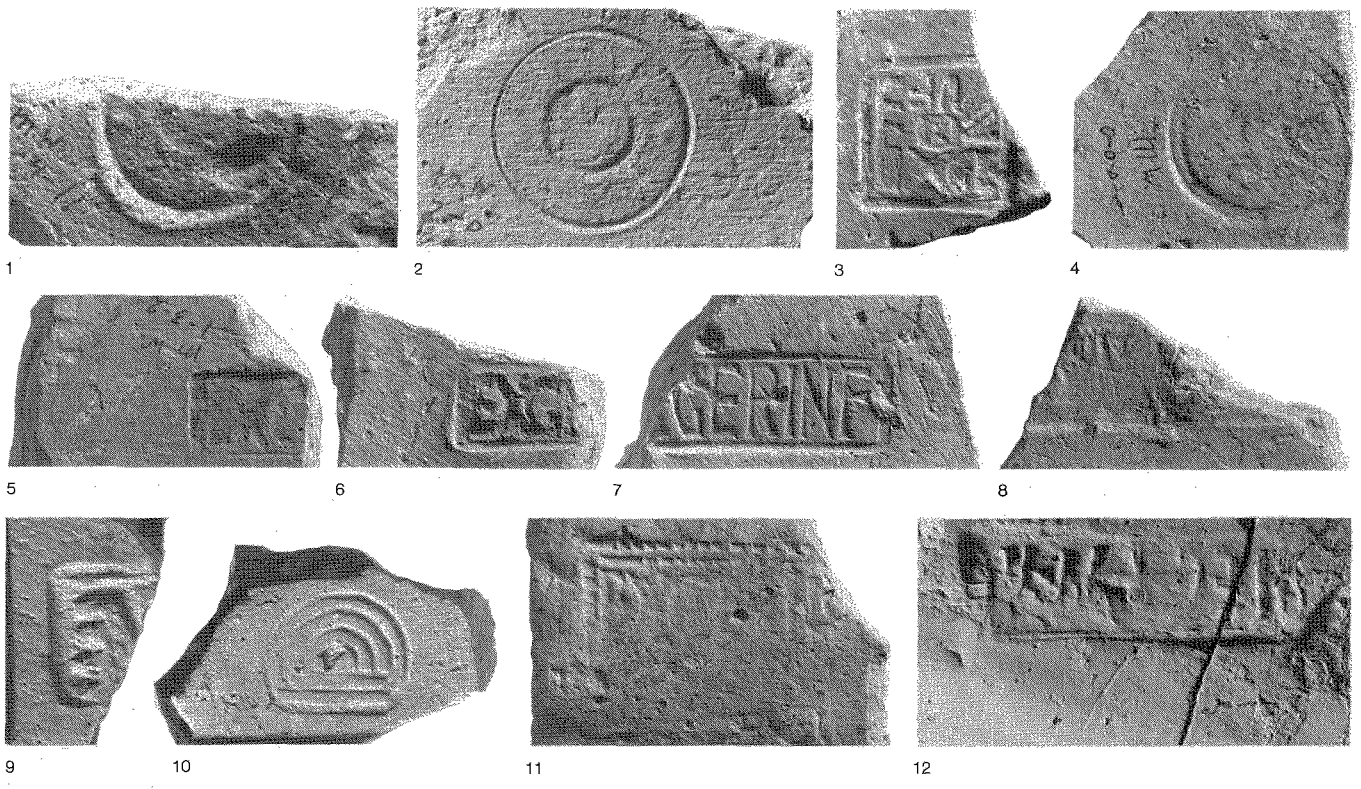


Fig. 111 Meinerswijk, tile-stamps from Period 5 deposits. Scale 1:2.

5.4 Fragment of a tegula with part of a curious rectangular stamp with partially negative, partially positive letter imprints (fig. 111,9) EX[GER. INF]: *CIL*XIII/6, p. 125, type 25 + γ = Holwerda/Braat 1946, Pl. 30, 62.

5.5 Fragment of a tegula with a half-round satellite-stamp (fig. 111, 10) belonging to a broken-off round EX GERINF stamp: *CIL* XIII/6, p. 125–5, type 19 + κ^2 = Holwerda/Braat 1946, Pl. 29, 33. Also known from Zwammerdam, Maurik, Neuss (Haalebos 1977, 184 and *Taf.* 29, 64), and Utrecht (Van Giffen a.o. 1934–38, 14, fig. 3, 83f).

5.6 Fragment of an imbrex with a rectangular stamp with negative letter imprints (fig. 111, 11) EXG[ER]INF in unknown frame. Compare Holwerda/Braat 1946, Pl. 28, 4).

5.7 Fragment of an imbrex with a rectangular retrograde stamp (fig. 111, 12) LEG I M ANT: Holwerda/Braat 1946, Pl. 33, 2. The name Antoniniana is characteristic for the reigns of Caracalla and Elagabalus.[29]

B Stone

4.1 Tuff block with an inscription LEGIMPF: LEG(io) I M(inervia) P(ia) F(idelis). The front measures 57 × 14 cm and the block is 26 cm wide (fig. 111a). A large part of the rear side and a smaller part of the front are broken off, thus damaging the L of the inscription. The block is probably a building inscription from the stone fort of Period 5, although such inscriptions do not normally seem to have been cut in tuff. It is most likely that the inscription indicates building activities of a *vexillatio* of the *legio I* in Meinerswijk. In view of the dating of Period 5 and the absence of an A at the end of the inscription, these activities – and thus the inscription – are likely to predate AD 211.

Somewhat comparable to the inscription from Meinerswijk is another building inscription from the Netherlands (but from an unknown findspot), LEG·I·M·P·F·ET / LEG·XXX·V·V·, read as: LEG(io) I M(inervia) P(ia) F(idelis) ET LEG(io) XXX V(lpia) V(ictrix).[30] The inscription is, however, put in a *tabula ansata* frame and cut in a block of limestone instead of tuff.

Glass

2.1 Three fragments of green glass from a square bottle Isings 50.

29 Stein 1932, 181.

30 Cf. *CIL* XIII, 8832 = *ER* II, 409.



Fig. 111a Meinerswijk, tuff block with inscription LEG(io) I M(inervia) P(ia) F(idelis). The front measures 57 × 14 cm. Originally the letters were painted red.

2.2 Fragment of bluish-green glass from a bottle Isings 50 or 51.
4.1 Wall-fragment of blue glass from a bottle Isings 50.
4.2 Shoulder-fragment of bluish-green glass from a bottle Isings 51.

Metal

1.1 Fragment of a leaf-shaped iron spearhead. The point and most of the socket are missing (fig. 112, 1).
1.2 Three fragments of what appears to have been an approximately life-sized bronze statue, deliberately chopped into very small pieces, presumably for recycling the metal. The fragments were found in the large refuse pit in the stone building (fig. 94). At the first excavation level this pit showed clear traces of fire, as did the surface in the northwestern corner of the excavation trench (west of the building on the level drawn in fig. 94). It is possible, therefore, that the smelting-activities post-date or predate Period 5 and do not belong to this phase. If the traces of burning inside the building and outside are indeed contemporaneous, the bronze fragments should actually belong to the last phase of Period 4. Of the fragments themselves, one fragment is identifiable as a toe, probably the small toe of the right foot (fig. 112, 2). It is hollow cast *à cire perdue*. Compare, e.g., Boube-Piccot 1969, Pl. 89.
3.1 Fragment of an iron dagger with a straight-sided blade and without a prominent midrib (fig. 112, 3).

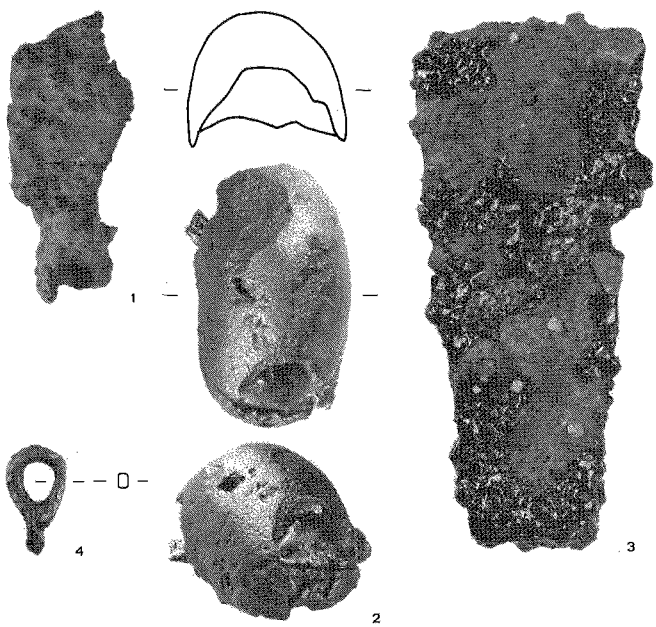


Fig. 112 Meinerswijk, metal objects from Period 5 deposits. Scale 1:2 (bronze toe 1:1).

4.1 Part of an iron double-spiked loop, most of the shaft is missing (fig. 112, 4).

Organic material

3.1 Two fragments of leather soles studded with badly corroded hobnails.

Dating

Most of the pottery from Period 5 belongs typochrono-

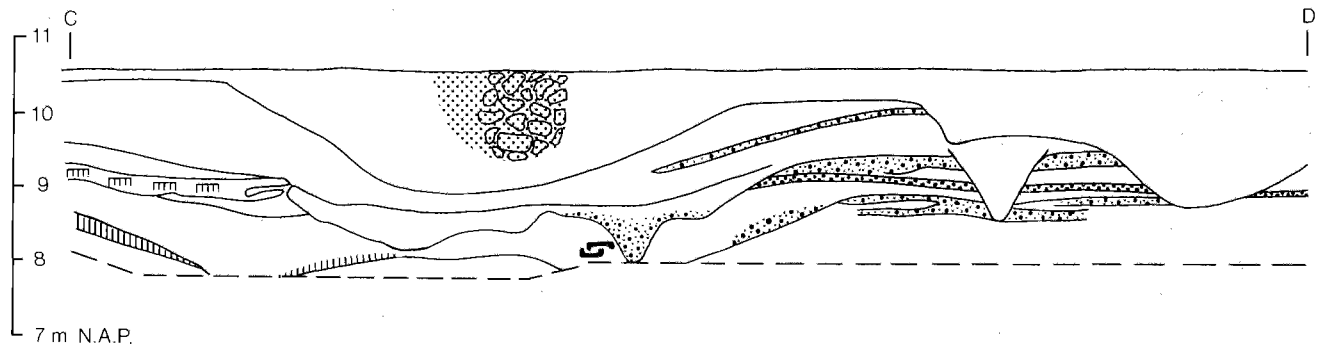
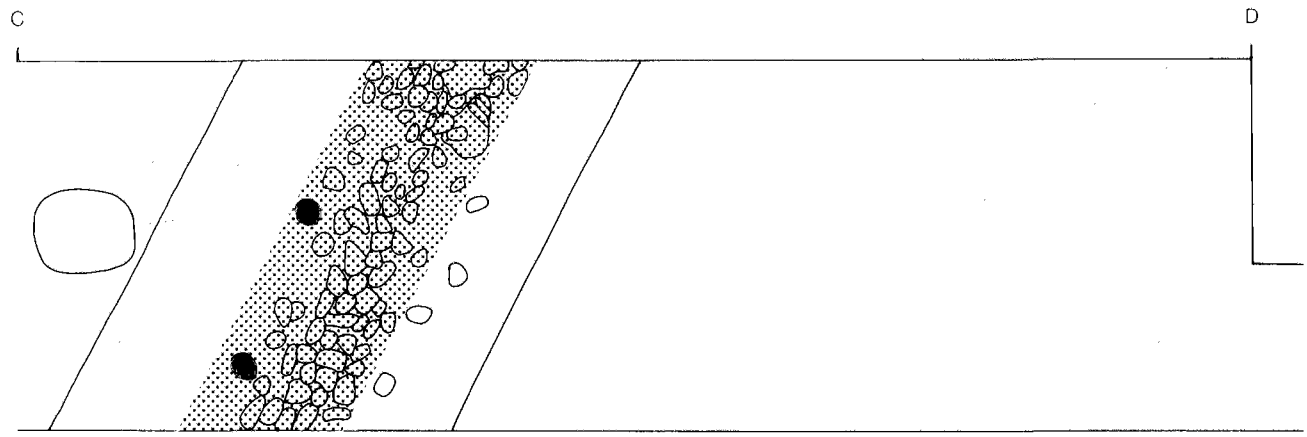


Fig. 113 Meinerswijk, profile CD and plan at c. 10.2 m NAP Period 6. Relevant features are shaded.



logically to the Niederbieber horizon and dates either from the late-2nd century or approximately the first half of the 3rd. The stamps of the *exercitus Germanicus inferior* indicate considerable building activities after AD 175, although at least one specimen (fig. 108) arrived in Meinerswijk early enough to land in a Period 4 deposit. It is possible that the stamp from the *legio I Minervia Antoniniana* dates these activities more precisely to between AD 211 and 222. However, the possibility of renewed construction work during Period 5 as discussed above and the absence of the title *Antoniniana* on the building inscription from the southern ditch both suggest that the LEGIMANT stamp may also date this rebuilding or repair work.

In any case it is clear that Period 5 can be dated approximately to the first half of the 3rd century. Arguments for a more exact dating are lacking. Precisely datable late 3rd-century material, such as the latest terra sigillata products from Trier or Rheinzabern, is not available but its absence among the finds from the small excavation trench is hardly significant.

9.2.6 Period 6

As indicated on fig. 113, there is one feature which definitely post-dates Period 5. Directly above the southern slope of the inner (northern) ditch of that phase is a foundation trench filled with secondarily used blocks of stone – mostly tuff – only occasionally bonded by cement that may have been poured over the stones after their deposition in the trench. Most of the blocks cemented together in an orderly fashion appear to have been used while already in that state. They undoubtedly originally belonged to the fort of Period 5. The trench itself was more or less clearly visible only at the first excavation plane, directly under the grass cover: it could not be observed clearly in the profiles.

Although it appears to be a somewhat ramshackle foundation, the construction was not built without care. The lowest part consists of large and heavy blocks, such as a cornice (fig. 116), the smaller blocks piled on top of them. Immediately south of the foundation two sizeable posts – presumably part of a row – have been observed, with a diameter of 30 cm and reaching to 75 cm below the surface of the site (indicated in black).

There are no definite clues as to the interpretation of this foundation. It evidently indicates some sort of wall, perhaps supported by sturdy posts, wich a maximum thickness of 1.40 m and built with the remains of previous stone buildings. It may have been built to protect the highest part of the site against the water but it could just

as well have been a defensive wall or have had some altogether different purpose. At the site of the excavation there are no levels preserved which correspond stratigraphically to this construction. As mentioned in the previous paragraph, it is likely that some soil was removed in the past, leaving only a thin upper layer of some 10 cm with material that could belong to Period 6. Confirmation for this assumption is also provided by some finds from the cores of borings by the RGD (Netherlands Geological Survey) at the eastern border of the site (a.o. the C14-boring Meinerswijk II: see chapter 3, fig. 18). The deepest part of the core contains Roman finds, but above those are some late-Roman or Merovingian sherds and in the highest part even later medieval 12th-century finds.[31] It thus seems very likely that post-Period 5 layers are still preserved around the highest part of the site, as is also indicated by the geological profile AB on fig. 18, which indicates that from the 7th century onwards these even partly covered the fossil channel delimiting the eastern and southern perimeter of the site.

The Finds

The finds from Period 6 can be divided into three groups of which only the first can provide direct dating evidence:

1 Finds from the foundation trench.
2 Finds from the topsoil north of the foundation.
3 Finds from the topsoil south of the foundation.

Because a sizeable portion of the material is evidently residual from earlier layers, these have been marked with an *.

Terra sigillata

1.1* Wall-fragment of a bowl Drag 37 from La Madeleine (see chapter 6.2.1, p. 145 and fig. 27, no. 43).
2.1* Wall-fragment of a bowl Drag 37 from La Madeleine (see chapter 6.2.1, p. 145 and fig. 27, no. 46).
2.2 Wall-fragment of a bowl Chenet 320 from the Argonne (see chapter 6.2.1, p. 153 and fig. 32, no. 139) and a base-fragment, probably from a late-Roman bowl or plate.
2.3* Fragment of a plate Ludowici Sb with a stamp Q[V]IИTVƧЯ (see chapter 6.2.1, p. 156 and fig. 33, no. 40).
2.4* Fragments of three cups Drag 27, two Drag 33, and two dishes Drag 18.
3.1 Base of a mortarium Drag 45 (possibly Alzei 3).

Colour-coated wares

2.1* Rim-fragment of a beaker Niederbieber 30.
2.2 Three-ribbed handle of a flagon or jug in an almost red-brown slip ware or conceivably with a brown 'marbled' colour-coating.

Gallo-Belgic ware

2.1* Rim-fragments of a type 1 jar, a type 2 cooking-pot (see chapter 6.2.3) in a terra nigra-like ware (fig. 114, 1–2).
3.1* Rim-fragments of a cooking-pot with inturned rim ('cork-urn' form) in a terra nigra-like ware (fig. 114, 3) and fragments of one or two ovoid beakers decorated in barbotine (*Perlurnen*), Hofheim 118.

Smooth ware

2.1* Necks of flagons Stuart 109B/110A, 110B, and 131.
2.2* Rims of a mortarium with vertical flange and of a large dolium.
2.3* Fragment of an oil-amphora Dressel 20.
3.1* Neck of a two-handled flagon Stuart 129B.
3.2* Three rims of mortaria with horizontal flange.

Coarse ware

1.1 Large fragment of a cooking-pot with crescent-shaped rim in very coarse brown-violet Mayen ware, Alzei 27 (fig. 114, 4). This find is of special importance because it was found inside the foundation and dates its construction.
2.1 Fragments (a.o. three rims: fig. 114, 5–7) of at least three cooking-pots Alzei 27 in Mayen ware.
2.2 Rim-fragment of a deep dish with thickened rim Alzei 29 (fig. 114, 8).
2.3* Rim-fragments of two cooking-pots, possibly to be identified as late examples of Niederbieber 89 (fig. 114, 9–10),[32] four specimens Niederbieber 89 and four Stuart 201; two bowls Stuart 210.
2.4 Rim-fragments of three cooking-pots with thickened, cordoned rim (type 3: chapter 6.2.5) *Trier-Umbaukeramik* type 43 (fig. 114, 11–13).[33]

31 The material was provided by J. de Jong, RGD Haarlem. These finds do, incidentally, confirm the C14-datings of 1500 ± 35 and 1350 ± 30 BP.

32 On the dubious chronological value of rims of this type, see chapter 6.2.5 and also Pferdehirt 1976, 118–20.
33 Hussong/Cüppers 1972.

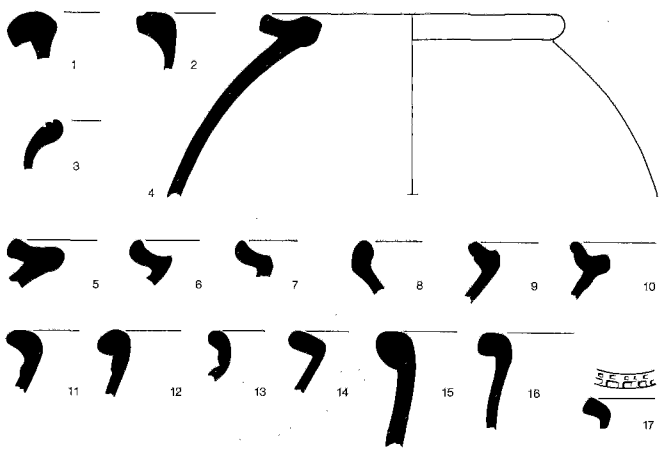


Fig. 114 Meinerswijk, pottery from Period 6 foundation and topsoil. Nos. 1–3 and 9–10 are residual from earlier layers.

2.5 Rim-fragments of three steep-walled Merovingian cooking-pots (fig. 114, 14–16) and a similar fragment decorated by wheel-rouletting (fig. 114, 17) which presumably dates to about the 8th century.[34]
3.1 Fragment of a vessel of very coarse brown-violet Mayen ware.
3.2* Rim-fragment of a bowl Niederbieber 104.
3.3 Handle of an early stoneware jug, late 13th century.
3.4 Wall-fragment of glazed stoneware, 16th century.

Building material
1.1* Fragment of a tegula with part of a round stamp EXG[ERIN]F and a satellite-stamp in the form of a stylized hoof-imprint (fig. 115): *CIL* XIII/6, p. 124–5, type $21+i^3$ = Holwerda/Braat 1946, Pl. 29, 29 and Haalebos 1977, *Taf.* 28, 62 and p. 184, 290. The stamp has been recorded at many sites along the *limes* of Lower Germany: Valkenburg, Roomburg, Zwammerdam, Vleuten–De Meern, Vechten, Maurik, Xanten, Gellep, and Neuss.
1.2 Numerous – and virtually all tuff – blocks of various shapes. A well preserved large block from the base of the foundation is illustrated in fig. 116. It is an elaborately moulded cornice, length 111 cm, maximum width 80 cm, and 24.5 cm thick. On the upper side a 12-



Fig. 115 Meinerswijk, stamp EXG[ERIN]F on secondarily used tile in Period 6 foundation. Scale 1:2.

cm-deep mortise for a dowel (*dookgat*) is preserved. Cornices with similar profiles (*cyma recta* moulding) are known from the temples in Elst (site 105).[35] The cornice may have been used in the Period 5 fort.
Apart from the cornice, and a few fragments of similar stones, some wedge-shaped stones should also be mentioned here. Originally, these have been part of an arch.

Dating
The only find directly suitable for dating purposes is the large fragment of a cooking-pot Alzei 27 from the foundation. Such pottery is characteristic for the Late-Roman Period and especially for the second half of the 4th and the early-5th centuries. It can thus be assumed that the activities of Period 6, the demolition of some stone walls or perhaps more likely the collection of building materials from structures already in ruins and the construction of a new wall from them, took place in that same period.
In view of the small excavated area and the probability that some soil was removed in the past, a considerable number of late-Roman finds especially from the area north of the foundation, testifies to occupation during the Late-Roman Period. It is not inconceivable that parts of some buildings were still standing and could be used after some repair work; this would explain the clear concentration of late-Roman material north of the foundation.
The presence of Merovingian and later finds at the site of the excavation and in borings is evidence for either continued, renewed or intermittant occupation of the

34 Pers. comm. W.J.H. Verwers.

35 Bogaers 1955, Pl. 40. For an identical profile, see the corbel stone from Hesselbach, Baatz 1973, *Taf.* 21, 1b.

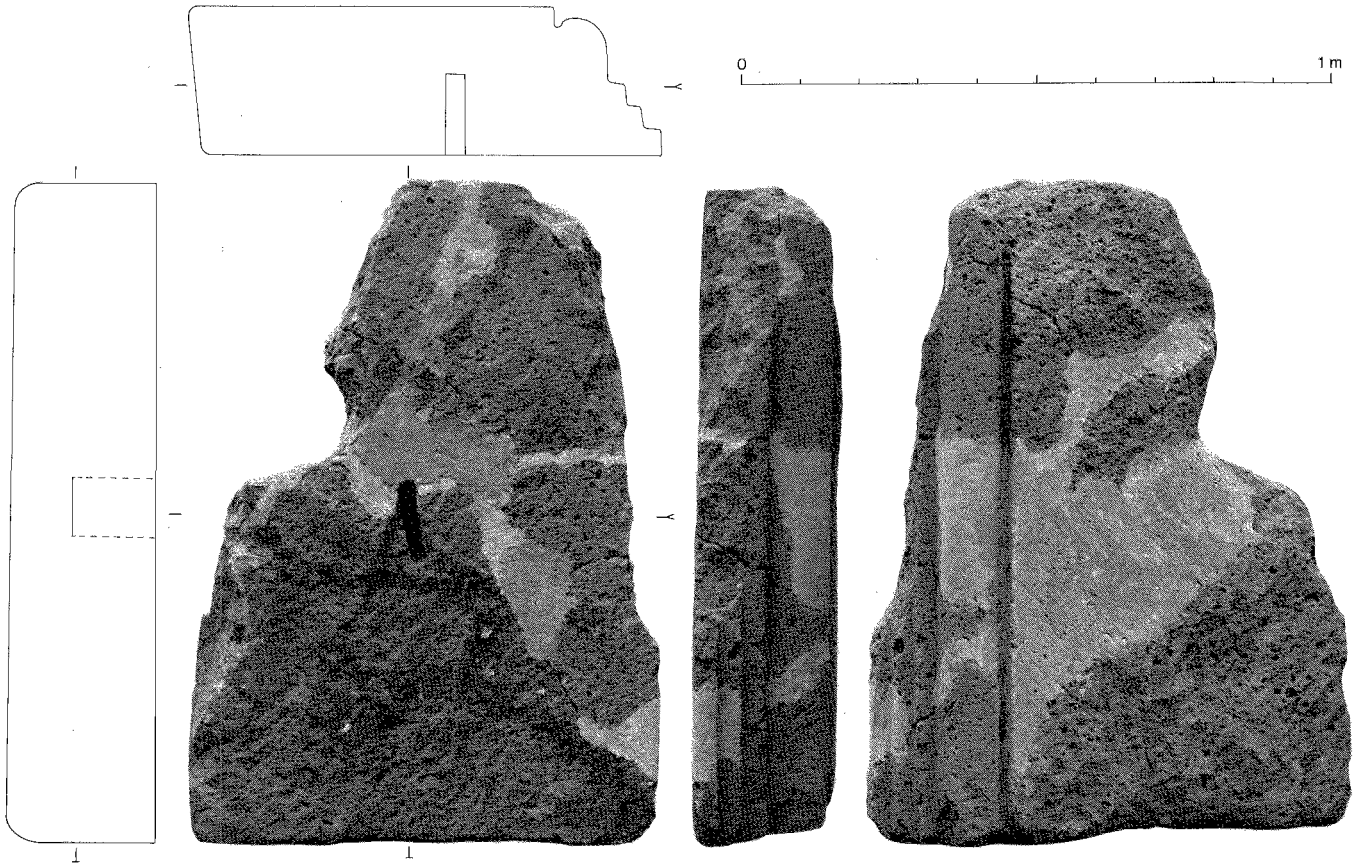


Fig. 116 Meinerswijk, large tuff block, moulded cornice from a construction of the Period 5 fort secondarily used at the base of the Period 6 foundation.

site until at least the 12th-13th centuries and possibly even later. If Period 6 is dated to IVB-Va, as suggested by the available finds, it is clear that future research will undoubtedly produce evidence for several additional early- and late-medieval phases.

Stray Finds

There are a few stray finds (fig. 117) from the excavation as well as the site as a whole which deserve to be mentioned. They are the following items:

1 Rim-fragment of what seems to be a moulded pipe clay bowl with a pierced wall.

2–3 Two rim-fragments of steep-walled Merovingian cooking-pots.

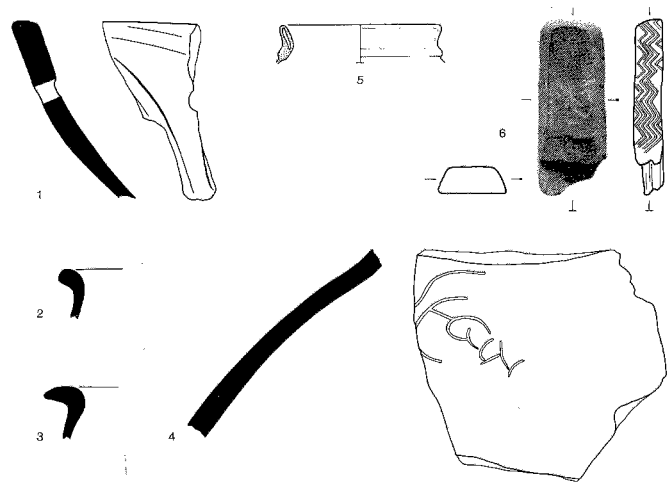


Fig. 117 Meinerswijk, stray finds from site 126. Scale 1:4.

3 Wall-fragment of an oil-amphora Dressel 20 with an *ante cocturam* graffito SECVN. A very similar graffito, read as SECVM, is known from an identical amphora with a stamp SCOROBRES from Split (Yugoslavia).[36]
5 Rim of a glass jar Isings form 67b or c.
6 Part of a whetstone of the familar type with herringbone 'decoration'.
7 Wall-fragment of a cup of Arretine terra sigillata (not illustrated).

9.3 CONCLUSIONS

Periodization

The excavation and other research in Meinerswijk have shown that the site was inhabited from at least the first decade AD into the Early- and possibly even Later-Middle Ages. Because its presence is primarily demonstrated by the evidence from borings and only very marginally by the data from the excavation trench, the post-Roman occupation phase(s) cannot be discussed in detail. In contrast, the site of the trench proved to be exceptionally favourable for our purpose: to gain an insight in the nature and chronology of the Roman Period occupation.
Although the excavated area is too small to take anything for granted, the stratigraphy and dating evidence are such that most if not all of the successive phases of the Roman habitation are represented. Only the earliest occupation, from the last decades BC, is missing which is all the more intriguing because the excavation did not reach virgin soil. On the other hand, there are no indications to conclude that such an early phase must have been present.
On the basis of the evidence discussed in the previous paragraph, the following phases can be proposed:
Period 1 : 2nd decade AD
Period 2 : pre-Flavian – AD 69
Period 3 : AD 70-IIa
Period 4 : IIa–IId
Period 5 : IId–IIIc
Period 6 : IVc–Va
The available data do not allow a more precise periodization. Although it contains a few chronologically fairly secure dates, there are also some important uncertainties. Period 1 can only be dated in the 2nd decade AD. The evidence is, however, wholly inadequate to fix the transition from Period 1 to Period 2 in time. If there was such a transition it should be under Tiberius, but for all we know there may well be a hiatus of two to three decades and the start of Period 2 may be a reoccupation of the site under Caligula or Claudius. The change from Period 2 to Period 3 is well founded, and for the following phases the time of transition can at least be narrowed down to periods of some 25 years. The hiatus between Periods 5 and 6 is quite probable, but its length cannot be determined precisely.
The end of Period 5 cannot be much earlier than AD 25(but it could be considerably later, especially when the (related) problems of general scarcity and identification of late-3rd century material are taken into consideration The beginning of Period 6 depends on the construction of only one structure that accidentally survived in the small excavated area and, although supported by the distribution of other finds, that structure is dated by only one sherd. Fortunately, the association between both is absolutely certain and the rim-sherd itself can be confidently dated to the late-4th century, but in itself that is not a very precise dating.
Although not necessarily relevant to the present discussion it is worth while to note that the periodization of Meinerswijk might be broadly similar to that established for the successive forts of Valkenburg[37]: Meinerswijk 2 ≈ Valkenburg 1–3 (AD 39/40–69), Meinerswijk 3 ≈ Valkenburg 4 (c AD 70–100), Meinerswijk 4 ≈ Valkenburg 5 (c AD 100–174), Meinerswijk 5 ≈ Valkenburg 6 (c AD 178–240), and Meinerswijk 6 ≈ Valkenburg 7 (c. IVbc)

Interpretation and historical context

Finds and features from the excavation leave little doubt that we are dealing with a military establishment. Although its layout and even extent in all of the successive phases are still unknown, the available data, in combination with such information as the geological context and the location in relation to rivers and surrounding sites, as well as the general historical background, do allow a number of conclusions.
As mentioned in the preliminary report,[38] the dating of the first occupation phase almost inevitably leads to the

36 Cambi 1983, 372 and fig. 16.
37 See Glasbergen/Groenman- van Waateringe 1974, 6 and Groenman- van Waateringe, in press (for Period 7). See also chapter 12, 446, 449–50.
38 Willems 1980a, 338.

assumption that Meinerswijk must have been one of the camps used by Germanicus when he assembled his troops in the *insula Batavorum* in AD 16.[39] If the interpretation of the graffito (fig. 98, top) is correct, the camp in Meinerswijk may have been used by legionary soldiers of the *legio V Alaudae* which was stationed in Xanten at that time. The geological situation around the site as discussed in chapter 3 would have made it very suitable as an assembly point for ships, no fewer than a thousand of which are reported to have been used in the campaign.[40]

The presence of an early camp in Meinerswijk has also been used (chapter 3) as an additional piece of circumstantial evidence to argue for the IJssel as a navigable stream in the Roman Period and which may well be identified with the *Fossa Drusiana* (cf. fig. 19). The evidently important strategic position of Meinerswijk at the point where the Rhine turns westwards would be even more significant if the identification is indeed correct. By itself this important position, confirmed by the later use of the site as a frontier fort, suggests that continued occupation after Germanicus' campaigns is perhaps more likely than immediate abandonment and reoccupation sometime around AD 40.

However this may be, it is clear that Meinerswijk must have been incorporated as some sort of frontier fort in the *limes* of Germania Inferior from the construction of the frontier under Claudius until its end in the late-3rd century. The details of the site's development and occupants during that time will need to be examined by future excavations. There are only a few rather general observations to be made on the basis of the material now available.

For Periods 1 or 2 there is clear evidence that the natural resources in the area north of the Rhine, the Veluwe, were exploited. At least there is clear evidence for the harvesting of heather and bracken or the cutting of heather sods that can only have come from that area. The first restricted but unmistakable evidence for stone buildings occurs only in the Flavian Period 3 which is, however, quite early compared to other frontier forts and may well be a further indication for the importance of the site. If the gladius 'from the Rhine near Oosterbeek' indeed originates from Meinerswijk, as was proposed above,[41] it could provide an indication for the presence of a legionary detachment in Meinerswijk. But this remains a mere possibility.

Evidence for a complete reconstruction in stone is only available for the 3rd century Period 5 and, as indicated by an inscription and a tile-stamp, the work was probably undertaken by soldiers of the 1st legion from Bonn. Building material from this fort was reused in the late-4th century for the construction of a wall (Period 6). Pottery finds, especially to the north of this structure, provide clear evidence of occupation into the 5th century. Strictly speaking, the small excavation has not produced definite evidence that late-Roman occupation was of a military nature. But although regrettably little is known as yet about the structure and extent of the late-Roman occupation it is clear that the alternative, a non-military occupation involving some substantial work and labour-investment in a former frontier fort on the Rhine in the late-4th century, is hardly if at all plausible.

A military occupation, however, would fit well into the picture of late-Roman activities along the Rhine as presented by Ammianus Marcellinus and, more important, is supported by a steadily increasing amount of archaeological evidence.[42] On the basis of the *Tabula Peutingeriana* it should be concluded that Castra Herculis, the first of the seven 'towns' which were refortified by Julian in AD 359,[43] is located in the area covered by the present study.[44] More specifically, it should be located on the Rhine downstream from Quadriburgium which can presumably be identified as Qualburg (site 460) and its position can be even more precisely calculated if the system of roads and places as presented in fig. 21 is more or less correct:[45] some 17.5 km north of Noviomagus–Nijmegen[46] and some 29 km east of Carvo–Kesteren (?).

These considerations at least suggest that the identification of Meinerswijk with Castra Herculis is quite probable. Castra Herculis should be located at least approximately at the point where the Rhine turns westwards but

39 Stein 1932, 92.
40 Tacitus, *Ann.* II, 6.
41 See p. 301 and note 264.
42 See also chapter 12.
43 Ammianus Marcellinus XVIII 2, 3–6.
44 See also the remarks in paragraphs 3.4.1 and 3.4.3 on the limitations of the *Tabula* as an independent source.
45 Note that fig. 21, due to an unfortunate printing error, is *not* correct for the *distance* between Noviomagus and Castra Herculis. As is evident from fig. 20, this should have been VIII (leugae).
46 See also chapter 8, p. 250.

its location at another site cannot yet be ruled out completely. From the known sites only nos. 123 in Driel and 135 in Huissen could provide an – archaeologically less feasible[47] – alternative.

There are no plausible arguments to reason Castra Herculis away from the Rhine, for example, by translating Rhenus as Waal instead of Rhine.[48] Although perhaps possible in theory, such a move is not only contrary to the consistent information of various classical sources if taken simply at face value but also unlikely in view of the archaeological evidence for late-Roman activities along the Rhine. Moreover, Ammianus in his enumeration does *not* include Noviomagus. As already noted by Haalebos,[49] if Castra Herculis were located on the Waal to the west of Nijmegen, that is very odd indeed! The solution to Haalebos' problem is, of course, very simple: whatever did or did not occur in Noviomagus was of no concern to Ammianus in the quoted paragraph because it was not related to his subject matter there, the enumeration of accomplishments of Julian along the Rhenus = Rhine.

What happened to Meinerswijk–Castra Herculis after it was finally abandoned as as Roman military stronghold is largely unknown.[50] It is certain only that the site was inhabited and even expanded in the Early-Middle Ages. It presumably belonged to the royal fisc, and Verkerk, in his historical reconstruction of Arnhem,[51] assumes that Meinerswijk may be the fort mentioned in the charter of 9 June, 726, in which Carl Martel donates the *villa Eliste* (Elst), situated in the *locus* (area) Marithaime *ubi castrum fuit*, where a fort has been. As mentioned in chapter 8.6.1, Eliste or Heliste and other property had been gi en to a certain Everhard by Childebert and was co fiscated again for treason. It is uncertain but apparent generally accepted by historians that this Everhard b longed to the family of the Meginhardi.[52] As already no ed by Heidinga, the early-8th-century territory of Eve hard apparently encompassed the *vicus* of Meginhar which makes a family relationship plausible. This plac which is known as *Meginhardiswich* in AD 814 and *vic Meginhardi* in AD 847,[53] was probably an emporiu raided by Vikings in the latter year. It is undoubted the same as Meinerswijk.

The post-Roman layers and the late- or post-Mer vingian expansion of the site (cf. fig. 18) thus make very likely that it is, or that it is part of, the early-med eval emporium. Although the how and why of the 8t century and later developments are beyond the scope the present study, it is perhaps useful to note two facto which may have been important in that respect. Firs the medieval site may have grown up around Cast Herculis especially because it was still a stronghold i late-Roman times and it may have been rather more tha a mass of ruins or partially eroded by the river. Secon this early-medieval development would not have o curred if the position of Meinerswijk had not still bee favourable in relation to land and water (trade) route The later development of Arnhem at the other site of t Rhine, as pictured by Verkerk,[54] was the result of number of factors, but from a macro- or supra-region perspective the location of Arnhem and Meinerswijk a not fundamentally different.

47 See above, p. 309–10.

48 Bogaers 1968, 157. See also chapter 12.

49 Haalebos 1976, 205.

50 The site is presumably the same as the Coadulfaveris (see fig. 140) mentioned by the Anonymus Ravennas (IV, 24). That Coadulfaveris = Castra Herculis is now generally accepted, see Bogaers 1968, 152 and note 18, Ewig 1980, 11. The Ravenna geographer probably wrote his *Cosmographia* in VIIIa (Cf. chapter 2, 27; Bogaers 1968, 152, note 15), but VIId is also maintained by some (e.g., Ewig 1980, 9). See also chapter 1 note 91.

51 Verkerk 1983, 6.

52 See Heidinga 1984, 216–7.

53 Sloet 1872–6 (*OBGZ*), nos. 27 and 39 (*Ann. Xantenses* Meginhardiswich is, by the way, the oldest *-wik*-name in t Netherlands and Westphalia (Schütte 1976, 197).

54 Verkerk 1983.

10 THE RIVER AREA: THE NATIVE BACKGROUND

10.1 INTRODUCTION

As indicated in its title, this study is concerned with Romans and Batavians or, in more general terms, with allochthones and autochthones and the interaction between them in a specific area and during a specific stretch of time. The present chapter is intended to set the stage for this: to examine the conditions in the area before the point when the process of interaction started. In chapter 2, this point was defined as the year 12 BC, in which large numbers of Roman troops arrived in the river area. As already mentioned there, one may question the adequacy of this choice on the grounds that more indirect Roman influence had already caused significant changes.

This is a matter related to the question of who precisely were the autochthones and the content of the term Batavians. We cannot ignore the explicit historical evidence for a new group of people settling in the river area, but it is by no means coherent and is certainly difficult to interpret. To reach a conclusion about this problem with the help of the archaeological data is important. But it is only part of the larger problem of how to interpret the available knowledge to offer some reasonable propositions about social and economic developments during the last centuries BC and conditions in the region on the eve of the events starting with the arrival of Drusus' troops.

In order to do this, a number of steps have to be taken. It is necessary first to examine the historical information and notably from an anthropological perspective. This can be done with the help of similar studies on various aspects of Celtic[1] and Germanic[2] societies, and on their interface.[3] These provide material from which a model of conditions in the river area can be deduced. At the same time such studies, as far as they are also based on archaeological data, can be used to gain insight into factual conditions at a supra-regional level. As such, they are also helpful for a theoretical approach, when one attempts to formulate some ideas about the general processes which led to the events of 12 BC and later. But this is a subject which merits a separate discussion.[4] In this chapter the first step must be to interpret the (supra-regional) situation up to and including the period of the Gallic wars, and the second to evaluate as far as possible the resulting developments in the next phase, between 51 and 12 BC, with direct reference to the river area itself.

10.2 THE SOUTHERN LOW COUNTRIES UNTIL THE END OF THE GALLIC WAR

10.2.1 Historical Information

In recent archaeological literature a wealth of evidence has been collected showing increasing socio-cultural complexity during the last centuries BC in areas on the northern periphery of the Roman republic, notably in central and eastern Gaul.[5] One aspect of this phenomenon is that a flow of information must have existed, the consequences of which are especially visible in its northern direction but which were, of course, reciprocal. Information about Celtic societies thus reached the Mediterranean where some of it was recorded and of these records scraps and pieces have even survived into modern times in various ways.[6] The first complete report which survives, however, is Caesar's *Commentarii Belli Gallici*. Apart from the deficiencies and merits of all surviving early sources, which were and are the subject of much scholarly discussion, one thing is very clear: they virtually lack information which is directly pertinent to the Dutch river area. It is possible to reconstruct in a general way the location of various tribes in its surroundings (fig. 118), but that is about all. Everything else has to be pieced together more or less indirectly. This is, in one way, a curious state of affairs. After all the river area or, in other words, the Rhine-Meuse delta, is by its nature situated at potentially excellent channels of communication with central and eastern Gaul. It may partially be

1 The relevant literature is considerable, but see esp. the works of Crumley (1974) and Nash (1976, 1978a, 1978b, 1984).
2 E.g., Thompson 1965, Krüger (ed.) 1976, Jankuhn 1976, Mildenberger 1977, and Steuer 1982, to name only a few of the more important and recent works.
3 See Hachmann/Kossack/Kuhn 1962, Mariën 1971, Hachmann 1976, and Roymans 1983.
4 See below, chapter 11.2.
5 Cf. note 1 above.
6 Of central importance (cf. Norden 1922, 65 f. and 170) are the largely lost books of Posidonius. See *ER* I, 27 and esp. Tierney 1960.

explained by the fragmentary nature of the surviving sources. But Caesar's reports are not fragmentary, and the only reference to the *insula Batavorum* (IV, 10) is generally considered to be a later interpolation.[7] Even when it is assumed that most of the delta was inhabited by the Menapii, which is a possibility but not a very serious one,[8] the information provided is rather vague and imprecise.

The conclusion from this lack of information must be that the area was apparently not very interesting from Caesar's point of view. As he admits himself it was very difficult to control the Menapian territory, and he implicitly acknowledges two failures in the third (III, 29) and fourth (IV, 38) year of the war. Despite the evidently great difficulties caused by the character of the landscape the tribe was finally brought under some measure of control in the sixth year (VI, 6) when it had become absolutely vital for tactical and political reasons.[9] It was thus difficult but not impossible to handle the Menapii, and one can be quite sure that had their land really been as important at the time as one might assume because it encompassed the mouths of three major rivers (Scheldt, Meuse, and Waal) it would have been duly conquered. In view of their resistance, the inhabitants would most likely have suffered the same fate as some of their neighbours, such as the Nervii, and certainly the Aduatuci and Eburones who were virtually annihilated.

It is unlikely that the Menapii also inhabited the eastern part of the delta,[10] which must have been relatively more accessible than the western area, but we can safely assume that where control of the western region was only important for specific reasons and not so much for itself, the same is true for the eastern river area. It could have belonged to the territory of the Eburones,[11] but then it does still not figure prominently, or at all,[12] in the descriptions of the campaigns in this territory. The one clear reference to the river area is the place where the German invasion-force of the Tencteri and Usipetes was finally crushed, at the junction of Meuse and Rhine (IV, 15). This Rhine can only have been the Waal, and the easternmost point could conceivably have been at Rossum/Lith (see figs. 123, 130, or 135) but if the ab sumptions are correct it was further to the west b the invasion had been in the territory of the Menaj that of the Eburones.

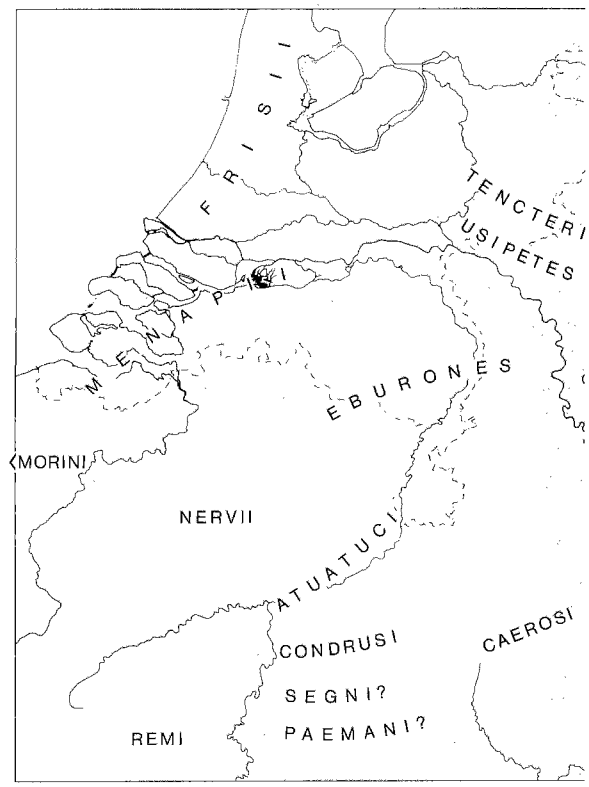


Fig. 118 The approximate location of tribes at the Caesar's conquest.

However this may be, it is clear that Caesar had n interest in the river area as such and the maximu incidental extent of his activities is almost certai Waal-branch of the Rhine. This shows that the was at least not of real strategical value and an imp link in any supra-regional exchange and commun network in his day. By implication, we may co that in the Late-Iron Age, as seen from central an

7 Cf. chapter 2, 23, note 16 (with a printing error: read book IV, not VI), but see p. 370 below.

8 Cf. Bloemers 1978a, 76, note 17; Van Es 1981, 25–7.

9 Namely, the possibility that they might otherwise provide a refuge and new operational base for Ambiorix, leader of a Belgic revolt, who had caused serious trouble (VI, 5).

10 De Laet 1961; Van Es 1981, 25.

11 Van Es 1981, 27 and note 34.

12 The only possible instance (VI, 31, 3) is a refer islands where Ambiorix' men hid themselves. Althou unlikely to pertain to the Overbetuwe or the Land van M Waal (see Van Es 1981, note 32), this is not inconc either (cf. chapter 7.2.1).

ern Gaul, it was on the periphery if not indeed at the dead-end of such a network.

This argumentation *ex nihilo* is, of course, not without its problems but it is supported by other inferences from the classical sources. We can take it for granted that the social organization of the population was very much like that of the Menapii or Eburones, who belonged to a group collectively described as Belgae. Some of these, the Eburones, Condrusi, Segni, Paemani, and Caerosi, are repeatedly denoted as Germani Cisrhenani,[13] and we can confidently assume the same applies to the population of the river area.[14] The literary evidence for the North Belgic tribes in general has recently been discussed by Roymans.[15] He reaches the conclusion that 'In Caesar's time the North Belgic tribes exhibit a number of traits typical of rather primitive, highly segmented tribal[16] societies that are made up of a large number of smaller chiefdoms. Particularly indicative of this is the absence of social stratification, a central political authority, an integrated economy, and a hierarchical settlement structure having urban centres'.

As noted by Roymans, there is thus a marked contrast between these people and the much more complex societies further to the south. They are also different from the tribes in the Belgic 'core' area north of the Seine and in the Somme valley, of whom they can be considered to constitute a kind of periphery at least in the last century BC.[17] In practice, the North Belgic tribes are very much like what can be inferred about Germanic society in general.[18] Although it is not necessary to discuss here in full all arguments provided in the literature mentioned, it is useful to summarize some important aspects which should be characteristic of society in the river area during the Late-Iron Age.

First, only limited social stratification is to be expected. In general, tribes and tribal segments (*pagi*) had chieftains, together making up a tribal council (*senatus*) but normally without a central authority or even coercive power superseding that of any one *pagus* should it wish to act differently.[19] Characteristically, central leadership was only temporarily granted in times of war,[20] although in some cases there were peace-time chiefs as well.[21] This may well have been connected with a kind of tribal aristocracy formed by a leading clan or lineage, but there are numerous indications that these cannot have been people at the apex of a truly stratified society and that there was, on the contrary, a strongly egalitarian ideology.[22]

Second, the level of economic development is limited, as well as interregional trade. There are virtually no indications of more or less urbanized regional centres among the Belgae. From the context of Caesar's report one might deduce these for the Nervii (*BG* II, 28) and the Aduatuci (*BG* II, 29) but normally, if *oppida* are mentioned at all, such as in the last paragraph, one can also interpret them as temporary refuges. A much more com-

13 Even the Nervii, whom Caesar calls Belgae, are reported to have been Germans by Strabo (*Geographica* IV, 194) and later also Tacitus (*Germ.* 28), but the interpretation of these comments is uncertain, see *ER* I, 102 v. Hachmann a.o. 1962, 45–6.

14 Strictly speaking, those living north of the Waal could well have been considered Germani *trans*rhenani by Caesar and at least for the area between the mouth of the Waal and the Oude Rijn there is even evidence for that because the area (see fig. 118) was probably inhabited by Frisians (Van Es 1981, 26).

15 Roymans 1983, 51–5.

16 Note that here 'tribe' is used as a modern anthropological concept cf. the terminology of Service (1962, chapter 4) and Sahlins (1968), comparable to what Fried (1967, chapter 4) has termed 'rank societies'. There are thus tribes, notably those in central and eastern Gaul, which are not at all tribes in a 'narrow' anthropological sense (see Roymans 1983, note 3) but stratified societies (Fried 1967, chapter 5). The ambiguity of the tribe concept (cf. Fried 1967, 154 f.) is unfortunate but apparently unavoidable.

17 Cf. the analysis of Hachmann 1976 (esp. fig. 1), followed by most authors on the subject and recently, most explicitly, by Nash (1984, esp. 99–105).

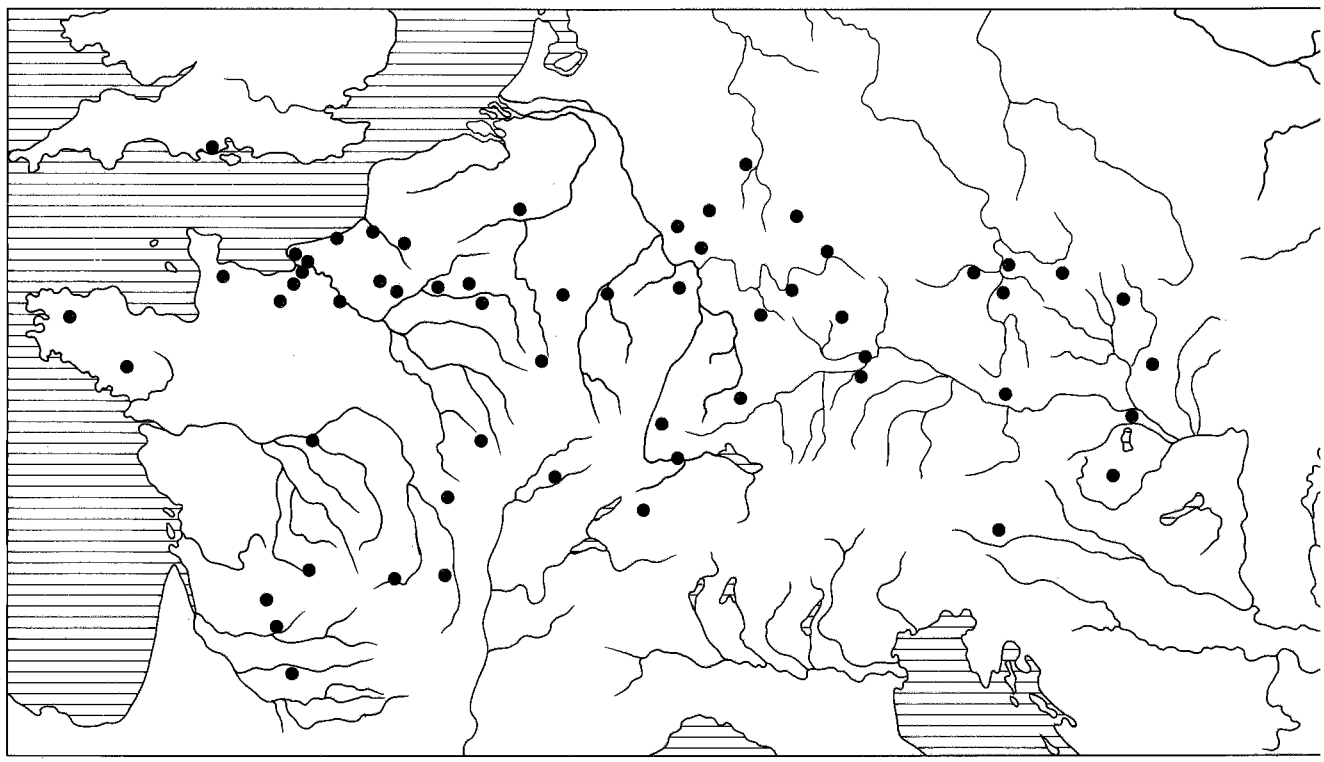
18 Cf. note 2 above, esp. Thompson 1965 and Much 1967.

19 The dissident *pagi* of the Morini (Caesar, *BG* IV, 22) are an example, as well as the segment of the Chatti known as Batavians (Tacitus, *Hist.* IV, 12 and *Germ.* 29).

20 Cf. e.g. Fried 1967, 181–2 and Sahlins 1968, 45, and Caesar's comments in *BG* II, 23 (Nervii) and VI, 23 (Germans in general).

21 The first is termed a *dux*, the latter (a.o.) *rex*, and there is a clear difference between the two, as formulated by Tacitus, *Germ.* 7: *reges ex nobilitate, duces ex virtute sumunt*. This refers to Germans at least a century later (cf. Thompson 1965, 32–41), but it may have been the practice in Caesar's time as well. The Nervii chose a *dux*, but the Eburones had two *reges*. Although the latter office seems to have been held for life and was reserved for members of a royal lineage or clan (*stirps regia*), it implied no coercive power (see esp. Thompson, *op. cit.*, 36–7 and Caesar, *BG* V, 27).

22 Expressed in its purest form in *BG* VI, 22.



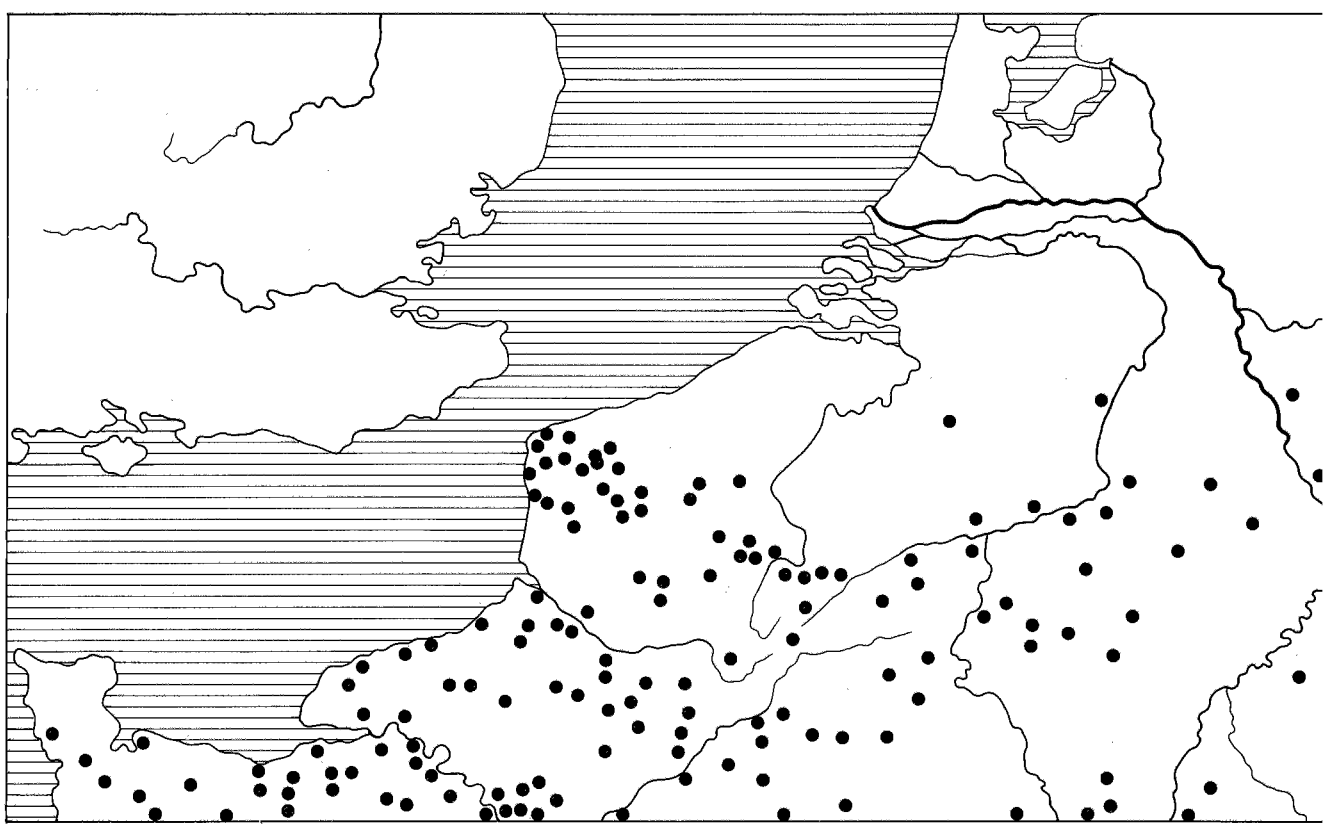


Fig. 119 The distribution of A: late-La Tène defended *oppida* of over 25 ha (above), after Collis 1982, fig. 9.1, and B, late-La Tène fortifications of all sizes (below), after Hachmann 1976, fig. 5.

mon response to an outside threat was to scatter widely. This is repeatedly reported for the Menapii and also for the Eburones (*BG* IV, 34): *Erat, ut supra demonstravimus manus certa nulla,* non oppidum, *non praesidium, quod se armis defenderet, sed in omnis partis dispersa multitudo.*[23] Clearly, this implies that there were no central places to defend at all costs or, rather, considered necessary to defend. Urbanized *oppida* serving as central administrative places were vital institutions in central and southern Gaul, representing considerable investment.[24] Not surprisingly, indications for central accumulation of wealth and for tribute flows are also lacking as well as those for forms of redistribution managed by and in support of an elite hierarchy.[25] By contrast, there is evidence (for Germanic societies) of redistributional levelling mechanisms counteracting wealth concentration, such as the annual reallocation of arable mentioned in *BG* VI, 22. In accordance with this, interregional trade, which could have provided an elite with prestige goods, was also lacking and merchants did not often visit the Belgae (*BG* I, 1). From the various descriptions of difficulties encountered in the movement of Caesar's troops it can be concluded that a network of proper roads was lacking, again in contrast with more southern areas.[26]

In conclusion, it can be said that in contrast to theoretical possibilities, the position of the river area was apparently not advantageous enough, or simply too far away, for the inhabitants to have reached a level of socio-cultural development which would have made them, or their territory, figure more prominently in the historical record. An additional illustration of this state of affairs is provided by the clear evidence that it did not figure at all in the contacts with societies in southeastern Britain, which went by way of the Belgic 'core' tribes on the coast of northwestern France.[27] This conclusion is strongly supported by what the sources reveal about the weakly differentiated and integrated social and economic organization of groups in or directly adjacent to the Rhine-Meuse delta, and differences with what is reported about Germanic society are only slight. It is also supported by another source of information: the general picture provided by archaeological correlates of social and economic complexity at the supra-regional level.

10.2.2 Archaeological Evidence

The archaeological information available for approximately the southern part of the area between Scheldt and Ems as described in chapter 1 (see fig. 4) has, fortunately, been the subject of some research already, but an encompassing, detailed investigation of this information is still badly needed. A great deal of material has not yet been studied or not in a coherent way.[28] What is available today is, however, enough to be fruitfully used as an independent source of information about the same subjects as in the previous paragraph, especially when viewed from the even larger perspective of northwestern Gaul.

One of the most prominent phenomena marking the transition, in Fried's terminology, from rank to stratified society or, in other words, the process of secondary state formation in Gaul, are the large 'urban' *oppida.* Apart from being heavily defended refuges, they were the optimally located centres for the administration of the different socio-political units and the seats of the ruling elite, which also made them centres for regional and long-distance trade and obviously themselves foci for the accumulation of luxury goods. The development of stratified society, in spatial terms from south to north and chronologically from the 2nd into the 1st century BC, is increasingly better understood.[29] From the distribution of such *oppida* as presented in the numerous publications (see fig. 119a) it is very clear that they occur

23 'As above mentioned, there was nowhere any definite body of troops, *any stronghold,* any garrison to defend itself in arms, but the population scattered in all directions' (transl. H.J. Edwards, Loeb Classical Library no. 72). See also *BG* III, 28 where the Morini and Menapii are reported to respond 'with tactics quite different from the rest of the Gauls' to evade a major conflict, scatter, and resort to guerilla warfare.

24 See esp. Nash 1976, Collis 1982.

25 On aspects of redistribution, see e.g. Earle 1977. For more southern groups, there is ample evidence for this, cf. Roymans 1983, 47.

26 Cf. the references in Roymans 1983, note 23.

27 Cf. e.g. Hachmann 1976, Haselgrove 1982, Cunliffe 1984.

28 It is currently being undertaken by Roymans.

29 See the various contributions in Cunliffe/Rowley (eds.) 1976, Nash 1978b, Collis 1982.

only up to the southern and southeastern limits of the area between Scheldt and Ems as outlined in fig. 4.[30]
The *oppida* were only the apex of a diverse range of settlement types, including large sites which are also lacking outside their area of distribution. There are, however, in northern France, Belgium, and the Rhineland, numerous other fortified sites which were constructed or reoccupied in the Late-La Tène Period.[31] These extend from the Pas de Calais and along the upper course of the Meuse up to the Ruhr valley (fig. 119b), and thus reach into a zone of approximately 100 km north of the northernmost *oppida*. This has been interpreted as an archaeological manifestation of a prolonged unstable situation, such as could be caused by frequent raiding, for example, for captives to be exchanged as slaves with Roman traders,[32] but raids by Germanic tribes and, possibly, partially internal developments could be mentioned as well. At the same time it can be seen as a first step, even if forced and temporary, towards nucleation which could have led to a development similar to that in central Gaul.[33] There are still problems with dating many of the sites involved, but it is fairly certain that among the North Belgic tribes up to and – not to forget! – during Caesar's campaigns, a kind of crisis situation produced nucleation. There is little evidence for a more permanent character of this nucleation, let alone for true central places. But then it has to be remembered that the period involved is probably in the order of only half a century.
However this may be, fig. 119b clearly demonstrates that although fortified sites could hardly have been established in the Holocene river delta itself they could have been constructed on the high Pleistocene ridges surrounding it, and, moreover, there is still an empty zone of approximately 100 km to the south of the delta where such sites are also lacking. One might suggest that the area was simply too far away for raids from the south. When a threat from the north is considered, it was perhaps not so much itself threatened but rather the southernmost part of the region where the danger came from. But in either case it cannot have been of major importance on strategic or political grounds, otherwise there would have been attempts to control it and conceivably some archaeologically visible response to such attempts. Therefore, we cannot but conclude that the population in the area concerned must have had a rather low level of socio-political organization. This is the reason for the poorly developed, and for the lack of developments in, settlement pattern which was (or became) different in adjacent southern regions.
As was to be expected, this conclusion is supported by the evidence from burials, although that is not exactly abundant. The southern Netherlands and northern Belgium seem to belong to one group which is characterised by increasing simplification in burial rites throughout the Iron Age, culminating in the archaeologically near invisibility of burials in the last centuries BC (cf. paragraph 8.2.2). Apart from what they might mean, wealthy burials are lacking from this region.
This situation is different in a zone from the middle-Rhine valley along the river Mosel over southern Belgium and in France north of the Seine (and from there extending into southeastern Britain). There are quite a few late-Iron Age burials known from this zone and, notably from the last century BC, also a number of wealthy burials.[34] There are various unifying characteristics in this burial tradition which should not be seen as a direct derivative from southern traditions.[35] The particular manifestation of burial customs south of our area is not entirely – in fact just barely – pre-Caesarian and a direct interpretation in terms of social personae recognized by those customs is not advisable because we know how much change there was in the area during the 1st century BC. The rich burials as such can perhaps best be seen as a form of conspicuous consumption of commodities, motivated by a desire for political legitimation. This is especially relevant in a society where rules are changing and unclear, with resulting competition over social positions.[36] It could also provide a reason for the observed

30 E.g. maps in Collis 1975, Collis, in Cunliffe/Rowley 1976, fig. 4, Nash 1976, fig. 1, Hachmann/Kossack/Kuhn 1962, *Karte* 2, and many more.
31 Collis 1975; Hachmann 1976.
32 Haselgrove 1984, 17–8; Nash 1984, 99–105.
33 For a simple and elegant model of changes in a settlement pattern in a crisis, and various possible consequences, see Collis 1982, 74–6.
34 See De Laet 1982, chapter 12 and the overviews composed by Mariën (1971), Collis (1977), and Cunliffe (1984).
35 Caesar (*BG* VI, 19) explicitly mentions the splendour of Gaulish funerals. See also Duval 1984.
36 Cf. Haselgrove 1984, 22–3. A similar line of reasoning is adopted by Pearson (1984, 78–80) to understand the increasingly rich Danish burials during the same period, although developments there are seen primarily as the result of internal processes.

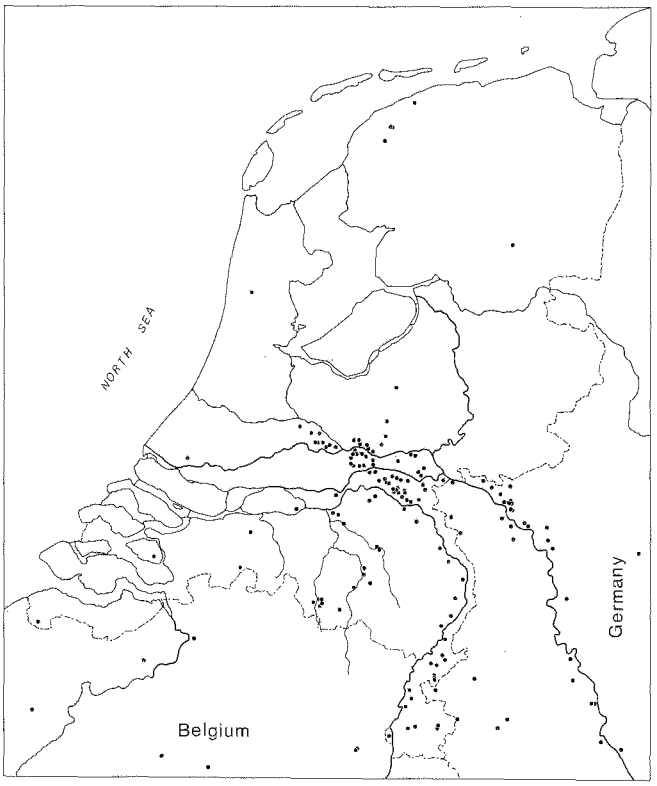


Fig. 120 The distribution of late-La Tène glass bracelets, after Willems 1983a, fig. 5.3.

distribution, assuming the causes are to be sought in trouble coming from the south. At the same time, we may conclude that a similar situation apparently did not exist in the southern Netherlands. Burial customs there remain the same and convey no sense of either the recognition of a deceased's status or of social legitimation and competition. Instead, as far as the surviving archaeological record is concerned, the burial evidence indicates sound and stable egalitarianism.

The general lack of wealthy burials does, by the way, not imply that accumulation of wealth by some more powerful individuals was entirely absent. There are a number of precious objects known, as well as hoards,[37] but they are relatively scarce. The same is true for Celtic coins, which are almost exclusively of gold and very rare until the end of Caesar's campaigns. Such coinage is generally considered to have functioned primarily as special purpose money in the prestige sphere.[38] There are, however, arguments to assume that in the southern part of the Low Countries, where coins are so scarce and only the Nervii and Eburones are known respectively supposed to have minted their own and only at a relatively late date,[39] the Celtic glass bracelets may have fulfilled the same function. But even when this interpretation is accurate there is still a difference from the area of circulation of gold coins and if the distribution of coins and (certain types of) glass bracelets should prove to be complementary as is – probably correctly – suggested by the distribution map (fig. 120), that difference is even strengthened.[40]

As far as primary agricultural production is concerned it is not probable that the southern Low Countries differed much from central Gaul. One might assume that surplus production was needed on a far smaller scale, but this is not necessarily true. Not so much because there was a large surplus-consuming elite here but because in central Gaul the size of the surplus-consuming population not directly involved in agricultural activities and living in the larger centres, relative to that of the total population, may not have been all that large either. Nevertheless, pollen analyses in the river area and the Rhineland have clearly shown a marked increase in culture-indicators and decrease of forest throughout the Late-Iron Age.[41] In principle, it would be more logical to attribute this to increased technical capabilities rather than an archaic technology as is sometimes suggested.[42] There are not, however, any signs that increased pro-

37 De Laet 1982, 628–35 and Roymans 1983, note 84.

38 See Scheers 1977 and esp. Roymans/Van der Sanden 1980. The latter publication also offers an overview (p. 181–3) of recent interpretations regarding the function of Celtic coinage.

39 Scheers 1977, 88–97. The first (gold) coins were minted during the Gallic wars. There is no evidence for coinage of the Menapii, Aduatuci, and the other Germani Cisrhenani apart from the perhaps somewhat uncertain (Scheers, *op. cit.*, 441–2) staters of the Eburones.

40 Willems 1983a, 110–2. On the limited distribution along the Rhine in Germany (only up to the Lippe valley) of forms Haevernick 3b and 7a–b, see Reichmann 1979b, 128–9 and 273–4. The distribution in Belgium is still largely unknown.

41 Teunissen 1982; see also paragraph 10.4.2 below and fig. 137.

42 E.g., Roymans 1983, 52. An increase in arable and forest clearance connected with agricultural innovation has been argued for Britain (Jones 1982).



Fig. 121 Celtic silver disc from Helden, diameter 21 cm (photo RMO, Leiden).

duction and population caused changes in the economy, which must have remained fairly autarkic. Exchange is visible in the distribution of glass bracelets and of products such as querns of basaltic lava (tephrite) from the Eifel and salt from the coast,[43] bronze or bronze objects, and perhaps iron. With the exception of the few extraordinary objects mentioned, such as the silver disc from Helden (fig. 121),[44] there are no indications of long-distance trade.

Pottery production was entirely a local affair and at a rather low level of technical sophistication. Wheel-turned pottery was not manufactured but, on the other hand, there is good evidence for the use of technically more advanced updraught kilns at least from the Middle-Iron Age onwards.[45] This could indicate semi-specialist production above the household-level but surely not sudden changes caused by influences from the south. In any case the pottery-production shows continuity throughout the Iron Age. It can be typologically differentiated from other groups,[46] and thus testifies to a relatively autonomous development.

10.2.3 Conclusion

Although the overview of the evidence as presented above is brief and incomplete, it is sufficient for a general evaluation of social, economic, and political conditions in the river area relative to those elsewhere. The Rhine-Meuse delta as a whole emerges not so much as a collection of islands occupied by fierce barbarians living on fish and birds' eggs (*BG* IV, 10) but as a fairly homogeneous and relatively isolated area with a thriving population, socially and economically well adapted to its surroundings. We cannot be sure of the tribal groups constituting this population, but it is certain that the delta was occupied by, or the border between, several of these. The western part was occupied by Menapii in the south and Frisii in the north. The eastern part was perhaps inhabited entirely by (a group associated with?) the Eburones, but it may have been divided between Germani (Tencteri, Usipetes?) in the north and Eburones in the south.

These divisions do not, however, seem to be very important. Undoubtedly the different opportunities offered by the landscape will have caused economic diversity between west and east in general but also – as will be discussed below (10.4.2) – between settlements in one area. But the overall level of economic and social organization must have been – as far as we can tell – the same throughout the delta. Both the historical and the archaeological record allow the conclusion that this level is comparable to what is known about Germanic tribes in general and quite different from that in Belgic Gaul north of the river Seine and around the Somme valley, let alone from that in central Gaul. The archaeological evidence might even be taken to suggest that there was a difference, at least in the last century BC, between the northernmost areas of the Menapii and Eburones and the other 'North Belgic' tribes. This evidence (fortifications, dispersion of coins, and the like) is, however, more likely to be the result of increasing instability in these regions caused by interference from the south, from which the Menapii and Eburones were largely spared until they

43 See Van den Broeke 1980; Van Heeringen 1985.

44 The disc was found in the peat area of De Peel (see fig. 135, SE corner) and is quite probably a votive offering. See also Allen 1971.

45 Dutch sites are those in Serooskerke, Ressen-Bemmel (site 165), and Horst-Meterik (Willems 1984, 372–4).

46 Cf. e.g. Mariën 1971, 215, who proposes an identification of the *Groupe de la Campine* with the Eburones.

were directly affected by Caesar's campaigns. There are no good reasons to assume that there were already deeply rooted differences before the Gallic wars and – although chronological control leaves much to be desired – there was perhaps not more than a few decades or half a century for this process.

This last assumption seems to be contradicted by evidence for the emergence of a well-defined elite group in the social hierarchy in southern Belgium at an earlier date, in late-Hallstatt – early-La Tène times. This could mean that much older differences existed, but such a conclusion implies the assumption of a more or less continuous development. There is, however, an alternative for such an interpretation, which is most strongly suggested by the evidence from the southern Netherlands and northern Belgium. There, it is evident from a number of rich late-Hallstatt (Oss, Wijchen, Baarlo, Overasselt, Meerlo, Hegelsom) or early-La Tène (Nijmegen, Eigenbilzen) burials[47] that in the 7th-5th centuries BC an elite manifested itself for which there is no evidence later on. This development is paralleled elsewhere,[48] and indicates a more or less general cyclical process, the effects of which vary in different areas and are likely to have been determined by varying internal dynamics: for the southern Netherlands, the indications are that there was a reversal to an egalitarian society, elsewhere it may have been less marked.

Such a reversal does not of course need to have been as strong in reality as is suggested by the evidence and it needs to be explained as well. It is not very useful in the present context to speculate on this subject, but one may at least infer that the emerging elite was clearly not able, for whatever reason, to continue to manifest itself by prestige goods in any way. Either external input or internal demand or both were failing, and that should have had its consequences. But the important point is that the archaeological evidence for the southern Netherlands and northern Belgium clearly indicates a low level of social complexity during the Middle- and much of the Late-Iron Age. The North Belgic tribes in general can be seen as relatively autarkic and segmented tribal groups which only in the last century BC, and then primarily the southern groups known as the Morini, Nervii, Aduatuci, and the southern Germani cisrhenani, became subject to interference from the south.

This caused a number of changes, such as temporary nucleation in fortified sites and, in most areas for the first time in centuries, sure signs of an elite group manifesting itself. This can be seen as a first step towards a stratified society and it might well have resulted in a development similar to that in central Gaul one or two centuries before. As it is, the process was strongly affected by the direct interference from the core area itself: the Gallic wars. In a way, this may have accelerated internal processes of change, due to the greater need for organization of socio-political and economic affairs. The introduction of coinage by some tribal groups could have been one such effect. But one should not forget that the North Belgic tribes, even when Caesar's remarks on their bravery and bellicose nature are partly ascribed to self-justification and propaganda, did have social institutions to cope with an external threat, such as the installation of a war leader.

The external threat in question, however, proved to be of a magnitude that could not be coped with. In this respect, there is an interesting difference between the northernmost and the other Belgic tribes. In the north, the main response was to scatter widely and to hide out. Therefore, there was at the same time no firmly organized resistance to evoke attacks nor the possibility of easily gaining control by an overwhelming force. Unless deemed absolutely necessary for other reasons, such tactics cannot be answered by an army except by a very costly and long-drawn effort with no guarantee of success, as generals are still finding out today. In contrast to this, the better organized resistance in the south, where armed men were rallying instead of scattering and the population was concentrated at defended sites, left only two alternatives: submission or utter destruction.

Although dispersal and guerilla tactics are in themselves no proof, the different fate of people in the river delta and those to the south can, in this way, be properly understood as the result of a difference in social organization. Admittedly, there are other factors, but their importance is related to the same difference. For example, as mentioned earlier, one can be sure that if the delta area had been important, for example as a major connection with Britain which it was to become only much later,[49] it would have been conquered despite all difficulties. But then, if the area had been so important, its pop-

47 De Laet 1982, chapters 11 and 12, Bloemers/Willems 1980–81, 37–9, Bloemers, in press.

48 E.g., Mariën 1971, 219, the general characteristics indicated by Collis 1982, 77, note 1, Duval 1984, and the cycles described by Pearson 1984 for the Danish situation.

49 Cf. Cunliffe 1984 and esp. fig. 9.

ulation would undoubtedly have had a more developed kind of social organization. Also, the delta area is eminently suitable for dispersal tactics but it is, in principle, equally well possible to concentrate people and defences there, just as in other areas one can disperse and effectively hide out in woods and valleys (for example, the Sugambri).

The latter tactic was, in fact, adopted at one point by the Eburones (cf. note 23). But as a tribe they also had an elite group with two *reges* at the top who were capable of organizing resistance. Ambiorix continued to resist even after initial defeat, much to the detriment of the tribe which was virtually annihilated, to a large extent physically and entirely as a socio-cultural entity. One may wonder whether this included people in the river area, which depends on how far north one is prepared to postulate Eburonian territory and how thorough the genocide in all its reaches. But in any case, all North Belgic tribes suffered heavily from the war, also those who are known to have survived it as socio-cultural entities: the Morini, Nervii, and even the Menapii whose territory was, after all, repeatedly laid waste. Although the archaeological record has very little to say about this, we can assume from the historical data that the entire complex of interwoven societal systems was disrupted, the population reduced, and the subsistence base damaged. The consequences of all this, the events until the start of Drusus' campaigns, and the resulting situation in the river area, need to be examined next.

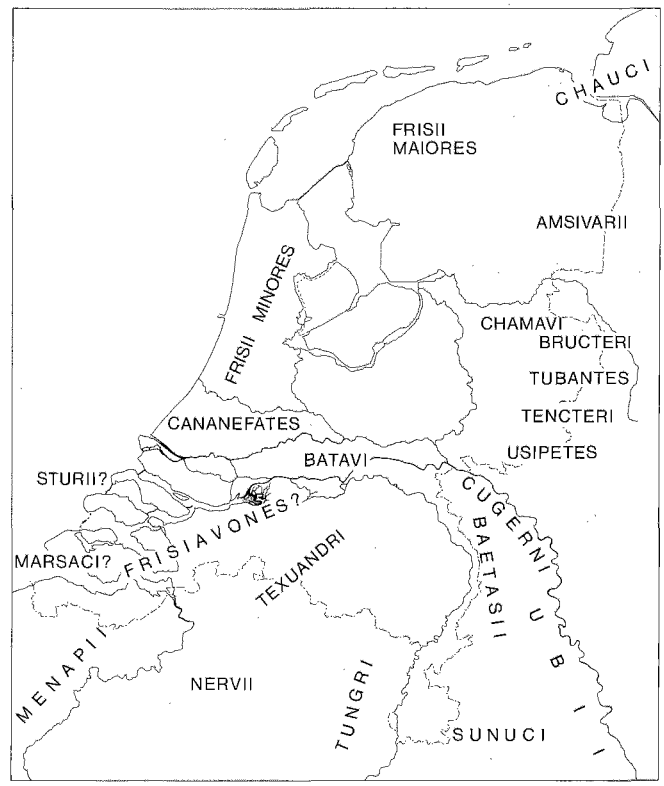


Fig. 122 The approximate location of tribes at the time of Drusus' campaigns.

10.3 FROM CAESAR TO DRUSUS

10.3.1 Historical Inferences

The period between 51 and 12 BC is notable for the absence of historical data pertinent to the situation in the southern Low Countries or, for that matter, to the entire former area of the North Belgic tribes. All we have are inferences based on the general framework of historical events and the information about the situation in 12 BC and later. The most important of these is, of course, the penetration in the area of tribes or tribal segments from across the Rhine. These are groups known as the Frisiavones, Cananefates, Batavi, Texuandri, Tungri, Baetasii, Ubii, and Sunuci. Their migration is not difficult to explain historically, in view of the previous destruction and depopulation of the area, although the reasons for leaving their former territories may have varied. Depending on the detail of later information their new locations can be reconstructed more or less precisely (fig. 122).

The degree of Roman interference with the area must have been restricted, although perhaps less so than could be concluded from sheer lack of information. After all, the civil war and the major enterprises of Octavian/Augustus, such as the war in Spain, attracted most attention. There is much to say for the assumption that the new groups settling in the area was the result of a conscious policy and not an entirely spontaneous process. We even known this for certain in the case of the Ubii,[50] and somewhat later the same thing is reported for the Sugambri.[51] With the probable exception of the western

50 Tacitus, *Germ.* 28 and Cassius Dio LVIII 49, 2.

51 Tacitus, *Ann.* II, 26 and XII, 39, Suetonius, *Aug.* 23 and *Tib.* 9. The relation between the Sugambri and Cugerni is uncertain, but must have been very close. The latter were perhaps a segment of the former (cf. Rüger 1968, 9; see also Vittinghoff 1976, 74 and note 5).

tribes, the Frisiavones, supposed to be a segment of the Frisii and later (?) separated from these by the Cananefates,[52] the new groups all came from the region east of the Rhine which is known to have had close contacts with the Belgic area. In addition, several are known to have been old allies of Rome. This is true for the Batavians whose relation to Rome was described by Tacitus (*Germ.* 29) as an *antiqua societas*, the Cananefates, who presumably held the same position,[53] and the Ubii of whom Caesar had already reported that they *amicitiam fecerant* and later *in deditionem venerunt*.[54] From the fact that they were settled on the west bank of the Rhine by Tiberius (cf. note 51) it follows that by then it was also true for (the Rome-friendly fraction of ?) the Sugambri known as Cugerni.

In contrast, military action was undertaken by Agrippa in 38 or 37 BC and various offensives are known against German tribes during the next two decades, including one (the *clades Lolliana*) that ended in defeat.[55] Groups such as the Suebi, Tencteri, Usipetes, and quite possibly a part of the Sugambri,[56] were prevented from settling west of the Rhine. The location of Cananefates, Batavi, Cugerni, and Ubii along the Rhine can hardly be a coincidence for they also effectively shielded off possible spontaneous migrants living *intorsus*,[57] not along the coast or the Rhine, such as the Frisiavones by the Cananefates.

The nature of these tribes in the interior is poorly understood but they appear to be for the most part rather makeshift compositions of older and new population groups, perhaps comparable to the anthropological concept of the composite tribe, the result of a breakdown and depopulation after contact with western civilization.[58] Possible examples are the Frisiavones (former Menapian territory, Sturii?, Marsaci?),[59] Texuandri (former Eburonian and Menapian territory; explicitly used by Plinius as a collective name),[60] and the Tungri (former territory of the Aduatuci, Eburones, Condrusi, Segni, Paemani, and Caerosi).[61]

There is every reason to believe, however, that we are not so much dealing with an entirely spontaneous regrouping into composite tribes but rather with a somewhat engineered process. There are several clues for this, such as the enforced internal peace,[62] placing new rulers at the head of tribes,[63] the actions across the Rhine, and the exclusion of some particular tribes mentioned above, or the very early proof, in 36 BC, for German troops in the Roman army.[64] All this, of course, does not mean that the Roman authorities were in perfect control in the entire period under discussion, nor that there may not have been – on the native side of the shield – a good deal of disintegration and disorientation, and not very much acculturation, as Roymans indicates. But on the other hand, there is no indication that one was 'at a loss in knowing what to do with this peripheral Belgic area' and neither 'did Roman power not manifest itself effectively and in a positive way'.[65]

Initially, there must at least have been a need for Octavian to secure the northern part of Gaul, and we can be confident that Augustus, perhaps even when still Octa-

52 Cf. Van Es 1981, 27 and note 35.

53 Cf. Bloemers 1978a, 81–2.

54 Caesar, *BG* IV, 16 and VI, 9. See also Rüger 1968, 5–9.

55 See the overviews in Rüger 1968, 8 and Von Petrikovits 1978, 53. As also noted by Rüger (1984, 10) a simple punitive expedition such as that by Marcus Vinicius against transrhenish tribes for the murder of Roman traders in 25 BC (reported by Cassius Dio LIII 24,2) was most likely a typical case for a whole series of similar events. This particular incident could, by the way, have survived in the records especially because this same person, a good friend of Augustus, became so important later, in 2 BC, as supreme commander of the Rhine legions.

56 Cf. Rüger 1968, 23, and note 99; Alföldy 1968, 84.

57 Plinius, *Nat. Hist.* IV, 106: *intorsus ... Nervii, ... Tungri, Sunuci, Frisiavones, Baetasii ...*

58 See Service 1962, 126–31. Also Roymans 1983, 57.

59 An overview and relevant literature in Bloemers 1978a, 79–80.

60 Plinius, *Nat. Hist.* IV, 106: *... Texuandri pluribus nominibus ...*

61 See e.g. Stein 1932, 5, Van de Weerd 1935, Bogaers 1971a, Mariën 1980, 59 and note 23, and the literature mentioned in Von Petrikovits 1978, 306–7.

62 E.g. Strabo, *Geographica* IV 3, 5, C. 194. For a general account of Roman measures, see also Drinkwater 1983, 20–4, 93 f., 120 f.

63 E.g. Commius for the Atrebates (*BG* IV, 21) and later (*BG* VII, 76) also the Morini. A later example is Italicus, made chief of the Cherusci (Tacitus, *Ann.* XI, 16).

64 Appianus, *De Bellis Civilibus* V, 117: Germani with Octavian on Sicily. As Bellen (1981, 39) convincingly argues, these are likely to have been raised by Agrippa in the foregoing years and quite probably especially among the Ubii and Batavi.

65 Roymans 1983, 57.

vian, planned various expansionist designs.[66] That these were purposively and effectively prepared is demonstrated by the arguments provided and, above all, by the fact that the former North Belgic territories along the Rhine were a readily available operations base for the large-scale conquests when needed. Nevertheless, most of what happened may well have been achieved by fairly indirect means, direct interaction remaining relatively limited. This is not only suggested by the historical record, which shows that the important events and thus the most direct Roman interference in local affairs were taking place elsewhere, but also by the archaeological information.

10.3.2 Archaeological Evidence

When regarded from the same viewpoint as the historical data, from a supra-regional and Roman perspective, the archaeological evidence in our area shows the same features. That is to say, on the whole such evidence is very deficient and indications for much direct interaction with the Roman administration or with traders are very limited. However, even in view of all the changes which must have happened in the period under discussion, one should not forget that it lasted only a few decades and the potential of the surviving archaeological record to register such changes, especially when followed by so many others immediately afterwards, is necessarily limited. Nevertheless, the nature of the changes such as large-scale depopulation and immigration of whole groups is such that the available archaeological evidence seems to be conspicuous by its limitation.

To be sure, it is not absent. For the territory of present-day Belgium, a number of data indicating changes, for example, in pottery forms, have been summarized by Mariën.[67] Other changes are the disappearance of gold coins, most of which seem to have been minted in the North Belgic area only during the Gallic Wars anyway, and the issuing of some silver and especially copper and potin coins.[68] This may be related to the use of natives for military service (the silver), but also be seen as a means to facilitate exchange in the new situation where traditional networks were disrupted and old neighbours perhaps replaced by new. A more or less contemporary[69] and quite probably causally related phenomenon is the (gradual?) disappearance of the Celtic glass bracelets towards the end of the 1st century BC. In some areas, notably the (Eburonian) region between Rhine and Meuse west of Neuss, there are perhaps also indications for the abandonment of settlements in the mid-1st century BC, an interpretation which is supported by the results of ecological analysis showing a marked decrease in agricultural activities there during the last century BC.[70] This is, however, not necessarily a general phenomenon and the ample evidence for the river area at least shows no such decrease.[71]

Direct (military) Roman presence in the area, and then exclusively in the south, can only be demonstrated from the beginning of the second decade BC,[72] in such sites as Liberchies, Tongeren, and Neuss.[73] But even evidence for indirect contact, in the form of Roman imports, is virtually lacking. Admittedly, one may expect that most of the conceivable trade products left no archaeologically retrievable traces but it is remarkable that one of the best indications for trade contacts, early Roman amphorae, are almost absent. From the distribution of such finds as Dressel 1 wine amphorae[74] it is abundantly clear that they almost entirely avoid the North Belgic area, the few known northern specimens being related to later Augustan camps. In fact, the distribution can only be taken as evidence for the relative poverty of the entire area but also for the fact that the new occupants were as weakly integrated into long-distance trade networks as were their predecessors.

66 Cf. e.g. Wells 1972, chapter 1, Mann 1979, 178–9, or Drinkwater 1983, chapter 1. A similar reasoning for Roman control and preparations along the Rhine in Rüger 1984, 10–11. For a penetrating analysis of Roman measures for control and infrastructural improvements in Gaul, see also Wightman 1977. For an opinion contrary to most writers on this subject, see Timpe 1975.

67 Mariën 1971, 228–34.

68 Roymans/Van der Sanden 1980; Roymans 1983.

69 Peddemors 1975.

70 Rüger 1984, 14–5. It is, however, unclear on which evidence such a detailed conclusion is in fact or, for that matter, can be based. Very detailed C14-dated pollen diagrams would seem to be the only possibility. At least in general the diagrams for the German loess area are rather more comparable to those in the Dutch river area (lecture Dr A.J. Kalis, Reuvensdagen-conference November 14, 1980, in Maastricht; see also Kalis 1983).

71 Teunissen 1982, 1984.

72 For an overview of earlier Roman activities further into Gaul, see Wightman 1977 and Drinkwater 1983.

73 Cf. Gechter 1979, see esp. fig. 53.

74 See Haselgrove 1984, fig. 3 and esp. Fitzpatrick 1985.

This is, in one way, not very surprising. As mentioned earlier, and repeatedly noted by most writers on the subject, there was a strong similarity between the tribes on both sides of the Rhine, more differences being found between north and south than between west and east. This state of affairs may indeed go a long way into accounting for the relative lack of indications for change. As Rüger put it: *Die Einwanderung rechtsrheinischer Gruppen hat die spätlatènezeitliche linksrheinische Tradition Ostgalliens ebensowenig geändert wie die faktische Zugehörigkeit zum römischen Reich seit etwa 50 v. Chr.*[75]
Such a statement is undoubtedly too strong in view of the difficulties inherent in providing archaeological evidence for such a short period and also with regard to what has nevertheless been found, let alone what may be discovered by concentrated research efforts in the future. But in many respects it is probably still correct. The burial evidence, for example, does not seem to change in this period. Wealthy graves are only found in the same zone as before, immediately south of the North Belgic area. Also important is the fact that the relative scarcity of archaeological indications for changes in population groups is not really in conflict with historical evidence, because a sizeable part of the original population did survive the direct and indirect effects of the Gallic Wars. The northernmost groups successfully applied tactics of evasion and others, such as the Morini and Nervii, suffered heavily but survived nevertheless. That leaves only the central part of the Eburonian territory between Rhine and Meuse, and partly between Meuse and Scheldt, in which to look for the most profound changes, and at least in the Rhineland there now is or seems to be some positive evidence for this (cf. note 70). In the delta, however, where the Batavians are supposed to have settled, the archaeological data indicate another situation, which can now be evaluated.

10.4 SITUATION IN THE RIVER AREA

From the preceding paragraphs, and also from what has been discussed in earlier chapters, it can be concluded that the Dutch river area was outside the mainstream of events during the entire Late-Iron Age. In the prehistoric situation socio-economic and political conditions were such that its geographical position was of little consequence. By the last century BC, it lay in fact on the extreme periphery of a Belgic core area in northwestern France and was even more peripheral in relation to the central and east Gaulish *civitates*.
As far as the eastern river area is concerned, we cannot be certain about the socio-political identity of its population. As noted above, in Caesar's time the Waal may have been a border between the northernmost group belonging to the Eburones and Germani transrhenani (Tencteri and Usipetes?). This is in accordance with the fact that we know (cf. chapter 3) that the Waal was the most important branch of the Rhine from a hydrographical point of view, but that does not mean much except as an explanation for Caesar's mistaken identification of the Waal as the Rhine. Archaeological support for a Waal border can perhaps be found in that some settlements to its south appear to be slightly different in the sense that they are somewhat 'wealthier' (see 8.2.1). However, if this difference means anything it is more likely to be relevant as an indication for qualitative differences at the micro-regional level. This is all the more so because there are no other differences between the areas north and south of the Waal, in particular not in the pottery. This is typologically quite coherent for the eastern river area as a whole.
Unfortunately, because of a lack of available material it is not so easy to determine if, as a group, it is distinct from the pottery in adjacent regions. Virtually nothing is published about late-Iron Age pottery in the area directly north of the Rhine and the difference in native wares north and south of the river during the Roman Period may not necessarily have been present in pre-Roman times. In general, however, it seems that the Rhine was a major divide and the published information about pottery south of the Meuse indicates that it is at least very similar to river area forms.[76] In contrast, the pottery assemblage from the coastal provinces in the west is different again.[77] The limited range of other artefacts, including the glass bracelets, and also the still limited data on regular house-plans[78] as well as, perhaps, the irregular 'post-hole swarms',[79] point to the same conclusion as indicated by the pottery: similarities are in the first place to

75 Rüger 1968, 14.
76 For a concise overview, see Van den Broeke 1980, esp. 53–4.
77 Little has been published as yet, but an inventory is currently in preparation, see Van Heeringen/Van Trierum 1981 (with further references).
78 Van Es 1982.
79 Above, paragraph 8.2.1.

be found in the south but there are, at least at the moment, no arguments to prove either that the zone directly north of the Rhine was very different or that it should be included in the same group.

It is evident that as yet – if indeed ever – archaeological data do not allow a firm statement as to which tribal unit the population of the river area belonged. But the lack of internal differences and also the specific character of the landscape make it more than likely that the region as such was inhabited by one, socially and economically homogeneous, group which, from a supra-regional perspective, was more affiliated with groups to the south than with those to the north. There is nothing against an interpretation of this group as one of the largely autonomous segments of the Eburones, but positive evidence is lacking.

It is not impossible that this river area group was known as the Batavians. As Sprey has convincingly argued,[80] etymological analysis indicates that *bat/bet* and *aue* are the constituent elements of the name, which would thus mean: (people of) the good/fertile waterland/river island. He suggests that we are dealing with a latinised German name and that the population was called after the region, not the other way around. This interpretation may, in fact, imply that the interpolation in Caesar's report referring to the Batavian island (in this sense thus a pleonasm!) may not really be corrupt and much later. It could be an example of the marginal notes from Caesar himself which are supposed to have existed by some authors and inserted into the text after his death.[81] But neither such an assumption nor the implication that the population in the river area was Germanic-speaking during and before Caesar's time are grounds on which further conclusions can be based.

The migration of a segment of the Chatti as recorded by Tacitus leaves little doubt that this, if not indirectly caused by, was at least made possible by the effect of the Gallic Wars (settling in uninhabited lands). Whether these people became known after an existing Germanic name for the region or after the name they themselves gave it[82] is not really important. Much more relevant is the matter how such a migration and its effect in the river area can be properly understood, and notably in relation to the archaeological evidence.

An important basic consideration in this respect is the degree of continuity or discontinuity visible in the archaeological record, and that is precisely the point where troubles start. In principle it is not very difficult to argue for continuity. As far as settlements are concerned, the evidence in chapters 4, 5, and 8 clearly indicates continuity from the Iron Age into the Roman Period for the region as a whole, from a chronological and also a settlement-typology point of view. Burial evidence is not available, and pollen diagrams show gradual change only in the Roman Period. Finally, the native pottery shows some development from the Late-Iron Age into the Roman Period, but that change is restricted and difficult to detect.[83] Whatever its interpretation may be, it is certainly not a major change in native pot-making traditions.

Unfortunately, it is all too easy to demonstrate that the evidence for continuity is not as firm as it may seem to be. Excavations are few and far apart, and the degree of chronological control in the period before the use of wheel-turned pottery is very minimal indeed. It is perhaps not very probable but nevertheless conceivable that whole scores of settlements were in fact not continuously inhabited and even that on most of these sites the habitation was temporarily interrupted around the mid-1st century BC.

Fortified settlements, which frequently demonstrate such interruptions, are lacking in the region (cf. fig. 119). The pollen diagrams can also be interpreted in another way, because it could be argued that even a total lack of cultivation of the land lasting only a few decades is probably not visible or very difficult to demonstrate in the diagrams. The lack of burials does not allow any firm conclusions in any direction and even the seemingly best evidence for continuity, that of the pottery, is not entirely unequivocal. There is no major change, but that could also mean that the pottery of immigrants was not very different from that of the original population.[84]

On the whole, archaeological data are thus not precise enough to reach definite conclusions, but it is at least not

80 Sprey 1953, 14–5. See also Heidinga 1984, 169.

81 See Hachmann 1976, 121.

82 For a discussion of the problem, see Sprey 1953, 18–9.

83 Cf. chapter 6, p. 178–82 and Hulst 1981.

84 In other areas to the south (cf. note 67) and west, along the coast (Brandt a.o., in press, Van Trierum, pers. comm.), differences between late-Iron Age and Roman Period pottery which are to be expected when large-scale migrations did occur are indeed found (which does not mean that there are no difficulties there, see e.g. Van Heeringen 1984). Hulst (1981) leaves the matter undecided for the river area.

very likely that the river area was largely depopulated and reoccupied by a different group. Even the crude measures discussed above would have given somewhat more evidence in the case of such radical change, but there is more than that. Our historical sources reveal that a group of Chatti moved to the river area. They lived formerly in Northern Hessen, according to the almost universal scholarly opinion, and the historical link between the two regions has recently found archaeological support in the form of Celtic coinage. The so-called silver triquetrum coins of the Lith group, characteristic for the river area, are interpreted as a sequel to the emission of gold triquetrum staters which are only found in Northern Hessen and the Rhineland.[85] Accepting this at the same time confirms both the historical sources and the conclusion that the region was not largely depopulated, because people from Hessen who almost entirely replaced the river area population would surely have left archaeologically detectable changes. It also indicates what the most likely interpretation of the events should be.

From what has been said in the previous parts of this chapter it appears that Caesar's troops were hardly if at all active in the Rhine-Meuse delta. Undoubtedly the tactics of evasion and dispersal instead of confrontation and combat could not prevent destruction and disruption insofar as the troops did operate, but even there the population as such largely survived. Thus, if the people in the river area were actually affected by the war, it may not have done them too much damage in the long run and the area continued to be occupied very much or entirely as before. This agrees with the limited archaeological data we have and indicates that the arrival of a new group in the area can only have concerned a relatively small number of people.

It is not known how large the fraction of the Chatti, which Tacitus calls Batavi, actually was. But in his description of the migration, Tacitus says that 'they occupied the edge of the Gallic bank, which was uninhabited (*vacua cultoribus*) and likewise an island close by, which is washed by the ocean in front but by the Rhine on its rear and sides'.[86] When taken literally, this could imply that the Betuwe, or perhaps the entire river area, was not *vacua cultoribus*, in contrast to at least the area south of the Meuse. The former is probably true, as discussed above, but there are not yet enough data available for the Pleistocene sand area of Brabant to say anything about archaeological changes there. On the other hand, the most direct archaeological clues for Batavians are distributed in an area which more or less agrees with Tacitus' description (see fig. 123). The silver triquetrum coins have mainly been found along the Meuse and at the point where Meuse and Waal are very close together, and then there is the altar from Ruimel dedicated by Flavus, *summus magistratus* of the *Civitas Batavorum* in IA.[87]

If this evidence is relevant, we may conclude that the area where the main body of immigrants settled is in the centre of the river area on both sides of the Meuse and the lower course of its small tributaries in Brabant, on a small stretch of the Pleistocene sand area. This would be in agreement with such descriptions as *extrema Gallicae orae* and *non multum ex ripa*. If the size of the new group was significant enough, future research in this area should provide more archaeological support for this hypothesis.

For the moment, however, all we have are artefacts related to an elite group of which Flavus presumably was a descendant and who issued the silver coins.[88] This group did, however, settle in a strategically important area as far as control of the entire river area is concerned and was apparently able to gain that control rather quickly.[89] It may well have had some sort of central place in the Rossum/Lith area on the northern bank of the Meuse and close to the Waal.[90] This at least presupposes a substantial number of men because, even though the new group was backed by Roman force as suggested earlier (10.3.1) they needed – and, if the interpretation offered above is correct, were intended – to perform the task of securing the river area themselves.

As mentioned in chapter 2, it is possible that the immigrants were primarily some sort of irregular auxiliary

85 Roymans/Van der Sanden 1980.

86 Tacitus, *Hist.* IV, 12 (translation C.H. Moore); also in Tacitus, *Germ.* 29.

87 Cf. chapter 2, 24 and chapter 11.3.1. See fig. 127. For a similar opinion on the implication of the findspot of the altar, see Rüger 1968, 33–4, contra: Bogaers 1972a, 328 and note 75.

88 This does not necessarily imply a private emission by the aristocracy. A tribal emission (by a council or a king) is also possible.

89 This would be even stronger when they were really *the* Batavi and the region was named after them and not the other way around as suggested earlier.

90 Cf. Roymans/Van der Sanden 1980, 213.

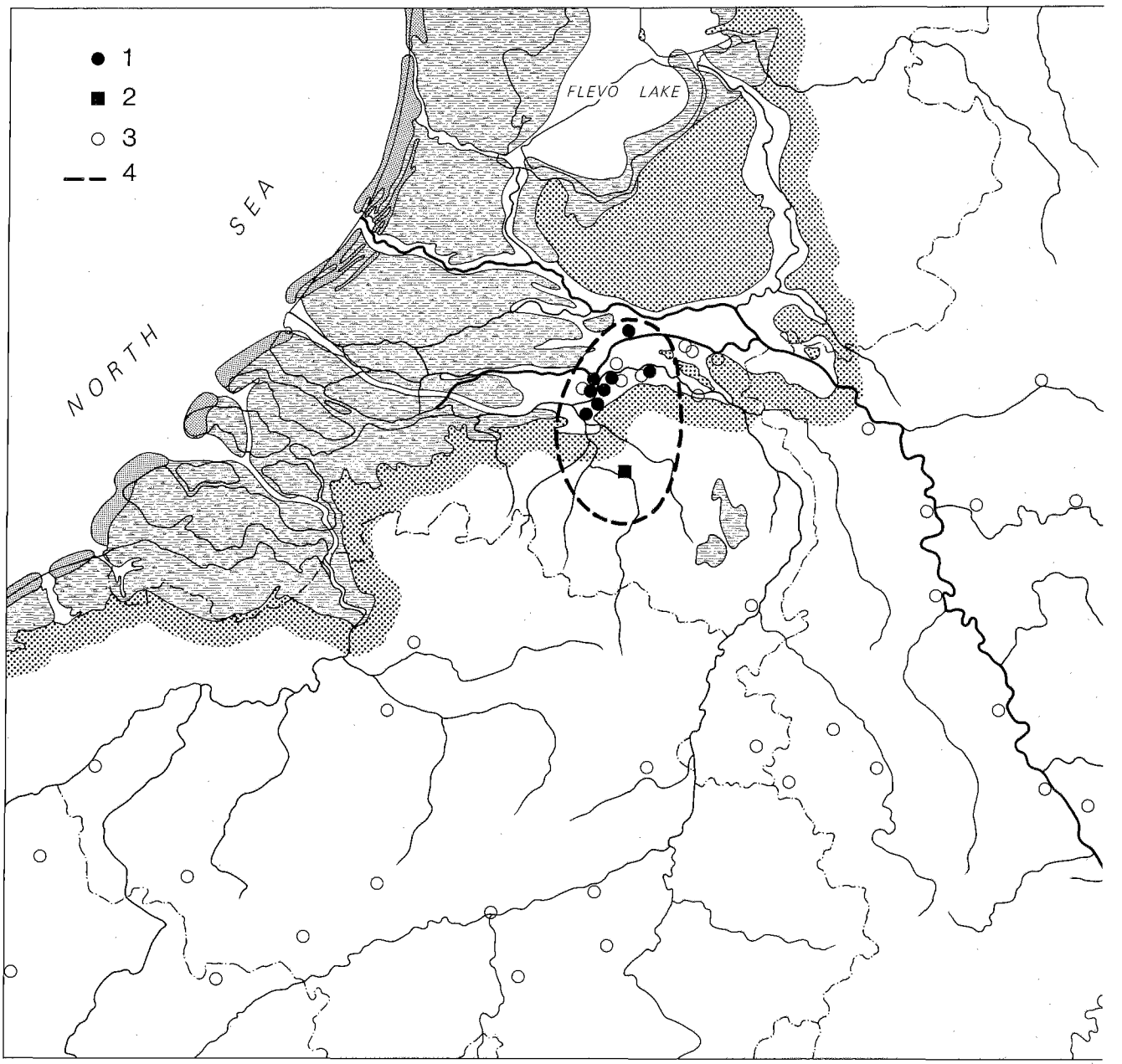


Fig. 123 The distribution of: 1 silver triquetrum coins of the Lith group, 2 findspot of the altar from Ruimel, 3 later Celtic bronze coinage (copper triquetrum coins of the Bochum group, GERMANVS INDVTILLI L, and AVAVCIA coins). The distribution of 1 and 2 may define the earliest area under Batavian control (4), while the distribution of later coins (compare fig. 128!) is determined by the Augustan military campaigns. Coin distributions after Roymans/Van der Sanden 1980. For a legend of the geology, see p. 13, fig. 3 or p. 421, fig. 135.

force and the site near Rossum/Lith was a camp, for which the silver coins could be an indication.[91] This interpretation is also attractive in that it might explain the seeming absence of foreign pottery: if women were the potters and if we are dealing with a largely male group of immigrants they would have been provided fairly quickly with local wares.[92] But the assumption of a largely male group is not very plausible, because the sources clearly imply that a whole segment of the tribe of the Chatti was involved, and we may assume that this encompassed at least a number of lineages but more probably a segment in the sense of *pagus*, one of the fairly autonomous units constituting a tribe.[93]

An assessment of events and social and economic conditions in the river area at the end of the Iron Age, on the basis of what is known from various disciplines, is clearly a matter of much interpretation and little certainty. Nevertheless, enough is known to arrive at a model which should be accurate in major outline and encompass at least a number of elements from which hypotheses testable by future research could be derived. This model – or speculative reconstruction – can be summarized as follows.

10.4.1 Social and Political Organization

Before the Gallic wars the fertile river area, *Bat-au*ƕ*a*, was either inhabited by a homogeneous group of people composed of one or a few tribal segments and probably affiliated with the larger tribal whole known as the Eburones or by a quite distinct group. In either case, the social organization was predominantly egalitarian with little difference between individuals, no permanent leadership, and economically a considerable degree of regional autarky and adaptation to local conditions with relatively little outside trade. The settlement system may have been diversified but it was not hierarchically organized and thus there were no central places.

The Gallic Wars did not substantially disrupt this situation both because sociopolitical conditions in the region or its position from a supra-regional perspective did not trigger much military action and because as far as that action did take place the response of evasion and dispersal prevented irreparable damage.

Although relatively undisturbed, society in the river area must of course have been affected by the wars. Existing relations with the south, whether economic (trade) or more encompassing than that (affiliation with the Eburones) were surely disrupted and considerable destruction and perhaps depopulation took place south of, if not in the south of, the river area. Famine and fugitives are also factors which may have had an effect in the river area although their true impact is impossible to estimate. During the following decades spontaneous and induced migrations as well as autonomous and more or less engineered regrouping took place in northern Gaul. But already at a very early stage, Roman concern for and thus more direct influencing of developments was strongest along the Rhine and, in so far as they were not already in the vicinity, the most dependent allies were manoeuvred into those areas. Conflict among the Chatti, causing tribal fission, led to the migration of the segment of the tribe that for some reason was allied with Rome. These people moved to a distant and for an autonomous move rather unlikely new location in the centre of the Dutch river area, perhaps to those locations where the original population had suffered most from the war and its aftermath. But the opposite, settling as close to a remaining centre of population, if there was any, is also conceivable.

Nothing can be said about a variety of possible differences, for example, in language, between the new and old populations but, the former being a Germanic tribe, we can be quite sure that from a general perspective of social organization there was much similarity. A well-established tribal aristocracy (*stirps regia*) may have been typical for the immigrants, and the minting of silver coins is presumably associated with them. In any case we know with certainty from the historical information that *the* Batavi were once part of the Chatti and that the new group became dominant in the new social formation. This also follows from the inferred Roman support, perhaps a more effective military organization, and from the fact that their apparent primary area of settlement was strategically so well situated.

It is rather futile to speculate on how, or how far, the old and new populations became integrated into one new tribal entity because we know so little about such basic relevant facts as the period of time or relative numbers of people involved. Moreover, there is no need even to as-

91 See paragraph 2.1, p. 23–4. Also Haselgrove 1984, 41–2 and presumably Haselgrove, in press; Nash 1976 or Nash 1978a, e.g. 41.

92 This argument does not, however, exclude the presence of some quantities of imported native wares as is demonstrated, for example, by the Frisian pottery associated with Frisian auxiliaries on Hadrian's Wall (Jobey 1979).

93 Cf. above, 359 and note 19.

sume that such integration should necessarily have taken place before the beginning of the Roman Period. If we are indeed dealing with a segmentary tribal system there is no point in presupposing an immediate coherence maintained from above by sociopolitical institutions, but to expect only a very weak integration at the tribal level. Several groups may initially have fruitfully coexisted under the domination of the new immigrants with only a kind of nominal common identity. At the same time, however, the entire system was subject to change by increasingly effective and direct interference from outside, thus providing opportunities for an elite to manipulate and develop the characteristics of a ruling class.

But there is no way of knowing how far this process actually went because in the first half of the 1st century AD the imposition – not necessarily entirely forceful – of a Roman *civitas* model obscures indigenous developments. The position held by Flavus, for example, that of *summus magistratus*, does not necessarily imply that he was the highest ranking individual at the apex of a well-defined social hierarchy. The function as such may have been more the result of external than internal developments and, although surely reserved for members of the tribal elite, does not presuppose a strongly developed pyramidal social structure.

10.4.2 Economic Organization

As mentioned before, economic life in the river area was fairly autarkic. A few commodities, such as salt and thephrite (basaltic lava) querns, part of the metal artefacts, and other things such as perhaps crude glass, are known to have been imported. These are surely only the archaeologically best traceable items in a wider range of commodities including perishable materials, so that regional autarky should not be taken to imply a closed economic system. There was external trade with surrounding regions and the distribution of late-La Tène glass bracelets (in particular of Haevernick type 3b) may eventually even provide us with an instrument to measure the degree and direction of such trade.

Despite all this, the overall picture is consistent with that of the social organization in that there are no centres for tribute flows where an elite group could be postulated. Luxury imports or indications that specialists were supported by such an elite are also lacking. Perhaps the closest to being specialists are the presumed glass bracelet manufacturers in the region between Waal and Meuse but there is no need to link these to a centralized or hierarchically organized economic system. What craft production there was must have worked on a small scale at the micro-regional or settlement level and thus with few or no full-time specialists. The latter can, however, only be assumed because there are whole areas of economic activity of which as yet little or nothing is known, such as crafts dealing in perishable materials or in the 'immaterial' (religious specialists).

From the indications – barely deserving to be called evidence – that we have, it seems that this situation was only slightly altered in the final decades of the Iron Age. There are no indications that the group of Chatti which settled in the river area drastically changed economic life. But they did come to dominate the region and perhaps even established some sort of central place. This could have been associated with some tribute flow and accumulation of wealth. A connection with the minting of silver coins and with at least one other form of specialization, namely, in military affairs, is compatible with that. Whatever forms and degrees of contact with the Roman state existed at that time, their impact can hardly have led to fundamental changes. In contrast to Caesar's actions in the northernmost Belgic area, everything we know about subsequent Roman policy along the Rhine is aimed at stabilization, not further disruption. This may perhaps also be deduced from the fact that the possibly fairly intricate system of agricultural production in the river area as developed during the Iron Age seems to have survived undisturbed well into the Roman Period.

The nature of this system has already been touched upon in chapter 8, in the discussion of native settlements.[94] Unfortunately, at the moment an analysis of the organization of agricultural production has still to proceed along indirect lines. Notably studies on faunal material and plant remains from river area sites are not yet available.[95] On the other hand, we do have, apart from the excavation data, a detailed insight into the geological conditions which provides a general framework of agricultural possibilities and impossibilities as outlined above (chapter 7.2.2). In addition, there are a consider-

94 See above, esp. p. 224–8 and 284–5.

95 This situation will be remedied in the foreseeable future only for the animal remains from a number of sites when an archaeozoological study by R. Lauwerier (see chapter 1, note 8) has been completed.

able number of pollen diagrams which yield information about the natural vegetation and human influence on it for the region in general.

All through the Iron Age, these diagrams[96] indicate for the Holocene river deposits an increasing spread of non-tree pollen, including *Cerealia* and, most notably towards the end of the period, a strong or even very strong increase of *Compositae* and *Cyperaceae*, as well as high values for *Plantago*. The evidence thus shows rapid and large-scale deforestation, formerly wooded areas (normally the riverine alder-woods: *Alno-Padion*) being transformed into arable but especially into grazing land. On the Pleistocene deposits and notably on the ice-pushed ridges, the oak- and birch-woods (*Quercetea robori-petraeae*) appear to have been only very slightly disturbed during this period.[97] For the coversand area, there are more indications for deforestation in the Iron Age but on a far smaller scale than in the river area proper, with limited clearances for arable and some increase of heath.[98]

In general, we may conclude from this that the high-lying and rather unfertile Pleistocene deposits were relatively sparsely settled and the Holocene river deposits, on the contrary, rather densely populated, with the Pleistocene coversands somewhere in between. The archaeological distribution map of Late-Iron Age settlements (fig. 60) is in complete accordance with this conclusion.[99] The largely uninhabited oak- and birch-woods were probably marginally exploited for fuel and timber, demonstrably for hunting, and perhaps for other purposes.

In contrast, a more complex system developed in the other two regions. For the Pleistocene coversand areas, the palynological data and the archaeological information about settlements and house-types, especially the characteristic two-aisled 'Haps' houses, are compatible with mixed farming. If it is assumed that all cattle were stabled in winter, which is not necessarily true, the modest length of the houses (in general around 15 m) implies that the number of cattle cannot have been very large but there may have been sheep as well, which were probably not stabled in winter.[100] The limited clearances in the forest on the coversands were primarily used to grow barley and wheat while the lower lying areas, with still fairly extensive alder-woods, were to some extent used to graze cattle.[101]

In contrast, the palynological data mentioned above show massive human interference in the natural vegetation of the Holocene river-clay region, with ample evidence for arable but much more for grazing land. The large number of settlements from the Late-Iron Age fits into this picture, but the available house-plans do not. Long houses with living-quarters and a large byre under one roof, which might theoretically be expected, are not at all common. In chapter 8,[102] evidence was presented to support the conclusion that the so-called 'post-hole swarm' type of settlement could be explained by a specialized subsistence-strategy, either arable farming or stock-breeding. This in contrast to houses with a byre which were taken to indicate in most cases the more common practice of mixed farming which is – leaving aside details concerning chronological development of and supra-regional differences in the construction – precisely why they are found almost universally in north-western Europe: mixed farming is not only a balanced and reliable subsistence-strategy, it is often the only one possible.

The very simple and small houses in the post-hole clusters are taken to be connected to economic specialization. In the Rhineland loess area, specialization in arable farming has been demonstrated and in the Dutch river area it is also possible on the fertile stream-ridges.[103] In view of their occurrence together with settlements consisting of 'normal' houses with a byre, and especially in view of the palynological information, it is suggested, however, that for most settlements a specialization in

96 Information based on published (e.g. Teunissen 1975, 1980b, 1982, 1984) and several unpublished diagrams kindly provided by Dr Teunissen.

97 Teunissen/Teunissen-Van Oorschot 1980.

98 Janssen 1972, esp. 426–31 and Teunissen, in Verwers 1972, 149–55 (*Anhang* 1).

99 Note that a number of sites south of the Meuse is not indicated on Appendix 1, cf. chapter 8, note 1.

100 See on these subjects IJzereef 1981, 177.

101 See note 98.

102 See note 94.

103 Especially relevant are the excavations in Eschweiler-Laurenzberg and Eschweiler-Lohn (Joachim 1980; Knörzer 1980). Similar sites are Weeze-Baal (Müller-Wille 1966) and Niederzier-Hambach (Simons 1983). Other comparable sites are the fortified settlement Hambach 382 (Göbel/Joachim 1980) and Grevenbroich-Gustorf (Reichmann 1979a), the latter especially because it shows that houses with a byre are not at all absent in the Rhineland.

stock-breeding is the more likely interpretation. This conclusion is now also supported by the archaeozoological analysis of river area sites by R. Lauwerier.[104] It has shown that the one 'post-hole swarm' site included, that of Heteren-Het Lage Land (no. 93), is the only one different from other settlement sites in that the percentage of bones from young cattle is much higher than elsewhere. This would indeed seem to indicate a strong emphasis on stock-breeding, although in view of the lack of stables of a rather extensive kind.

We may assume that such specialization was made possible by the environment, notably the presence of the flood-basins which provided suitable grazing land in the dry season. But the environment is not all that counts. Socio-economic conditions in the region and beyond must also have been such that they allowed a fairly complex trade network because a specialized economy necessarily implies at least to some extent a surplus of one group of commodities and a shortage of another. Moreover, this network must have been reliable over a longer period otherwise the in itself somewhat risky strategy of specialization would never have rooted so deeply and have existed for such a long time from the Middle-Iron Age until the mid-2nd century AD.

On the other hand, the network as such may have been fairly complex but it was probably not very extensive as is shown by the lack of much imports from far afield. This agrees well with the absence of indications for more developed forms of social organization and we can assume, therefore, that the necessary trade was a function of the fairly egalitarian tribal society and not managed by a central authority.

In this context, it is perhaps useful to mention briefly in some respects similar sites from the northern Netherlands, namely, the late-Iron Age walled enclosures discussed by Waterbolk.[105] They were in use between c. 350 BC and c. AD 100,[106] which is roughly comparable to the known period of our post-hole swarms and they lack 'proper' houses but still have normal domestic refuse and a number of granaries and structures interprete as barns. The interpretation is that the enclosures serve to stock cattle and harvest products, and perhaps had a additional religious function. The interesting point i that these sites in any case diverged from the norm an had a special purpose,[107] and that this also resulted i habitation without normal houses. In this sense, they ar a parallel phenomenon, but such an observation does no imply that the enclosures, which are relatively rare, ar comparable to the river area post-hole swarms in all re spects. Their fort-like appearance and relatively empt internal surface suggests indeed a different function which could be that of central storage or stocking place To interpret these sites as territorial centres controlle by a fairly powerful elite, and to make a functional com parison with hillforts is, however, perhaps somewha exaggerated.[108] They may indicate some first develop ments towards such sites,[109] but even their fort-like ap pearance is in principle perfectly compatible with function for communal purposes in a still strongly egali tarian society.

To return to the eastern river area, the conclusion mus be that when Drusus' troops arrived there in 12 BC, the found the islands between the rivers rather densely po pulated, with an economy primarily geared towards ex tensive stock-breeding, surrounded by largely uninha bited wooded hills and an only partially exploite Pleistocene sand landscape with scattered agricultura settlements. The population of this area still formed fairly egalitarian tribal society known as the Batavian after the area they inhabited and had fairly recentl come to be dominated by a group of Chattian immi grants. These may have had a centre (or military camp at a strategic spot in the middle of the delta to which the were manoeuvered, if not actually sent, by Roman au thorities with whom they were allied, probably eve then under a treaty stipulating freedom from taxes i return for military assistance.

104 See chapter 1, note 8. I am grateful to R. Lauwerier for discussing with me his provisional results and for his permission to incorporate the information here.

105 Waterbolk 1977. The best examples are Zeijen I,2, Vries 2, Zeijen I,3, Zeijen II,2, and Vries 3. Whether the 'enclosures' from Rhee should be included is somewhat doubtful (cf. *op. cit.*, 169).

106 Waterbolk, *op. cit.* 162; the author prefers, however, narrower dating of c. 200 BC – AD 50.

107 Which is becoming increasingly clear as more contempo rary sites become available: compare Waterbolk 1982.

108 Waterbolk 1977, 168–70.

109 Cf. Groenman-van Waateringe 1980, 1040.

11 ANNEXATION, GROWTH, AND BREAKDOWN. THE RIVER AREA FROM 12 BC–AD 270

11.1 INTRODUCTION

With the arrival of Drusus' troops in the river area a period of nearly three centuries started in which the inhabitants were incorporated into a much larger social and economic organization than before, that of the Roman empire. In this chapter, some of the processes leading to and determining this and following events will be discussed, as well as their effect in the region and the specific consequences as revealed by the archaeologically and historically documented developments. As outlined in chapter 2, these developments should be divided by the analytically highly relevant caesura constituted by the establishment of a formal frontier along the Rhine in the mid-1st century AD. This event had more than a direct political and military significance. As a result, the entire Rhine-Meuse delta became a frontier region which strongly influenced the nature and intensity of social and economic processes in the following two centuries.

Some of the theoretical concepts involved are discussed in paragraph 11.2, together with a discussion of their relevance to Roman expansion.[1] Their relevance to the Dutch river area is examined in the following paragraphs, but it can also be briefly epitomised by three historically known Batavian leaders. The first is Chariovalda, only mentioned in passing by Tacitus and virtually unknown as a person.[2] He exemplifies the chieftain of a client tribe which, as a separate society with its own norms and values, could exist in profitable symbiosis with the Roman state. The second is Julius Civilis, a figure representing the period of stress when tribal society was transformed to fit the changed needs of the empire and also the acceleration of this process caused by native reaction.

The third is Postumus,[3] who typifies the militarized Roman population of the frontier zone which resulted from the developments started in the 1st century. At a time when the peripheral frontier zone, although first to be affected by trouble from beyond the border, had developed into a socio-economic centre, his actions illustrate the magnitude of this change. His Gallic empire, centred on the former periphery, is an attempt to build a new state. And the difference between people trying to maintain a state-level of social organization and those staging a native revolt against their incorporation by the Roman state is considerable.

But the developments of the second half of the 3rd century on the whole took a course leading to disintegration, and the military action and movement of population groups into the frontier zone – itself at least partly caused by previous influence (transfer of information) from or through that zone – provide a second major caesura for our region. It was perhaps, but not necessarily, a more gradual change, but neither history nor archaeology allow us to monitor developments very closely in this period. The different situation in the 4th century and its further consequences will be examined in chapter 12.

11.2 IMPERIALISM AND COLONIALISM

11.2.1 Theoretical Considerations

In recent years, a number of important theoretical studies in history, sociology, and economics have given a new impetus to the use of diffusionist models in archaeology.[4] The explanatory potential of such models had been somewhat discredited as a result of the culture concept in which they were used,[5] although they remained a basic assumption in much work and notably in Roman archaeology were Romanization is a concept of central importance. The interest in the results of work in other social sciences, which itself springs from the need to gain a better understanding of our western colonial past and its present-day consequences,[6] has already provided many new stimuli to the study of late-Iron Age and Roman Period Europe.[7]

1 See also Willems 1983a.

2 Tacitus, *Ann.* II, 11.

3 On the inferred Batavian nationality of Postumus, see below, p. 409. The argument as such is, of course, not dependent upon the historical accuracy of this inference.

4 These are, among others, the works of Wallerstein (1974, 1980), Galtung (1971), contributions in Louis (ed.) 1976, and Ekholm/Friedman (1980).

5 Cf., e.g., Van der Leeuw 1974, 175–6, 178–80. Obviously, when independent developments can be shown, normative archaeological theory has problems with diffusion.

6 Cf. Bloemers 1983a.

7 Examples of the fast-growing literature are the works of Nash and Crumley (cf. chapter 10, note 1), Bartel (1980), Hingley (1982), and the proceedings of several recent conferences such as Green/Haselgrove/Spriggs 1978, Burnham/Johnson 1979, Brandt/Slofstra 1983, or Blagg/King 1984.

This does not mean that there is some sort of generally accepted theory on imperialism, colonialism, and related phenomena. On the contrary, there are many different opinions and interpretations and sometimes there is not even agreement on the precise meaning of theoretical concepts. Therefore, it is useful to examine some of these concepts and thereby to formulate some ideas about the more general processes behind the specific developments within the chronological and spatial coordinates delimiting the present study.

Imperialism and colonialism are dominance/dependency relations between one society and another. The concepts are sometimes used interchangeably, which to some degree they are, but imperialism is normally considered the more general term for the situation in which one society or state controls another and in fact the cause of colonialism, although they are, at the same time, different forms of domination which may coexist.[8] The difference lies mainly in the degree or rather character of this control which, in the case of colonialism, is inevitably direct and involves the movement of people.[9]

There is much controversy about the causes of imperialism. In Marxist theory it is seen as the consequence of capitalism while others also view it as a kind of inevitable development resulting from many incidental circumstances or, conversely, as a deliberate choice. Important as it may be,[10] such a discussion is not directly relevant here. In all cases, the prerequisite for domination is an inequality in the resources of the social systems involved, including their own internal structure. This difference may be expressed in forms of coercion. It is perhaps useful to look for general laws in domination processes, but it is more likely that the specific historical context and the societies – or even certain individuals – involved will have determined the precise causes in each case. All do, however, have at least one other common denominator in addition to coercion, and that is that at least at the point where imperialism turns into colonialism, there must be an interest at stake. The nature of this interest, and whether it simply arose or was deliberately pursued, is more or less irrelevant: it may be economical political, ideological, all such motives together, or some thing else. From an analytical ('etic') point of view i may even be completely unintelligible, it just has to b important enough. It is easy to see why this should be sc for colonising has its price: it means the expenditure o energy.

This becomes especially clear when we look at the rela tion between imperialism and colonialism. Imperialism when not defined as an umbrella concept but the ante cedent stage of, or the alternative for, colonialism, als implies coercion. But it is a form of domination whicl consumes much smaller amounts of energy. It does no involve moving significant numbers of people but it i primarily based on the transfer of information. The dis tinction between 'power' and 'force' made by Luttwal will serve to clarify this point.[11]

In his analysis, force is essentially a physical phenome non which is consumed in application and wanes ove distance: '...military force is indeed governed by con straints on accumulation, use, transmission, and disper sion akin to the physical laws that condition mechanica force'.[12] Power, on the other hand, works very different ly: 'It works not by causing effects directly, but by elicit ing responses – if all goes well, the desired responses' When you use force to obtain obedience, you use energy but when you are obeyed because of your power, it is th object of your power who is the actor and therefore th one who uses energy. The power itself is not a physica phenomenon, so it is not consumed by this action. It re mains the same, just as – in principle – it does not dimin ish over distance. But power does not exist unless it i perceived: it is not something at the level of matter or en ergy, but on that of information.[13]

Of course there is a relation between the two, in that ul timately power is based on force and on its perceptio and 'correct' evaluation, which is obedience. Power doe not work when the means of perception are lacking c when the evaluation is 'wrong' or, in other words, whe prior information about force has been unintelligible c

8 See esp. Horvath 1972 and Fieldhouse 1981.

9 Fieldhouse, *op. cit.* distinguishes further between the process whereby a whole population moves to the new territory (colonization in its classical Greek sense) and that involving only an elite to manage the dependent society (colonialism).

10 See, e.g., North's (1981, 8–9) appeal for more investigation into the underlying expansion-bearing structures in Roman republican society as opposed to debating the issue of agressive v. defensive Roman imperialism by ancient histori ans.

11 Luttwak 1976, 195–200.

12 Luttwak, *op. cit.*, 196–7.

13 For a more encompassing analysis of power, including it manifestation at the individual level, and based on much th same fundamental considerations as discussed here, see Adam 1977.

insufficient. Unless, of course, an error was made or a risk taken. In all these cases force has to be used directly, but it is evident that a careful power-policy requires much less input of energy than the use of force, where the output is proportional to the input.

Luttwak uses this distinction between power and force in his admirable analysis of Roman strategies of imperial security, but it is equally applicable in a distinction between imperialism and colonialism. Imperialism is a patron-client relationship, based on a flow of information which allows a low-cost domination because of the client's perception of the patron's power. Colonialism, on the other hand, is characterized by the use of direct force. It is not just a flow of information but a flow of energy which is decisive in this case. This makes colonialism a relatively expensive enterprise and that is why there has to be some sort of ('emic') necessity or interest at stake for the colonizer. Another consequence of the use of energy is that, in contrast to information, it is finite. Depending on the capacity of the system and the general conditions under which the process takes place, somewhere a limit is reached, which brings us to the concept of the frontier.

It is not helpful to discuss this in a very general way, because there are evidently some major differences between imperialism and colonialism before and after the 15th century when they became intercontinental in scope. When thus limited to ancient empires, or archaic civilizations in the sense as described by Service,[14] it is possible to apply the ideas concerning the structure of frontiers developed by Lattimore.[15] In an expanding empire, the frontier is dynamic. There is sufficient energy left to control or incorporate new units. In Lattimore's terminology, this would be a 'frontier of inclusion'. In the colonial situation, if a point is reached where an energy-surplus is no longer available, a static frontier develops, the 'frontier of exclusion'.

The reasons for a frontier to become fixed are, of course, more complicated than this and may be found in the natural as much as in the social environment, but in the end energy is the main factor. Even the very important role of innovation is, in principle, only of temporary importance as is shown by fig. 124.

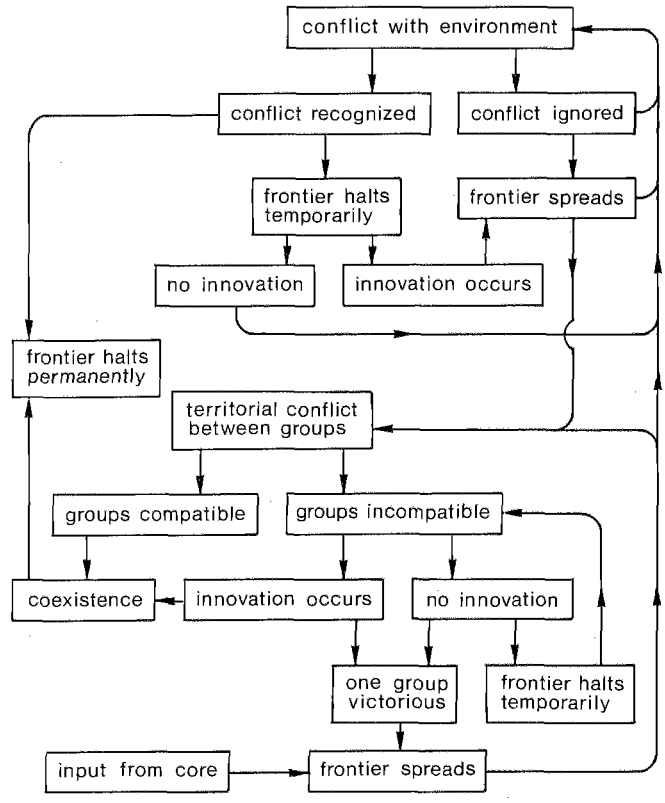


Fig. 124 Model of frontier dynamics. Adapted and interlinked diagrams after Hudson 1977, figs. 7–8, illustrating the role of environmental and group conflict as a stimulus to frontier innovation.

When we look at the areas constituting an empire instead of at these general processes, it is clear that the same energy argument causes differences from the core region to the periphery. For the empire as a whole, several zones can be distinguised as proposed by Lattimore.[16] These are determined by the geographical range of:

1 Unification by military action

14 Service 1962 [1971], 166–9. See also Thapar 1981, esp. 409–13. It is perhaps useful to stress that the analytical concept of colonialism as employed here should not be understood in its 19th- and early-20th century meaning, which has many connotations that are anachronistic where archaic civilizations are concerned.

15 Lattimore 1962. They were developed on the basis of his studies on Chinese frontiers. Although sometimes disputed when applied to a particular case (see e.g. R. McC. Adam's paper and the comments on it in Moore 1974, 1–20), they fit well into modern theories of comparative frontier studies (see esp. Miller/Steffen (eds.) 1977).

16 *Op. cit.*, 480. See also Groenman-van Waateringe 1980 and Willems 1983a.

2 Centralization under uniform civil administration
3 Economic integration

The radius of military action is greater than that of civil administration. Following Lattimore's ideas there is, therefore, an inner zone reaching into territories that can, after conquest, be added to the state and an outer zone into areas that can be invaded for profit or for the purpose of breaking up barbarian concentrations dangerous to the state, but that cannot be permanently annexed. Economic integration has the shortest range, because it is a function of the ability to transport bulk goods at a profit. These analytical zones presumably have some universal validity. They are the result of what, at a higher level of abstraction and remaining at the level of energy and information, can be called a difference in potential between two points, *in casu*, a core and a periphery.[17] Across the potential, flows may exist to varying degrees, depending on the amount of 'friction', defined as the variables which counteract flows, such as technological difficulties. The result at any point in time may be spatially defined and interpreted as in Lattimore's scheme.

This is useful, for example, in examining the degree and direction of energy and information processing and the nature of friction.[18] But as far as imperialism and colonialism are concerned, such a scheme represents only the static side of the coin. When the individual areas within an expanding empire are considered, a dynamic picture emerges, which also shows that both imperialism and colonialism are essentially temporary phenomena. An area at first under imperialist domination by a neighbouring group may then be formally colonized and finally fully integrated although, depending on the constraints posed by the social and natural environment and the capacity to overcome them, this development may halt at any stage.

For the periphery of an empire, this means that a frontier is created along a perimeter defined negatively by those areas that, for whatever reason, cannot be colonized, or positively by the maximum territory brought under direct administrative control (which may lead to a very different outcome). Such a formal frontier is, of course, only one of many boundaries on a transect of control from a core area outwards that could be analytically relevant according to the subject of study. In addition, the kind of formal frontier and the way in which it functions will vary widely depending on the relevant circumstances in different cases. The above-mentioned literature contains various approaches to these problems. Instead of elaborating further what is discussed there it is more appropriate to examine next the processes underlying or leading to developments in the Dutch river area during the Roman Period in the light of what has been said so far. After all, what theoretical issues are relevant or can be fruitfully examined in a particular case is also determined by its specific context and there are particularistic elements which no general theory can fully cover, such as natural catastrophies or the highly significant role of one individual.

11.2.2 Roman Expansion

There is so much to say, and so much has been said, about Roman imperialism that only a few aspects directly relevant to the present study can be mentioned. These do not include the early development of the Roman state, but rather the influence of that state on surrounding areas leading to its great expansion from the 2nd century BC onwards.

A number of recent studies have provided considerable support for the idea that with the growth of the Roman state the intensification of trade contacts with societies on its northern periphery caused changes there.[19] A process such as described by Nash for central Gaul, with increasing social stratification and wealth concentrated in the hands of successful elites, followed by a need to consolidate this position and thus to create an efficient administrative structure, may indeed have led to a process of secondary state formation there. According to Nash, the role of mercenaries and later the slave trade was of crucial importance. However this may be, the archaeological and historical evidence show that Celtic society was permanently changed from weakly organized tribal groups into centralized and hierarchical polities. Undoubtedly this development was strongly influenced by information about conditions in the Mediterranean, but information flows both ways, and the knowledge of and contacts with the Gaulish area allowed not only imperialistic domination but led to actual annexation of

17 The conceptual framework is derived from that discussed extensively by Van der Leeuw 1983.

18 An example is Hedeager's (1978) study on Roman-Germanic exchange.

19 See Nash 1978b, Bloemers 1983a, Haselgrove 1984, to name only a few.

territory at an early stage: the creation of the republican province of Transalpine Gaul (later Gallia Narbonensis) between 125 and 121 BC.

More than sixty years elapsed before the next phase of conquest and a change from imperialism to colonialism took place in a northerly direction. This was Caesar's Gallic war, and its designation as Caesar's war is not at all inappropriate. It can be argued that the changes in Celtic society in Central Gaul, brought about by contacts with Rome, had effectively prepared the way for this conquest. But that does not make such a major enterprise the logical next step. After all, client states under imperialist control were still of major importance elsewhere more than a century later. Perhaps it is true that Caesar's personality was the decisive factor. But even then, the conquest as such can still be seen as yet another consequence of peripheral imperialism.

As was recently argued by Bloemers, the sociological concept of peripheral imperialism is eminently suitable for understanding the process of Roman expansion.[20] The idea is that apart from, or even instead of, a deliberate policy formulated by the rulers in the core area, expansion also arises from the activities of their representatives in the periphery, who deal directly with local elites. Support of one party in a conflict, personal ambition, and the like, may then trigger a series of events starting with some actual interference and ending in a true conquest. Roman history has recorded many such situations.

For Gaul, and especially for its northern parts, Caesar's untimely death may have prevented completion of the colonialist incorporation. But, as discussed in chapter 10, it did proceed far enough to allow effective manipulation of events in those northern regions. The limited available data show, however, that this manipulation could not rely simply on a power-policy and that direct interference, the use of (military) force, was necessary. The reasons for this are presumably related to what was said in the previous paragraph about the nature of power, which rests on the perception and correct evaluation of it by the dominated party. The demonstration of Roman force in Caesar's campaigns should have been quite sufficient for that, but at the very least it was apparently not entirely effective. One might attribute this to a case of incorrect evaluation or initially still some lack of information, especially on the part of the transrhenish German tribes, but there may be more to it than this. It can also be argued that, despite all demonstrations, the power of Rome was *not* perceived although it was surely seen.

The reason for this can be found in the social organization of the German and North Belgic tribes. In a centrally organized hierarchical society it is a paramount chief or the decision-making upper stratum of society who do the relevant perceiving. This institution, in organizational terms a high-level regulator, allows both the perception and – at least in principle – the correct evaluation of power, which is why the institute of client kingdoms was fairly effective in the East.[21] In a tribal organization, however, such a central information processing and control unit is lacking and it can be argued that, for a tribal society as a whole, there are no efficient direct means of perception. In terms of systems theory,[22] it can be said that there is not enough channel capacity to process the information input because the cultural system as a whole lacks a high-level regulator who can act with speed and flexibility. In practical terms, any petty chieftain can take a chance and there is no central authority to hold him back.

One obvious response of a cultural system in its effort to cope with changed circumstances more effectively is organizational change, and that is one way to view the process of secondary state formation in Central Gaul in the last century BC and also the developments in northern Gaul during and after the Gallic war. But there is another side to this, namely, that of the larger organizational structure controlling new units, in our case the Roman empire. For effective control, a multi-level (hierarchical) regulatory system is needed. Given a certain amount of time, as in slow imperialist expansion, this may develop gradually as a response of dominated societies around the perimeter. But in the case of rapid colonialist expansion by conquest, new units cannot be controlled permanently, let alone be incorporated, unless their structure is somehow altered more directly.

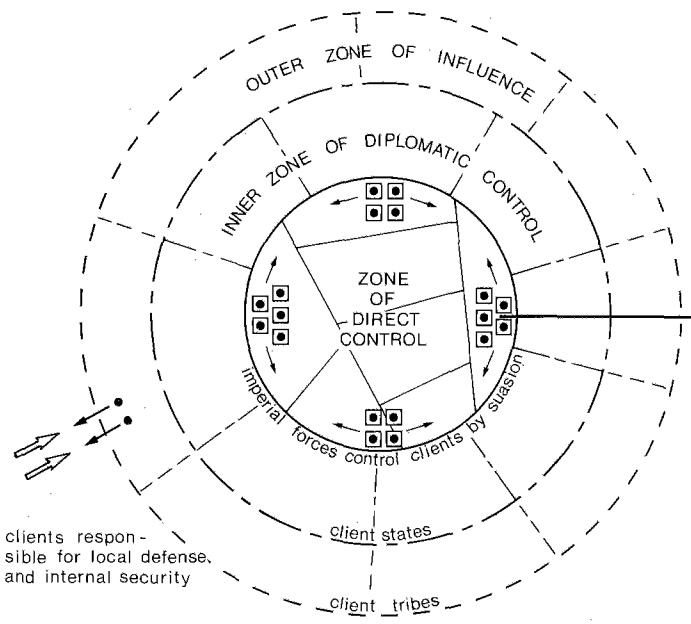
Roman interference in the North Belgic area between Caesar and Drusus can also be seen in this light, but other explicit examples are recorded in the classical literature. For example, the Frisians, in Tacitus words, 'had imposed on them a senate, a magistracy, and laws' by

20 Bloemers 1983a. On 'Caesar's war', see also Drinkwater 1983, 14–7.

21 Cf. Luttwak 1976, 30–1.

22 See Peebles/Kus 1977, esp. the paragraph on 'organization, energy, information, and ritual', 427–31.

HEGEMONIC EMPIRE



clients responsible for local defense, and internal security

provincial territory

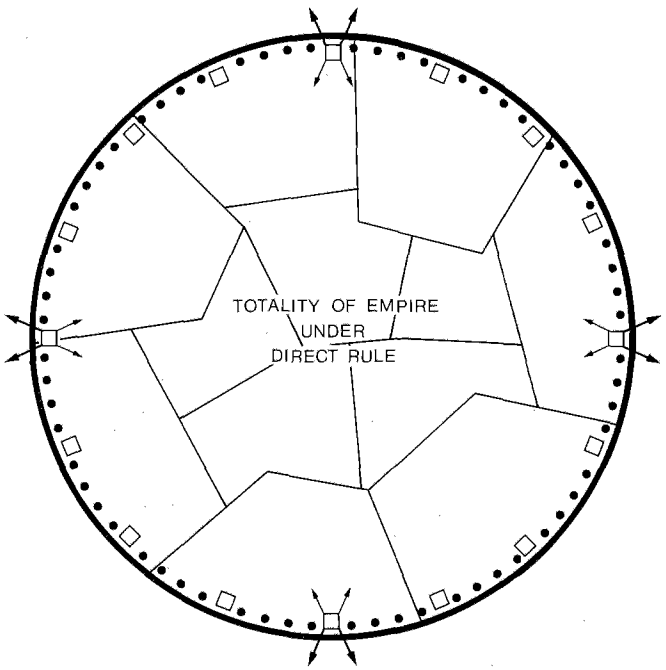
client state boundaries

client tribe boundaries

combined task forces of legion and auxilia

disposable and concentrated imperial forces available for wars of conquest and intimidation of clients

TERRITORIAL EMPIRE



guarded or fortified frontier perimeter

provincial boundary

legions and auxiliary units distributed for frontier defense

imperial forces directly responsible for perimeter defense and internal security.

all clients annexed or abandoned. all imperial forces deployed for frontier deterrence and defense *no "disposable" forces for offense* or defense.

◁ Fig. 125 Two models of empire. After Luttwak 1976, fig. 1.2.

Corbulo, precisely because they needed to be controlled.[23] The Roman administration even had specialized officials for this task, namely, the military *praefecti civitates*, whose main purpose was to establish local self-government and the training of local leaders in its methods.[24] The same Frisians had had such an official since their treaty with Drusus until AD 28, when an incompetent office-holder by the name of Olennius caused a successful rebellion and was forced to flee.[25]

All this is in accordance with the long-established view that Roman control over new territories acted through their own pre-existing or newly created administrative structure. But there are further consequences. As Haselgrove recently put it as a central premise of his paper, 'Rome ... looked to adopt intact whatever of the existing structure she could, and to alter or abolish only those features which ran counter to her long-term interests. The nature of indigenous society was therefore a key factor in Roman expansion, and also in the occasional checks she experienced, as in Germany.'[26] The last conclusion is very much in accordance with what has been said earlier. A major underlying reason for the ultimate failure to conquer Germany can be seen as the inability of German societies properly to perceive Roman power. Their structure made them the subject of much direct use of military force but on the other hand it prevented that force from having its intended effect. After all, the ability of the empire to apply such force was limited, certainly in view of the magnitude of the task, if not with respect to the capacity for staging campaign after campaign then at least as far as imposed changes of the indigenous social structure were concerned.

From a historical or economic point of view there is more to say about all this, but basically it is considered here the main reason why expansion into Germany stopped and a frontier was established. One may, for example, point to political difficulties or to the 'environmental conflict' which arose from the difficulties to find enough food locally to feed the army (see note 140 below). But that situation was not due to a harsh natural environment but to its limited exploitation and thus again to the socio-economic structure of the groups living there. Outside the established frontier, in what Lattimore would call the outer zone of military control, force, diplomacy, and other, commercial, contacts were a constant factor.[27] But only inside was there centralization under uniform civil administration. It is in this area that processes of (further) acculturation and integration into the socio-economic system of the empire are triggered.

However, during the 1st century AD, a transition can be observed from what Luttwak describes as a hegemonic empire (with 'frontiers of inclusion'), towards a territorial empire, with most of the military apparatus deployed along its 'frontier of exclusion' (fig. 125). This introduces a vital difference not only between areas within or outside the designated perimeter, but also between those directly at the frontier and the areas more to the interior of the empire. These are not merely closer to the core area and thus more easily incorporated into Lattimore's inner zone of economic integration. There is another difference as well, namely, that after the conquest troops move on and the regions concerned can develop further within the new context. At the frontiers, once established, the situation is different because there troops are stationed permanently, thus perpetuating aspects of the otherwise transitional colonial situation.

When almost the total instrument of imperial force is located in the frontier regions, it is clear that these have a disproportionately large infrastructure imposed on them. At the northwestern frontier of the Roman empire it is this factor in particular which may initially have prevented and later led to a very different kind of integration.[28] This is a subject which can be examined further with data from the eastern river area,[29] but it is important to see what the reasons for this assumption are.

These have to do with the way in which the socio-eco-

23 Tacitus, *Ann.* XI, 19.

24 See Bogaers 1960–61, 268 and note 21 with further references.

25 Tacitus, *Ann.* IV, 72. See also chapter 8, 263, and note 149.

26 Haselgrove 1984, 6.

27 Which did result eventually in permanent changes there. In principle, this is also a process which can be seen as one of the necessary conditions for the developments in the 3rd century and later.

28 Distance from the core, when measured by the cost of transport, is less relevant on a river frontier connected by excellent waterways to the hinterland, and environmental problems are virtually irrelevant here.

29 See below, paragraph 11.4.2.

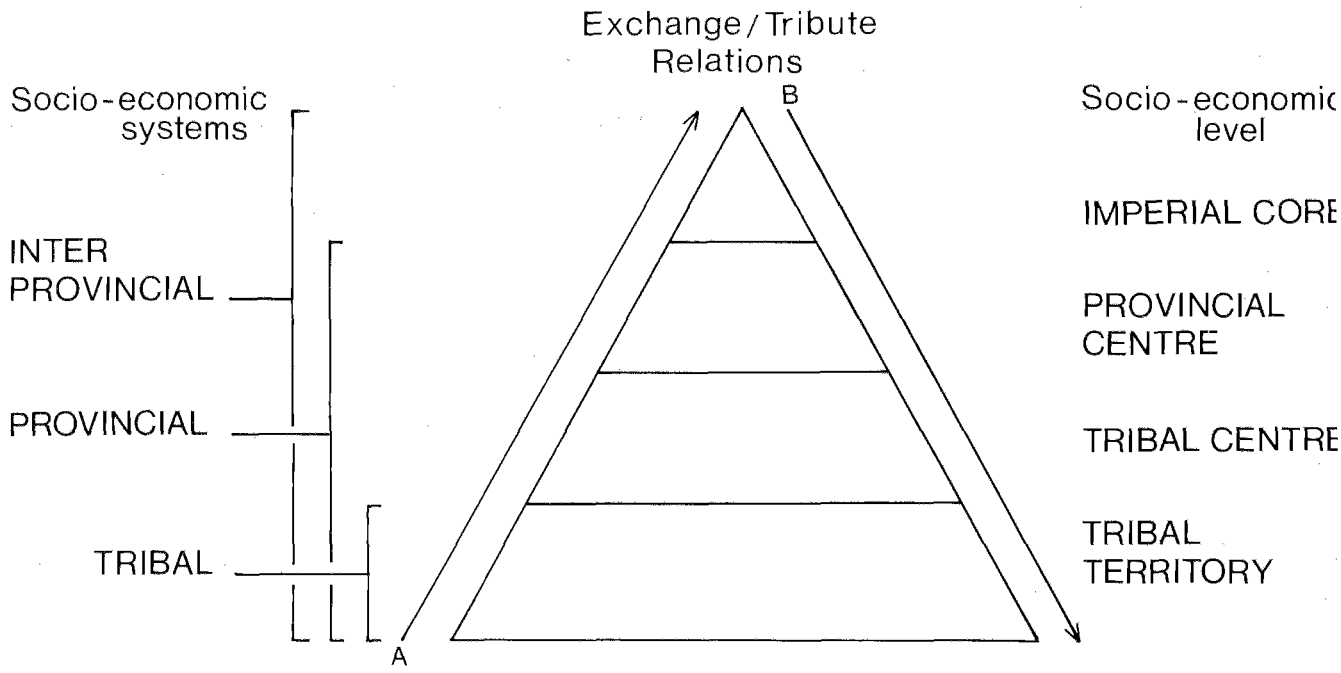


Fig. 126 Model of the exploitative empire system, after Hingley 1982, fig. 1. A = the distribution of raw/basic goods and of taxation, B = the distribution of manufactured and luxury goods.

nomic empire system functioned. As a basic principle, this can only be seen as exploitative, founded on a chain of dominance/dependency relations comprising taxation and unequal exchange in commodities and centred on the core area.[30] A model of this exploitative system is represented in fig. 126. Although there are some technical deficiencies,[31] the model as such is adequate as a description of principle. In practice, however, it should be realised that such a model only works within Lattimore's inner zone of economic integration. Hingley disposed o this problem by demonstrating the possibility of energ transfer over long distances by various means.[3] Whether this is entirely justified remains to be show but even if it is there are three considerations not take into account. First, exploitation as such need not only b translated in terms of exchange or taxation. There ca also be another vital interest at stake, namely, security and an area may be primarily exploited for its potentia in this respect.[33] Second, imperial expansion is a compli cated process and, as entailed by the ideas of periphera imperialism discussed above, this may lead to expansio beyond effective limits from an exploitative point o view. This, however, can only be a temporary situatio which will eventually, or very quickly, be amended.[3]

30 See Hingley 1982 for a discussion.

31 As noted by Hingley, *op. cit.*, Appendix 3, it does not take into account the bypassing of notably the provincial level, where not all exchange is channelled through the provincial centre.

32 *Op. cit.*, 22. In addition to the African and Sicilian seatransports of corn to Rome, which are not to the point because these areas were, by its definition (above, p. 380), in the inner zone of economic integration, trade in slaves and precious metals are mentioned.

33 See, e.g., Luttwak's penetrating analysis of the strategica value of Trajan's conquest of Dacia (1976, 97–104).

34 General Corbulo's recall by Emperor Claudius is a example, although not a good one. The enterprise can be see as a case of peripheral imperialism with independent action b a representative of the core on the periphery, but in this cas there are good reasons to doubt that (cf. below, p. 390).

More important structurally is a third factor, namely, the basic character of the dominance/dependency relationship which allows the exploitation. This is based on power and, ultimately, on force. Originally, these were applied outwards from the core. This remains true for power, which is the essential element of the empire system as such. But the change from a hegemonic to a territorial empire implies that the instrument of force, the army, is no longer located in or around the core but at the periphery. For a truly general model, in terms of energy flows, this does perhaps not make all that much difference, because it can be argued that such flows still pass through the core and from there to the periphery, notably in the form of coinage. But for a model such as in fig. 126 it implies that a substantial part of the raw/basic goods does not pass up the pyramid but goes more or less directly to the periphery, or stays there.

From these arguments it may be concluded that the outer provinces, while having administrative relations with the core (which include taxation), are not necessarily also integrated in the imperial economic system.[35] This brings us to the much debated question of the economic dependency of the northern frontier provinces: were they exploited,[36] or was there self-sufficiency or indeed support from outside.[37] An attempt to contribute to the answer with data from the Dutch river area is included below,[38] but it is perhaps not very plausible that a general answer exists because the actual situation may vary widely between different areas and in different periods. This despite the fact that there are quite convincing arguments for the view that the budget of the empire as a whole showed a surplus raised in the provinces of the Mediterranean and spent in the northwestern provinces where most of the army was located.[39]

In principle, these findings are thus in agreement with theoretical expectations and they can also be illustrated by literary evidence such as Strabo and Appian writing about Britain or,[40] for our area, Tacitus' remark about the army in Germany, which *Galliarum opibus subnixus*, was backed by the resources of Gaul.[41] But all this does not, as most writers on the subject have observed, conflict with a generally accepted basic policy of having provinces at least pay for themselves. As has also been observed, this premise is not directly relevant for any factual situation. But provinces are different , for example in environmental potential, and troops were never evenly distributed around the frontiers. Therefore, the situation in each area and possible changes during the successive stages of the Roman occupation need to be examined separately.

This is easier said than done, as is demonstrated by two recent studies about the burden represented by the army in Britannia, published in the same volume and arriving at diametrically opposed conclusions.[42] These papers also illustrate the danger of simplistic reasoning based on theoretical crop yields to establish a kind of provincial carrying capacity. The value of such data for calculations is disputable,[43] and they need to be studied in perspective. That is, taking into account variables such as the pre-Roman situation and all manner of change, notably in the size of the army, the volume and direction of trade, or the internal organization of the market system. These are factors to be examined below with respect to the Dutch river area, even though is it just a small region and only part of an in itself already small province. It is, however, a section of the zone directly behind the frontier of exclusion constituted by the *limes* and, within the framework of particularities of historical events and re-

35 Note that when 'integration' is indeed defined in Lattimore's terms as a function of the capacity to transport bulk goods at a profit, there is a vast difference between the theoretically 'outer' provinces along the southern Mediterranean and those in the north, even without other factors such as the structure of pre-Roman society and the nature of most frontiers there.

36 Cf. Hingley 1982 or Millett 1984 for Britain.

37 An overview of relevant recent literature in Fulford 1984.

38 See paragraph 11.4.2.

39 See especially Hopkins 1980 and Birley 1981. Hopkins' proposals do, in fact, correspond quite closely to what has been suggested here. He argues (*op. cit.*, 101) for the empire as comprising three spheres, namely:

a an outer ring of frontier provinces in which defensive armies were stationed;

b an inner ring of relatively rich tax-exporting provinces such as Spain, southern Gaul, northern Africa, Asia Minor, Syria, and Egypt;

c the centre, comprising Italy and the city of Rome, the seat of the court and the central government, which, like the armies on the frontiers, consumed a large volume of taxes.

40 See Birley 1981, 45.

41 Tacitus, *Ann.* I, 47, which refers, however, to the Tiberian and thus to the expansion-phase or pre-*limes* situation.

42 Millett 1984, Fulford 1984.

43 Cf. Fulford, *op. cit.*, 130–1. His objections are fully substantiated by the evidence such as collected by Slicher van Bath (1960) on crop yields between AD 500 and 1850.

gional characteristics, some consequences of the general perspective outlined above should emerge.

11.3 FROM DRUSUS TO CORBULO

11.3.1 Imperial Policy and the Formation of the Civitas Batavorum

From what was said in chapter 10 and the concepts introduced in the previous paragraph, the events in and around the river area before 12 BC can now be described as the result of an imperialist policy of a hegemonic empire securing its outer reaches of influence. These had been greatly expanded by a war of conquest which was most successful in those areas where previous energy/information flows had created internal socio-economic change. Indeed, it can be argued that Caesar's campaigns were in many respects a case of peripheral imperialism made possible by those antecedent processes. Apart from historical peculiarities and obvious difficulties on the levels of matter and energy, the lack of previous information flow can be seen as one of the limiting factors determining the outcome of the Gallic wars in terms of expansion to the north. The following decades were characterised by the effort to control the new northern periphery by the intermittent use of force but primarily by the manipulation of client tribes. This is normal policy for a hegemonic empire, and precious little information has been recorded about it.

Not surprisingly, this is very different for the next phase with large-scale military campaigns designed to expand the empire even further. Whatever else there is to say about this enterprise, one thing is very clear: it was a major effort planned and executed by those at the apex of the imperial core itself, who must therefore have felt that there was a necessity or interest at stake to justify the enormous expenditure of energy. It was surely not a relatively autonomous kind of operation triggered and steered by processes of peripheral imperialism, although this interpretation does not preclude that such processes played no part,[44] and neither does it implicitly entail support for a view which sees Roman imperialism as aggressive instead of accidental.[45]

The causes of the Augustan campaigns of expansion in the north can be and have been studied from many perspectives and an overview is rather beyond the scope of the present discussion. They range from pure imperialist aggression to such factors as the possibility of employing those legions still under arms after the end of the civil war,[46] the grave measure of geographical error which caused the necessary effort involved to be seriously underestimated,[47] and other more circumstantial causes. If anything, as appears from classical as well as modern literature on the subject, the problem is too complex to be attributed to any single cause. The evident availability of an energy-surplus is only a necessary, not a sufficient condition, although it may be both once the surplus is converted into a large standing army.

The history of the numerous political and military events is rather well known and need not be extensively repeated here.[48] Instead, it is more appropriate to examine briefly only the major events directly related to the river area as well as the information and deductions about the position of the region in a wider context.

Augustus himself stayed in Gaul from 16–13 BC, obviously to reorganize personally the hinterland for the planned campaigns into Germany.[49] This involved a new provincial organization of Gaul and moving the legions into the assembly area along the Rhine. The following census, in 12 BC, was directly related to this administrative ordering and a precondition for regular taxation.[50] In the new ordering, the Rhine zone itself seems to have been set apart, presumably to become part of the planned province of Germany. As things turned out, this resulted in two military districts which only under Domitian, between AD 83 and 90,[51] were formally converted into provinces.[52]

44 See, e.g., Timpe 1975, esp. 141 f., and Bloemers 1983a, 11.

45 See Garnsey/Whittaker 1978, chapter 1; see also note 10 above.

46 E.g. Wells 1972, 12; Von Petrikovits 1978, 54, Rüger 1984, 10–11.

47 Arguments from A. Klotz expanded by P.A. Brunt, followed by others (e.g., Luttwak 1976, 50 and note 158, Wells 1972, 6).

48 See Rüger 1968, chapters 1–3, Luttwak 1976, chapter 1, Von Petrikovits 1978, chapter 2.4–6 or Van Es 1981, chapter 3 for overviews.

49 Cf. Von Petrikovits, *op. cit.*, 54. In contrast to most other writers on the subject, Drinkwater (1983, 20–1 and 95) proposes an earlier date (27 BC) but his evidence is not very convincing.

50 Rüger, *op. cit.*, 14–5, 19.

51 Rüger, *op. cit.*, 50.

52 E.g., Bogaers 1960–61, 265 and Rüger, *op. cit.*, 16–31, both with older literature.

The historical evidence for the role of the river area in the first offensive under Drusus, which started in 12 BC, has already been mentioned in chapters 2 and 3. The operational nucleus of his army consisted of five or six legions supported by at least three times as many auxiliary troops.[53] These consisted of various kinds of Gaulish units brought to the assembly areas,[54] but also of locally recruited irregular units (*tumultuarii, Volksaufgebote*).[55] Although this is not explicitly mentioned, it can be deduced that Batavians from the river area also provided such units for Drusus' army.[56] The same may well have been the case when new operations in Germany were necessary and Roman troops under Tiberius in AD 4–5 again assembled in the river area. This was a successful operation and P. Quinctilius Varus, who took over the command, was apparently given the task of converting Germany into a province, completing Drusus' prior efforts.[57] After his defeat in AD 9 new campaigns for the third time brought an expedition army to the river area in AD 15–16. In this campaign, under Germanicus, Batavian participation in the large army is explicitly recorded.[58]

Little is said in the literary sources about conditions in the river area except various geographical and hydrographical details.[59] From what is revealed, it may be concluded that the Batavian area was initially very much the domain of a client tribe, with its own military organization and bound by a treaty to the hegemonic Roman empire. The inhabitants were not subjected to taxation and, although the intermittent presence of large numbers of troops and the drain of manpower caused by the German campaigns surely had an effect, economic life must have continued very much as before.

The same can presumably be said about the social organization, although the Roman policy of working through the native social structure must have reinforced it and notably the position of the tribal elite. In addition, there are sure indications for a transformation of the tribe and its territory to fit the needs of an administrative organization designed to encompass the new province of Germania Magna. In other words: the establishing of the *civitas* model. One of these indications is the gradual granting of Roman citizenship to the native nobility mentioned earlier.[60] Active interference such as the stationing of a *praefectus* is not recorded, but there are other clues. Very important is the evidence for centralization which, as discussed in chapter 10, is lacking for the preconquest phase. Even apart from the archaeological data the literary evidence, which has recorded the names of Batavodurum and Oppidum Batavorum, shows that such centralization did take place.[61] Another valuable clue is the epigraphical evidence for the office of *summus magistratus* (fig. 127), datable to the first half of the 1st century AD.[62]

Equally important as centralization is the changing nature of the Batavian troops, which can be deduced from the relatively numerous literary data. This has been done by Callies,[63] who demonstrates that by the end of the '60s the Batavian troops had already to a very large degree become regular auxiliaries. The final departure

53 Von Petrikovits, *op. cit.*, 55.

54 E.g., Alföldy 1968, 78, Furger-Gunti 1981.

55 See Callies 1964, 137–72, Alföldy 1968, 77–9.

56 There is literary evidence for units provided by the Nervii (Livius, *Periocha* CXLI; see Callies, *op. cit.*, 142) and even by the Frisii (Cassius Dio LIV 32, 3; see Byvanck 1943, 92, Callies, *op. cit.*, 143, note 58) and for an even earlier recruiting of Batavians for the imperial bodyguard (Bellen 1981). Moreover, there is ample evidence that such irregular units were quite common in this period and before (cf. note 55), there are Tacitus' references to the treaty with the Batavians and there is explicit evidence for Batavians only somewhat later (see note 58).

57 See Rüger 1968, 20, with further references.

58 Tacitus, *Ann.* II, 8 and 11. The army consisted of 12 000 legionary soldiers, 26 *sociae cohortes* and 8 *equitum alae* (*Ann.* I, 49) in AD 14 while in AD 15 (*Ann.* I, 56) from Germania Inferior four legions are mentioned with 5000 auxiliaries (10 *cohortes* and *alae*?) and a number of *tumultuarii*; and in AD 16 (*Ann.* II, 16) eight legions, two praetorian cohorts, and numerous auxiliary troops.

59 See Bogaers 1960–61 and chapter 3.

60 Chapter 2, 24. See also e.g. Vittinghoff 1976, 74. Men such as Chariovalda, leading the Batavian *tumultuarii* under Germanicus, and Flavus, the supreme magistrate, still lacked citizenship but Julius Civilis, for example, may have been granted citizenship when he was recruited by Gaius (see Bogaers 1955, 190, Hassall 1970, 133, Bloemers 1978a, 183) and others, such as Claudius Labeo and Claudius Victor, may have received it from Claudius.

61 See chapter 8, 236.

62 *CIL* XIII, 8771 = *ER* II, 91. Cf. chapter 2, 24 and note 21.

63 *Op. cit.*, 146–8.



Fig. 127 Altar from Ruimel (mun. Sint-Michielsgestel). The text should be read (cf. Bogaers 1960–61, 268) as follows: [M]AGVSA/NO HERCUL(i) / SACRV(m) FLAVS / VIHIRMATIS FIL (ius) / [S]VMMVS MAGISTRA(tus) / [C]IVITATIS BATAVOR(um) / V(otum) S(olvit) L(ibens) M(erito): 'Sacred to Magusanus Herculis: Fla(v)us, Vihirmas' son, supreme magistrate of the civitas Batavorum, willingly and deservedly fulfilled his vow'.

from the *antiqua societas*[64] took place in AD 69, when a *dilectus* was instituted,[65] the drafting of regular auxiliaries. However, as Alföldy convincingly argues, Batavian units still operating in the old style under their *dux* in Germanicus' time probably were already regular auxiliary troops in AD 43 at the latest, when they were posted to Britannia with the 14th legion to which they were attached.[66] This surely implies a changed relationship as yet another symptom of the administrative transformation taking place during the half-century preceding the Batavian revolt.

On the basis of what was said in chapter 10, it can als concluded that a fairly quick transformation of socio- litical units south of the Rhine may well have been a ical step, if not a necessity, in the situation after the cially denied but, in reality, evident failure to an Germany and Tiberius' termination of Germani efforts in AD 16.[67] The native organization in these gions – whether or not one is prepared to accept the gree of Roman engineering proposed earlier is not e relevant – was not a balanced and long-established but quite recently disrupted and restructured. A sim conclusion has been proposed by Vittinghoff, who gues that: *Anders als bei intakten, historisch gewachse Stammeseinheiten, die schon lange in ihrem Gebiet seßi waren und in überkommenen soziopolitischen Ordnun lebten, waren hier die verantwortlichen römischen Ma haber, losgelöst von solchem vorgegebenen Rahmen dingungen, weitgehend frei in ihrem Handeln.*[68] In an lier paper[69] the same author argues that such early s governing *civitates* may have been necessary because military apparatus as such was not equipped for the ministrative burden. Hence his conclusion that the mal status of Germania Inferior (and Superior) as a m tary district does not at all imply that these *civit* should therefore also be considered military territory There is no evidence of a large-scale military presenc the river area after AD 16, and of the eight legions in G many[70] the four stationed in Germania Inferior were camped upstream, the two nearest to the delta only Xanten.[71] It must be concluded that the troops in

64 Tacitus, *Germ.* 29 uses the term but in a different cont from which the usual deduction (e.g. Bogaers 1960–61, and note 8) is that the conditions from before the revolt v reinstated. There is no need to question this, although the tion that is was a favourable event should not be readily cepted. See also p. 402–3 below.

65 Tacitus, *Hist.* IV, 14.

66 Alföldy 1968, 45–8. For a summary of various opini see Bloemers 1978a, 82–3. The possibility that Batavian horts were only sent to Britain in AD 61 (Stein 1932, 166) been persuasively disposed of by Hassall 1970.

67 Note that this last major campaign contains several ments of peripheral imperialism again, if one is prepared to Germanicus' ambitions in this light (see Timpe 1968).

68 Vittinghoff 1976, 75.

69 Vittinghoff 1974. See also chapter 6, 191 and note 261.

70 Tacitus, *Ann.* IV, 5.

71 For an overview, see Gechter 1979, esp. 123–8, figs. 5 and *Tabelle* 14.

river area were auxilaries or some *tumultuarii*, or both, most of them conceivably Batavians because we know of the existence of one *ala Batavorum* and nine Batavian cohorts, eight of which were sent to Britannia in AD 43.[72] But before this, the troops were probably involved in the campaign against the Frisians in AD 28;[73] Batavians were certainly involved in Caligula's aborted campaign of AD 39–40[74] and perhaps also in the war against the Chauci in AD 41.[75]

As will be discussed below, the number of Batavian soldiers is in itself also an important source for further inferences. Unfortunately, although there is little doubt about their nature, the size of the Batavian units is not entirely certain. The total number of Batavian units in Claudian times, and probably even earlier, is eleven. These were nine cohorts[76] (because of their demand that the cavalry element be increased[77] these nine can be identified as part-mounted *cohortes equitatae*), one cavalry unit (*ala Batavorum*),[78] and the often ignored but quite important elite troop of the *numerus Batavorum*,[79] the emperor's bodyguard. Regrettably, no direct clues are available as to the strength of all these units, because it is evidently important to know whether they were *quingenariae* or *milliariae*. For the cohorts, the most plausible indirect clue is surely that forwarded by Alföldy, who used the known strength of the post-AD 70 Batavian auxiliaries as evidence. At that time, we know of a *cohors I quingeneria Batavorum* and the *cohortes* I, II, III, and IX *milliariae Batavorum*. Alföldy argues that these were not troops newly raised after the revolt, but the reorganized rebel troops,[80] which therefore must have been quingenary cohorts to keep the total number of men at approximately 4500. In addition, there is the general evidence assembled by Birley which shows that milliary units normally came into being only from the Flavian period onwards, in part through the doubling of existing quingenary units and in part through new creations.[81]

As far as the *ala* is concerned, this same evidence should also imply that it was not of double strength.[82] However, a reasoning similar to that for the cohorts, with a total number of soldiers remaining approximately the same, could lead to the conclusion that it was milliary because the newly formed *ala* after the revolt was the *ala I milliaria Batavorum*.[83] Of course, it is entirely conceivable that new troops were in fact raised or that the *ala* only became milliary in the 2nd century. But there is another interesting option, because it is conceivable that the new *ala* was formed from the existing one together with the Batavian *corporis custodes*, which were definitely mounted,[84] and had been disbanded and sent home by Galba.[85]

This would mean that the *numerus Batavorum* in Rome was of cohort strength at least. But this designation is only one of a number of names by which the German

72 Sprey 1953, 27; Callies 1964, 147; Alföldy 1968, 46; Hassall 1970.

73 Tacitus, *Ann.* IV, 78. See also Alföldy 1968, 139.

74 Suetonius, *Gaius* 43 and 45: when not as auxiliaries, then at least as the main body of his *corporis custodes*, Bellen 1981. The sometimes disputed visit of Caligula to the river area (see Bloemers 1978a, 77) is supported by an inscription on a wine-barrel from the first fort at Valkenburg which is likely to have belonged to the emperor's personal supply: C(aius) CAE(sar) AUG(ustus) GER(manicus). See Glasbergen/Groenman-van Waateringe 1974, 37, sub 5 and plate 11, and Haalebos 1977, 284. This same conclusion was also forwarded by J.E. Bogaers: Wat staat er op het vat van Velsen, *Westerheem* 27, 1978, 8–12 (esp. p. 10–11).

75 Cassius Dio LX 8, 7; Suetonius, *Claudius* 24, 3.

76 See Alföldy 1968, 43–8.

77 Tacitus, *Hist.* IV, 19.

78 Alföldy 1968, 13–4. There may, in fact, have been even more Batavian cavalry. This is suggested (cf. Sprey 1953, 85; Bogaers 1955, 190) by the fact that among the picked horsemen of the *ala Singularium*, under the command of Julius Briganticus (Tacitus, *Hist.* IV, 70), there may have been many Batavians. There is also evidence for Batavians in yet other units, such as the *eques* Flavus, son of Blandus, who was probably enlisted before AD 70 and served in the *ala Frontoniana* (cf. Alföldy 1968, 39 and 190, no. 75 = *ER* III, 241, no. 1207 A).

79 Suetonius, *Gaius* 43; in *Augustus* 49, 1 he refers to the same unit as *manus Germanorum*.

80 Additional evidence for this simple and elegant view could be provided by the fact that Agricola's Batavian cohorts at Mons Graupius were probably veterans, as is suggested by the phrase *vetustas militiae* applied to them (Tacitus, *Agricola* 36). For a new evaluation by K. Dietz, see note 189.

81 Birley 1966, 58.

82 Alföldy (*op. cit.*, 141) therefore implicitly considers the pre-Flavian Batavian *ala* as quingenary, although he shies away from stating this directly (*op. cit.*, 13–4). It is also implicit in Von Petrikovits 1978, 238.

83 This is, at least, the universal opinion of writers on the subject although the earliest dated record for the unit is a Dacian diploma (*CIL* XVI, 108) from AD 158.

84 Cf. Cassius Dio LV 24, 7, who described them as 'picked horsemen'. See esp. Speidel 1984, 38–9 for further evidence.

85 Suetonius, *Galba* 12, 2.

bodyguard of the Julio-Claudian emperors is known.[86] It is even referred to as *cohors Germanorum*,[87] for which Bellen has proposed a strength between 500 and 1000 men and which consisted for two-thirds of Batavians.[88] These figures are probably realistic,[89] which means that a number of some 500 Batavians is quite well possible. In addition, if other troops were only reorganized after the rebellion it is inconceivable that the elite troops, which according to Bellen and Speidel were carefully chosen for the emperor's horseguards, should have been wasted by not re-enlisting them.[90]

From the above considerations, it appears that the most realistic estimate of the total number of Batavians in the Roman army, at least from Gaius to Nero and quite possibly considerably earlier, was some 5500 men. Although it might be reduced by a few hundred, this figure is in principle also the minimum estimate. It does not even take into account Batavians serving in other units (cf. note 78). When an *ala* or even cohorts of double strength are assumed,[91] total numbers would reach 10,500. As will be shown below, this is also highly unlikely for other reasons.[92]

From what has been said so far, it is evident that the population of the river area must have been under considerable pressure before the middle of the 1st century, both in terms of the demands made upon them for military support and in terms of the imposed or manipulated changes in socio-political organization. From a supra-regional point of view the strategic if not already economic importance of the region also increased greatly during this period, due to its geographical location. An example is the trade between Gaul and Britain; there is evidence that the Rhine route drew off trade which had previously passed through more southerly channels.[93]

But the region as such, although at the actual perime of the zone of direct control after the Frisian rebellion AD 28, still remained a zone along the frontier of inc sion of an expanding, hegemonic empire.

That this is not merely an abstract analytical judgem but that it also indeed correctly evaluates what was in minds of people at the time is shown by Corbulo's ex dition against the Frisii and Chauci in AD 47.[94] This terprise may well have been partly caused by the gen al's personal ambitions and thus can be described a case of peripheral imperialism. But, as argued convi ingly by Van Es,[95] it is inconceivable that, even wh acting on his own initiative, the newly appointed co mander of the army of Lower Germany would on p pose and knowingly have acted against the emperor's plicit wishes. Therefore his recall, sweetened by the stowal of the insignia of a triumph, can only be taker the result of a conscious change of policy from the de sion-making group in the core area, the emperor and advisors.

This decision, which was never reversed, led to a c centration of troops south of the Rhine and infrastruc ral measures such as the digging of the Fossa Corbulo and triggered the establishment of a chain of fron forts by adding to and improving the already availa dispersed installations.[96] This can properly be seen one of a series of events marking the change from a he monic into a territorial empire and the final institutio a frontier of exclusion, the area beyond which beca the outer zone of military (and diplomatic!) acti Inside, in the river area, the increased activities and visible, permanent presence of troops as well as ot measures which we can only guess at, designed to cc plete the process of welding the native population in

86 Cf. Bellen 1981, chapter 3. For a short and complete overview, see Speidel 1984, 36–7.

87 Suetonius, *Galba* 12, 2. However, as Speidel (1984) has shown this designation probably means nothing and Bellen's arguments concerning the military organization and title of the unit are not correct.

88 Bellen, *op. cit.*, 53 and 36, respectively (the remainder consisted largely of Ubii).

89 Cf. Speidel 1984, 31. The 'military' designation as *cohors*, which he does not accept (cf. note 87), is not a vital argument for the estimate of the strength of the guards.

90 In contrast to all other units, for which this can only be assumed, it is reported that the horseguards were in fact dissolved (cf. note 85).

91 For older literature, see Sprey 1953, 27. Recently Hassall 1970, 135 and Krüger (ed.) 1976, 398, note 154.

92 See p. 397. The regional evidence thus also indicates the conclusion about reorganization of the pre-Flavian u instead of new drafting for the troops formed after the revo also quite probably correct. Otherwise the number of recr able men in early-Flavian times must have been almost twic high as need be assumed when all rebels were re-enlisted only casualties replaced.

93 Cf. Cunliffe 1984; see also chapter 10.

94 Tacitus, *Ann.* XI, 18–20.

95 Van Es 1981, 35.

96 Cf. chapter 2, 25–6. See also below, 403–4.

true Roman *civitas*, further accelerated this development. But before exploring this further, it is necessary to examine the evidence from the river area itself in relation to the general processes and historical developments outlined so far.

11.3.2 The River Area in the Early-Roman Period

Within the framework outlined above and on the basis of the data and inferences presented in paragraph 8.3, a number of issues can now be further investigated. These refer in the first place to changes in the region brought about by the phase of direct contact initiated by (the preparations for) Drusus' campaigns and leading to its factual annexation at least after AD 47.

Influence of the army

Only comparatively little direct evidence is yet available concerning the considerable military infrastructure necessary for these and subsequent campaigns. For the famous waterworks, the mole and the fosse, for which such direct evidence can hardly be expected, indirect clues have nevertheless provided certainty, respectively convincing arguments, regarding their presence and location as discussed in chapters 3 and 7.2. The camps, however, and especially the earliest ones, remain elusive. Evidence of occupation by Drusus' troops of the early camps indicated on fig. 19 (see also fig. 128) is, for those in the river area,[97] only available for Nijmegen, and then only from site 412 (Nijmegen-Hunerberg) when 12 BC is concerned.[98] The first occupation of site 417 (Kops Plateau) is, however, not much later and, on the present evidence, must be dated to c. 10 BC.[99] In addition, some kind of camp, however temporary, must have existed near the Bijlandse Waard (the hypothetical site 183, Lobith-Tolkamer) where the mole was built. The site is simply too far away from any known Drusian camp and the presence of the mole is a certainty, and therefore this conclusion is obvious and inevitable even in the absence of further archaeological proof.[100]

A similar line of reasoning can lead to acceptance of a fourth Drusian camp in the eastern river area, somewhere near the fork of Lower Rhine and IJssel. But in this case the reason for its presence, the fosse, is not a certainty but a probability, though a very strong one for which convincing arguments have been provided in chapters 3 and 7.2. The camp has, however, not been found although, as outlined in chapter 9, it is entirely possible that there is still a phase preceding the occupation of Period 1 in Arnhem-Meinerswijk (site 126). But there is not a shred of evidence at present to include site 126 among the Drusian camps.

Fortunately, we can be quite confident that Meinerswijk did figure as a camp in Germanicus' time and, although excavations are needed to confirm it, the same is probably true for Driel (site 117/8). The pottery from Meinerswijk 1 and the available earliest pottery from Driel so far do not reach further back than the 2nd decade AD. On the western fringes of Batavian territory, however, the well-known camp in Vechten is surely older and, although it is improbable that the site was occupied in Drusus' time on the basis of currently available evidence,[101] the finds indicate occupation at least contemporaneous with Tiberius' campaign in AD 4–5.

Apart from the camps along the Lippe,[102] three other sites beyond the Rhine should be mentioned. The first is the small base with extensive harbour works at Velsen in Frisian territory, which was occupied from c. AD 15–30.[103] The second is the as yet difficult to interpret, presumably temporary, installation in Bentumersiel at the mouth of the Ems which may well be datable to approximately the same period and a third might be indicated by a few finds from Winsum (not on fig. 128).[104] An over-

97 Other occupied sites were Xanten (Vetera I) and Moers-Asberg; along the river Lippe the huge camp of Oberaden and the adjacent establishment of Beckinghausen should also be mentioned (cf. Von Schnurbein 1981, esp. 20–1).

98 Cf. Bloemers 1979a, 27; Bogaers/Haalebos 1975, 139. It is not certain whether this pottery dates the large two-legion camp which may be somewhat later but in any case the site as such must have been occupied for some time between c. 15–10 BC.

99 Bogaers/Haalebos 1975. Gechter's (1979) proposal of 12 BC is rather early on the basis of the available evidence, but not entirely impossible.

100 Although forwarded by Bogaers/Rüger 1974, 90 it is, curiously enough, neglected on such recent maps as Gechter 1979, *Abb.* 54 or Von Schnurbein 1981, *Abb.* 6 which are on a small enough scale to allow its representation without difficulty.

101 Cf. Haalebos 1976, 200–2. The limited excavations are not, however, sufficient to conclude that an earlier phase is definitely lacking (see the overview in English by Wells 1972, 101–11).

102 The most recent overview is by Von Schnurbein 1981.

103 See Morel/De Weerd 1980 and Morel 1983. See also Bloemers/Louwe Kooijmans/Sarfatij 1981, 80–2.

104 Ulbert 1977. On Winsum, see also Boeles 1951, 127–8 and Van Es 1965–66, 60.

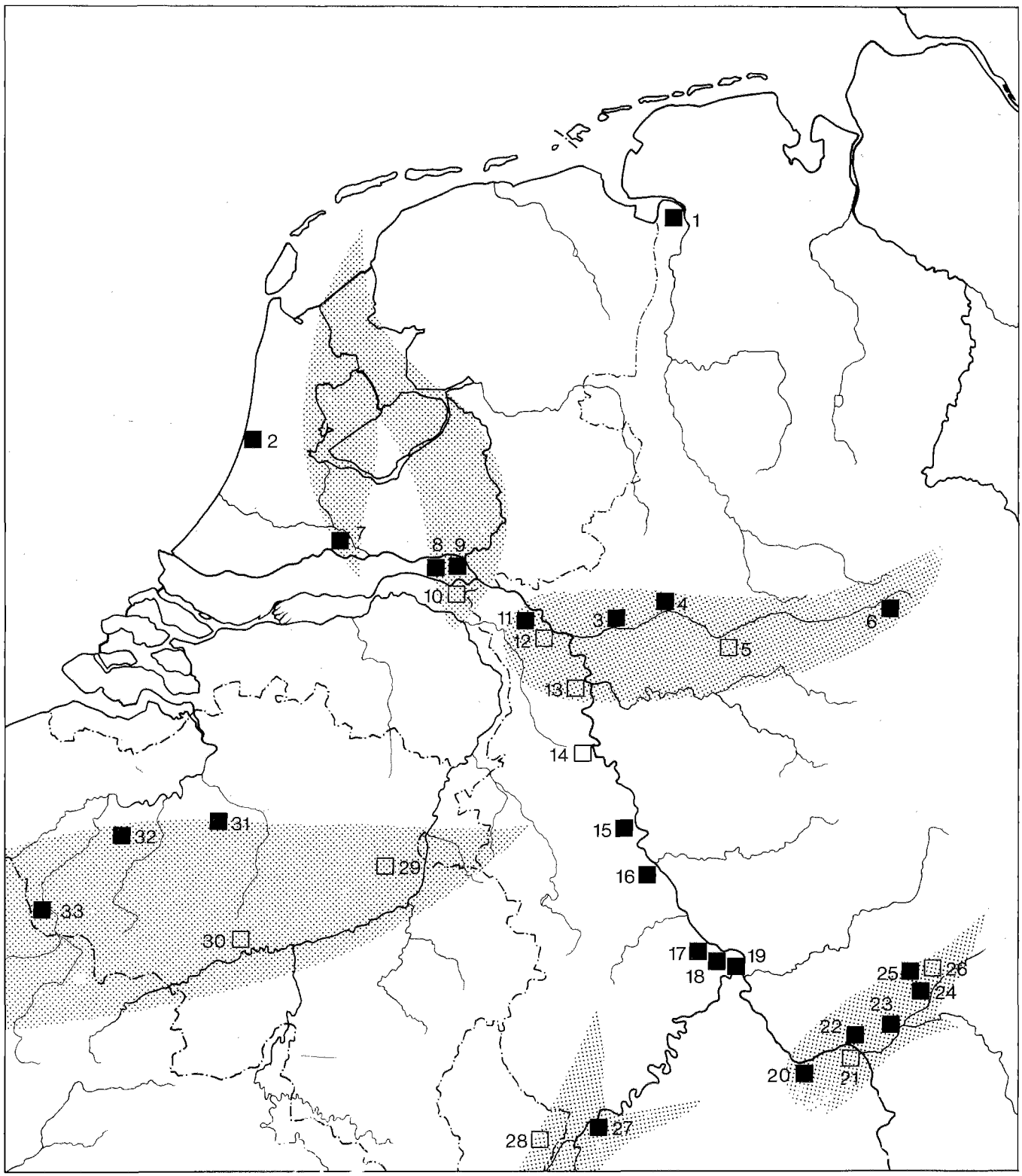

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Fig. 128 Early-Roman military camps. Open squares: camps used during (preparations for) the first campaigns under Drusus, and usually also later, black squares: later Augustan-Tiberian camps, shaded: major supply-routes to the assembly areas on the Rhine and routes for the offensives. The level of investigation of the sites varies! Sites in the rear, where the interpretation is the most problematical, are only indicated when there is evidence for direct military involvement. 1 Bentumersiel, 2 Velsen, 3 Holsterhausen, 4 Haltern, 5 Oberaden, 6 Anreppen, 7 Vechten, 8 Driel, 9 Meinerswijk, 10 Nijmegen, 11 Altkalkar, 12 Xanten, 13 Moers-Asberg, 14 Neuss, 15 Köln, 16 Bonn, 17 Aldernach, 18 Urmitz, 19 Koblenz, 20 Bingen, 21 Mainz, 22 Wiesbaden, 23 Frankfurt-Höchst, 24 Friedberg, 25 Bad Nauheim, 26 Rödgen, 27 Trier, 28 Titelberg, 29 Tongeren, 30 Liberchies, 31 Elewijt, 32 Velzeke, 33 Tournai.

view of the supra-regional situation with respect to the river area is presented in fig. 128, which is derived from several recent overviews.[105]

While all sites can be regarded as military camps it should be added, however, that only for those on and beyond the Rhine can the stationing of troops be taken for granted.[106]For those in the rear, which are often not yet precisely datable, this is more problematical. As far as they are not Drusian or pre-Drusian camps associated with the moving of troops into assembly areas along the Rhine, they should presumably be interpreted as supply bases, in the words of Mertens 'run by and for the army, and probably laid out by military engineers'.[107]

From a supra-regional perspective, it is clear that the bases at Mainz, Xanten, and Nijmegen stand at the head of three main invasion routes along the Lower Main, the Lippe, and the Drusian fosse (IJssel), and in their turn are connected by lines of supply bases directed towards Mainz and Neuss. But this is no more than the basic concept which rapidly evolved in various ways, with Köln soon replacing Neuss as the main focus for supply lines,[108] and additional invasion routes, such as that along Vechten (if not there from the beginning) and Velsen in the north. The dispersal of troops after Tiberius finished his brother's work in 7 BC and the construction of a large fort – obviously intended to last – in Haltern perhaps in the same year,[109] testify to the intended annexation of Germania Magna terminated in AD 9.

With regard to the impact of the various structural measures and the intermittent presence of large numbers of troops on the society in the river area, a number of deductions can be made. As a very basic fact, we know that the landscape itself was changed, but not only because of the waterworks. The period of the large campaigns until the 2nd decade AD was characterized by large-scale clearances and arable farming, as evidenced by the very precisely dated pollen diagram from Nijmegen.[110] Although this deforestation also served to provide wood and there are other examples of materials used, such as the harvesting of heather used in the Meinerswijk camp, this is a very important discovery. It proves beyond any doubt that at least a serious effort was made to provide food locally. It is virtually certain that the diagram in question has no regional significance, but that does not alter the conclusion. It is, in fact, what was to be expected because it is hardly conceivable that the existing agricultural regime could be changed so abruptly and geared towards corn growing as happened from the very start of the occupation at the Kops Plateau. Perhaps even more important: it is extremely unlikely that the Roman general staff would have wished to force their Batavian allies to do such a thing. In this respect it is useful to repeat what was mentioned earlier, that the early-Roman camps in Nijmegen were established at a location – on the Pleistocene ice-pushed ridge – which had some military advantages and which was virtually uninhabited. Seen in the present perspective, the latter fact may well have been of some importance.

As far as the available data permit us to reach a conclusion, the general pattern of agriculture, which was earlier found to have been strongly directed towards cattle-

105 Especially Gechter 1979, Von Schnurbein 1981, and Mertens 1983. For further literature and details see there.

106 With the restriction that at Bentumersiel a true camp has not yet been convincingly demonstrated and in Driel no excavations could be carried out so far.

107 Mertens 1983, 163. The sites have ditches; sometimes an earth-and-timber rampart has been discovered.

108 Cf., e.g., Rüger 1984, 16. Also, more direct routes to Xanten and Nijmegen along the Meuse are entirely conceivable.

109 Von Schnurbein 1981, 43–4, 94; a slightly later date is possible.

110 Teunissen/Teunissen-van Oorschot 1980. It is worth noting that, as discussed in paragraph 7.2.2, the Kops Plateau is in principle not very suitable for agriculture of any kind. The fact that it was nevertheless used as such is thus an important conclusion in its own right.

breeding throughout the Late-Iron Age, changed little or not at all during the phase under discussion. In fact, the diagram provides strong support for this general conclusion because shortly after c. AD 10, with the possible start of a second major occupation phase, the indications for cereal cultivation are reduced as abruptly as they started but are not replaced by those of a growing forest. Instead, there is clear evidence for extensive cattle-breeding.

Bogaers and Haalebos took this as a further indication for their – unfounded[111] – suggestion that the site could have become a civilian settlement. The finds do not offer any basis for such a conclusion although the stratigraphical and pollen-analytical data surely point to a change after c. AD 10. Until further excavations are carried out,[112] it is therefore more logical to assume a changed nature of the troops stationed at the Kops Plateau; these were at first probably (at least partly) legionary as is evidenced by the graffiti with mainly Latin names and even one indication for a legion, namely, the *Legio I G*.[113] The growing of corn in connection with such troops is perfectly understandable, indeed precisely what was to be expected. Their departure or, as Bogaers suspects, destruction under Varus in AD 9 should have left the site to auxiliaries or, most likely, to Batavian allies[114] who would surely not have been provisioned from elsewhere with basic foodstuffs and, if anything, have practised agriculture in their own traditional way. Although there is, except for chronology, nothing to substantiate this, it is entirely possible to envisage Chariovalda's herds grazing on the Kops Plateau on the eve of Germanicus' new campaign while he and his troops assembled there.

Irresponsible as it may be from a historical point of view, such an interpretation fits the known facts about the Kops Plateau, about the nature of society in the river area, and about the use of irregular units from the region by the Roman army. It is even consistent with the model outlined by Nash for the pre-conquest Belgic groups as warrior societies having an economy with an especially well-developed pastoral sector.[115]

Late-Iron Age society in the river area, the periphery of this Belgic core area, would seem to fit this picture rather well and the arrival of Chattian immigrants and resultant development into a people repeatedly and explicitly described by classical authors as particularly qualified in warfare will hardly have changed conditions. As Nash points out, what she terms a 'warrior economy' affects a society's use of territory, because pressing military demands upon men of productive age during peak seasons of the agricultural year means that less labour-intensive but territorially more demanding pastural activities often increase at the expense of intensive arable farming. In addition, she sees the accumulation of herds not just as a subsistence strategy 'but as an important form of wealth necessary for articulating contractual relations among compatriots'.[116]

Such a line of reasoning is entirely plausible and from the known demands on the Batavians, attached to the Roman armies as *tumultuarii* in no small numbers, it is probable that their economic system, at least during the first decades of Roman presence, was further strengthened in its existing pattern. Moreover, it can be argued that Batavians could never have had such good relations with Roman authorities and served in such large and at first presumably increasing numbers if their economic system had been very different. It is notoriously difficult to part a farmer from his plough, let alone hundreds or thousands of them, and not provoke reaction. From this perspective the Batavian military involvement can thus be seen as an additional argument for the economic system outlined earlier.

Population size

When the above line of reasoning is pursued in another

111 Bogaers/Haalebos 1975, 169; see above, p. 236: there is no indication for an increase in native pottery. In addition, the civilian settlement and Batavian capital has already been identified as site 403 on the Waal, and the Kops Plateau, because of its high location and as evidenced by the lack of pre-Roman habitation, was surely not an attractive place for a settlement.

112 These will be started in 1986.

113 See Bogaers/Haalebos 1975, 163–7. For an overview of comparable data from broadly contemporary camps, see Galsterer 1983, 70–2. Bogaers would apparently prefer to read *legio I Gallica*.

114 Although this is in no way conclusive evidence, it is nevertheless remarkable that the military metalware from the 1971–2 excavation contains many finds associated with mounted troops: three out of four items from the first (pre AD 10) phase (nos. 2–4) and five or six out of seven from the second phase (nos. 5–9 and perhaps 11) as well as two of the four stray finds (nos. 14–15). See Bogaers/Haalebos, *op. cit.*, 157–8. In addition, the graffito mentioning the *legio I G* could conceivably have been from an *aquilifer* or an *eques* from that legion (*op. cit.*, 164).

115 Nash 1984.

116 Nash, *op. cit.*, 100–1.

direction, the number of Batavian soldiers should also provide some clue as to the size of the population. This group of soldiers, at least from about AD 40 onwards, must have amounted to some 5500 men.[117] The size of the population necessary to yield such a number of men, the majority of whom can be assumed to be in their twenties, is quite large. The more so when we must accept as a basic premise that this number was not the maximum number of warriors that the society as a whole was capable of bringing in the field for a campaign or for one battle but, instead, the number that was constantly kept under arms while still allowing socio-economic life to go on more or less undisturbed. Probably we should assume that the balance was rather 'less' than 'more', as we know from later developments that the population was under some pressure at least, and the Roman authorities were more than likely to have required a maximum number of these valuable troops.

From these considerations it can be deduced that the percentage of the total population under arms must have been considerable, but nowhere near the figure of 25% proposed by Steuer and some others.[118] The ethnographic and historic[119] evidence only shows that the maximum number of fighting men in a population can be as high as one-fourth. There is, however, a passage in Caesar's report which gives a perfect description of how a warrior society, *in casu* the Suebi, normally operated: 'It is said that they have a hundred *pagi*, from each of which they draw one thousand armed men yearly for the purpose of war outside their borders. The remainder, who have stayed at home, support themselves and the absent warriors; and again, in turn, are under arms the following year, while the others remain at home. By this means neither husbandry nor the theory and practice of war is interrupted'.[120]

Apparently, socio-economic life was thus geared to function normally when about half of the men capable of bearing arms were away from home. Applying this to the Batavi, and there is no reason why this should be a dubious procedure, results in a figure of 11,000 fighting men. In view of the number of one-fourth mentioned earlier, the minimum size of the population can thus be estimated at 44,000. This must indeed be a minimum when it is considered that such factors as replacement of casualties, or the conceivably very important impact of a dropping birth rate due to postponed marriages or separation of couples,[121] are not taken into account. Before AD 43 this may not have been much of a problem.[122] But from then on, with eight cohorts away in Britain, in addition to those serving the emperor as his bodyguards, it certainly was. The functioning of a warrior society, witness the Suebi system, may well have been naturally suited to allow a client tribe to provide large numbers of *tumultuarii* for Roman campaigns. The permanent absence of these people, however, is a fundamentally different matter, with consequences liable to provoke a reaction. But then, that is precisely what happened.

There are, evidently, other ways to arrive at a population estimate. Bloemers, calculating on the basis of a demographic reconstruction for the population of the settlement in Rijswijk and regional carrying capacity,[123] arrives at an estimate for the Cananefates of between c. 3200 and 9200 people from which c. 1000 auxiliaries were raised. The large margin of error results from the complicated statistics involving many different and varying factors such as fertility, infant mortality, and the available productive land. In general, it seems that for men available for the auxiliaries (permanently absent) an average of 1.2 men per household every 20 years is likely,[124] perhaps slightly higher (1.5 men) under favourable conditions.[125] For the Batavian population, following Bloemers' calculations, this would imply that in order to provide 5500 soldiers on a permanent basis, roughly between 4600 and 3700 households (estimated at between

117 See above, 390. The minimum may have been c. 5000, a maximum beyond 6000 is very unlikely.

118 For an overview, see Steuer 1982, 61–2.

119 E.g. Caesar, *BG* I, 29 (Helvetii), of which one-fourth is able to bear arms, but then the remainder is explicitly identified as consisting *pueri, senes mulieresque*, of children, old men, and women. Von Petrikovits (1978, 238) assumes a maximum of one-sixth, but arrives at a maximum estimate of 100 000 Batavians. His calculations would give an – unstated – minimum population size of 60 000.

120 Caesar, *BG* IV, 1. Interestingly enough, in one sentence further on these same Suebi are reported to 'make not much use of corn for food, but chiefly of milk and of cattle, and are much engaged in hunting'. For further comment, see Thompson 1965, 3 f.

121 For a penetrating analysis of this and many related problems, see Imhof 1977, chapter 3.

122 Assuming that after Germanicus most troops except the *corporis custodes* were still in the vicinity and at least regularly at home.

123 Bloemers 1978a, chapters 5b and 6d.

124 Bloemers, *op. cit.*, *Abb.* 24.

125 *Op. cit.*, 107.

5 and 8 people per household) are necessary. This is, however, only the number of households (and thus between 18,500 and 36,800 people) to keep the number of soldiers constant, not the number necessary for providing them initially.

It is difficult to believe that in the first half of the 1st century the average existing household could have provided more than one person on a permanent basis, especially when it is considered that the household size of 5–8 people is a well-founded estimate.[126] Although more than one soldier from one household is possible, it is even attested to,[127] there were surely other households not able to contribute. The maximum population of 44,000 (5500 households of 8 people) calculated this way equals the minimum proposed earlier, but smaller numbers are possible. However, in view of the argument provided above, it is not very likely that the total population involved could have been much less than c. 40,000.

It is extremely difficult to relate this figure to the carrying capacity of the Batavian tribal area, as Bloemers did for the Cananefates, because the eastern river area covers only part of this area for the rest of which detailed geological information is not yet available and, moreover, its true extent unknown. There is, however, an alternative approach based on a general estimate of the tribal area and the data from the eastern river area. The maximum boundaries should enclose the area south of the Rhine between approximately the mouth of the Vecht or somewhat further (e.g. Bodegraven, Woerden, see figs. 130 and 135) in the west and Qualburg in the east, extending over the entire delta to an unknown distance into the Pleistocene sand area in the south. The analytical border from figs. 3 and 135 may serve as an estimate of the southernmost extension because if anything it is too far south. In terms of habitable area, the central and eastern river area should be roughly comparable,[128] while the southern part is somewhat larger but, at the same time, agriculturally less attractive.[129]

From all we know it is surely not an exaggerated estimate when it is assumed that the population in all three areas was approximately equal. This may be used to calculate the total number of settlements on the basis of the figures for the eastern river area during the Late-Iron Age and Early-Roman Period, which is a maximum of 250–300 settlements.[130] This gives a maximum total number of 900 'Batavian' settlements, which should accomodate a minimum number of 40,000 people. Even allowing for a very large central place, for example, of 1000 inhabitants, this gives an average of 43.5–44.5 people or at least 6 up to 9 households per settlement.

From the little information we have about settlements, this figure cannot be rejected absolutely, but as an average it seems to be excessive. Above,[131] an average of 'one or a few' was considered, although the loosely connected household units forming the larger dispersed settlements known from the Middle-Roman Period may also have existed. A settlement such as Rijswijk-De Bult consisted of only one household in the Early-Roman Period and this situation is not likely to be an exception. In all probability, therefore, the two lines of reasoning do not lead to an acceptable conclusion. What is acceptable is not so easy to define, but on the basis of what is known or suspected about sites, an average of 3–4 households could be possible, because it allows both a number of known or suspected one-household settlements as well as larger communities. What, then, are the implications of this assumption?

A first possibility might be that the estimated minimum number of 40,000 Batavians is far too high. This is rejected here because it is impossible to reduce it to the 20,000 people which should have populated c. 900 settlements of 3–4 households each, but which could never have permanently provided 5500 soldiers.

A second possibility could therefore be that the estimated number of soldiers is too high. This is conceivable, as not all units need to have been full strength. But even when all units consisted of no more than around 400 Batavians there would still have been around 4500 men,

126 *Op. cit.*, 55, note 10 and relevant literature in Imhof 1977. Calculations in terms of households are preferred because the term allows avoidance of the intricacies of the concept of family and its different meanings.

127 See *L'Année Epigraphique* 1952, 147–8: The Batavian brothers Gamo and Hospes as well as Indus and Eumenes. Although there must have been more cases like this there is no reason to assume an unequal sex-ratio with a surplus of males. Except for a factor like maternity mortality, an unequal sex-ratio such as caused by regular female infanticide is improbable. The practice of infanticide was perhaps forbidden (cf. Tacitus, *Germ.* 19), although presumably not under all circumstances (cf. Much 1967, 293).

128 See e.g. Van Tent 1984.

129 Cf. chapter 7.2. The available evidence also indicates a less, if not considerably less, denser occupation.

130 Above, p. 223 and 232–3.

131 See e.g. chapter 8, p. 284.

perhaps as few as 4000 when the contingent in Rome was only small and the evidence for a ninth cohort is rejected. This number would, however, still presuppose a population of c. 32,000 people.

This difficulty does have another interesting implication, namely, that it is wholly impossible that the number of Batavian soldiers was higher than hitherto assumed. If anything, it was smaller and the possibility of milliary units can be completely rejected, which conforms well to the general absence of indications for such units in pre-Flavian times.

Yet another possibility is that the total number of settlement sites was not 900 but 1800. As it is inconceivable that the other two regions of the tribal area were more densely populated than the eastern river area, this would mean that the estimated maximum number of settlements in our study area is not 300 but at least 600. As there is at least 300 km^2 of habitable surface,[132] this is theoretically possible, but there is no way in which so many sites can be fitted into the evidence on site densities in the river area.[133] Even when the estimates put forward in chapter 8 should still prove to be too low, the overall picture of known sites and the control of this picture by the extremely detailed soil surveys in a few areas excludes even a remote possibility to double the estimated maximum; but it may be higher than hitherto assumed and when combined with an average of more than 8 people per household could provide a partial solution to our problem. An additional possibilitity is that current assumptions about the density of habitation in the Pleistocene area are wrong, which could also increase the total number of sites.

A fourth way to bring our estimates into line, a reconsideration of the size of the tribal area, is not an attractive procedure. The perimeter outlined earlier is surely the maximum extent conceivable and cannot be extended when room is to be left for such groups as the Tungri, Cananefates, or Ubii. Only north of the Rhine could some area perhaps be added. An alternative option, that the Batavian units did not consist almost entirely of Batavians, can be rejected because the available information clearly indicates that this was not the practise for units from Lower Germany and Belgica.[134]

In addition to all these deliberations, there may be other implicit or explicit assumptions or deductions which are not entirely correct. The discussion so far has shown that figures first designated as minimum estimates can easily be turned into almost untenable maxima by changing one or other basic assumption. As a whole, however, there are a number of probabilities or even certainties to be derived from all this. These can be formulated as follows:

1 The size of the Batavian population can be estimated at between 30,000 and 40,000 people, constituting between 4000 and 6000 households grouped into between 1000 and 1500 villages, or an average of 1250. In all probability, this figure is valid not just for the mid-1st century AD but for at least the century preceding the Batavian revolt and for some time after.

2 To accomodate this population the maximum size of the tribal area is not really large enough when earlier site density estimates are maintained, which means on the one hand that this maximum size is indeed plausible and on the other that population densities were presumably even higher than assumed in chapter 8; for the eastern river area at least 400 settlements need to be assumed, which means that site densities on deposits not surveyed in detail, or eroded, should be much higher than indicated by the available evidence. Especially the later eroded banks of Roman-period rivers or those covered by later bank deposits come to mind in this respect.

3 It is certain that the Batavian auxiliary units were not of double strength. They may have been just up to regular strength, but if they were the pressure on socio-economic life in the area must have been enormous. This pressure must have been quite considerable even when all units were in fact undermanned or in case of the (remote) possibility that there was one unit less than is generally deduced.

Socio-economic structure

As mentioned earlier, there are no indications that the native subsistence system in the river area, with an emphasis on stock-breeding, changed at all during the Early-Roman Period. It was presumably even well suited to the demands upon the population in the form of manpower for the army. Only after AD 43, when many men were permanently absent, this system may have come under considerable stress. Obviously, the Roman troops in the area – which were probably not numerous except during relatively short periods when they were very large indeed – will have obtained supplies from the local population, but the exchange remained limited.

There are a number of indications for this. First, there is

132 Chapter 7, p. 221.
133 See also chapter 3, p. 73–5.
134 Cf. Alföldy 1968, 100.

no evidence for an increase in arable farming except on the Kops Plateau and only for a short period which can be directly linked to the presence of Roman legionaries. Evidently, the army was forced to grow its own corn to reduce or save costly imports. Other foodstuffs, except probably meat, were imported, for example, wine, olive oil, and other exotica. Second, the number of early-Roman artefacts in native settlements is very limited indeed,[135] which also testifies to the limited exchange. Although an interpretation in purely economic terms is not justified, the same is true for native burials, where only very few individuals received Roman pottery. This in marked contrast to the Nijmegen cemeteries.[136] Third, native pottery is very scarce in military camps where all pottery which could not be manufactured on the spot is imported.[137] In addition, other artefacts were also produced in the camps, in Nijmegen as well as elsewhere.[138] Fourth, the Batavians are specially reported to have been exempt from taxation, which also rules out one-sided 'exchange'.

It can be concluded, therefore, that the new military and administrative structure was supported and maintained by outside means in so far as it could not provide for itself. Obviously, the Rhine and the Meuse[139] are excellent transport routes for imports, although many of these goods, in particular before AD 9 and later during campaign years, must have been intended for personnel beyond rather than on the Rhine.[140] It is very significant that this interpretation of maintenance by outside means can also be extended to the capital Batavodorum, which can only be identified with site 403 on the Waal, in the centre of Nijmegen.[141] The excavations have shown that this site was as Roman as it could possibly be, and that it was a new foundation in Augustan times.

It is thus almost a perfect case of an administrative centre 'given' to the Batavians as the nucleus of their emerging *civitas*. It is not at all certain that the settlemer quickly became more than the official centre with litt more than formal ties to the native socio-political stru ture. This, at least, can be observed for the early stag of formation of the Gaulish *civitates*,[142] where the dire involvement with the capital was left to underlings whi the chief magistrates of the *civitas* were people with res dences and interests elsewhere. Evidence for this situ tion among the Batavi could be the findspot of the alt dedicated by the supreme magistrate, Flavus,[143] in Ru mel, the fact that a major temple was built, in about A 50, not in Nijmegen but in Elst (site 105), and the *ag* and *villae* of Julius Civilis on the *insula Batavorum*.[144] I contrast, official monuments, such as a column whic may have been erected on the occasion of Germanicu victory in AD 17,[145] were placed in Nijmegen.

But even apart from such interpretations, the archaeological evidence clearly indicates that the settlement sy tem in the river area can be seen as a collection of litt differentiated settlements tied into a social and econom network that only very gradually came to be admir istered through an imposed and artificially created an maintained centre in Nijmegen. At least at a materi level, evidence for the beginning of a process of acculti ration is very scarce, for example, the limited burial ev dence. But the developing trade and industry in Nijm gen, the influence exerted by other military installatior in the river area,[146] and not to forget the Batavian parti ipation in military enterprises surely served to accusto the native population to Roman concepts and values.

On the other hand, large numbers of Roman troops we present only for short periods and the Batavians we only gradually, perhaps in the '20s and '30s of the 1 century AD, converted into more or less regular auxili

135 Chapter 8, p.240–1 and fig. 68.

136 Chapter 8, p. 243–6.

137 Von Schnurbein 1981, 69 f. See also chapter 6, 160, fig. 35, for early kilns.

138 See Bogaers/Haalebos 1975, 156.

139 There are early sites along the Meuse, notably Maastricht and Venlo, where e.g. Arretine terra sigillata has been found. They are not indicated on fig. 128 because there is no direct evidence for military activities there.

140 Although the suggestion that Roman generals were not able to winter their troops in Germany (Drinkwater 1983, 23, 26) is not correct as such (e.g., Haltern), it is surely true that they had to be provisioned from Gaul and the known installations could all be reached from the Rhine over water.

141 Chapter 8, p. 236.

142 See Drinkwater 1983, 103–11.

143 See figs. 123 and 127.

144 Incidentally mentioned by Tacitus (*Hist.* V, 23) becau they were cunningly spared from destruction by Cerialis in A 70. On the nature of these sites, see also below, p. 423.

145 Bloemers/Louwe Kooijmans/Sarfatij 1981, 86; see fi 67. It is possible, however, that the column was only erected i late-Tiberian times (pers. comm. J.H.F. Bloemers).

146 There is as yet no evidence for military *vici*, cf. above, 239 and 243.

ries and then still remained native units under their own commanders. Direct interference with native affairs was presumably minimal. Sites on the bank of the Rhine were perhaps moved,[147] or used for a military settlement,[148] but, as noted earlier, the only really sizeable area occupied for Roman installations and other uses is in Nijmegen, on previously uninhabited land. Admittedly, this may partly have been a fortunate coincidence because the sites were probably chosen with a military objective in mind, but other objectives cannot be disregarded: the choice of location was ideal from various perspectives.

Significant changes occurred only in the '40s, in the early years of Claudius' reign. Caius had only just recruited additional Batavian troops[149] when the eight cohorts were moved to far-away Britain in AD 43, an event that must have considerably upset social and economic life in the region, even though archaeology has little to say about this directly. In AD 47, Corbulo's recall and the subsequent withdrawal of troops to behind the Rhine was followed by an outburst of activity in the entire river area, with the building of frontier forts and all manner of other infrastructural works which decisively changed the degree and nature of Roman interference in the region.

In addition to a (perhaps rebuilt) fort in Meinerswijk, a new fort was probably built in Duiven-Loowaard (site 194) and perhaps in Kesteren.[150] Driel (site 117/8) has also yielded ample evidence for Claudian occupation, and so have a number of forts further to the west mentioned in chapter 2. In Cananefate territory, Corbulo had a fosse dug which connected the mouths of the Rhine and Meuse behind the coastal dunes.[151] In Batavian territory the perhaps obsolete Drusian fosse is no longer mentioned but the *moles* or *agger*, as an important element in the *limes* system, is also reported to have been worked on.[152] For Nijmegen, literary or archaeological data indicating major changes are lacking, but the activities there surely increased. The Kops Plateau (site 417) continued to be occupied, at a cemetery such as that of the Museum Kamstraat (site 409) the majority of the burials dates from the reigns of Claudius and Nero,[153] and Batavodurum (site 403) also grew considerably,[154] and there is convincing evidence for stone buildings in this same period.[155]

Elsewhere in the hinterland, a fort was built in Cuijk (site 499), obviously to control traffic along and across the Meuse at the intersection with the major road from Tongeren to Nijmegen. Finally, in the centre of the Overbetuwe, in Elst (site 105), a true Gallo-Roman temple was built in about AD 50. As concluded by Bogaers in his dissertation, this was evidently the work of (Gallo-)Roman engineers and soldiers and the initiative of Roman authorities. He sees it as a 'national' sanctuary, the location of which may have been determined by conditions in native society, and was a politically motivated 'gift' from Roman authorities as a symbol of the Batavian-Roman *societas*.[156] Although it remains somewhat curious that such a symbolic site should be located outside Nijmegen,[157] the interpretation as such is surely acceptable and all the more so in the light of the general context. One is inclined to see the temple as the Batavian parallel to the *ara Ubiorum* in Köln, which has been interpreted as the central cult site for Germania Magna,[158] and to view it in a similar way as the formal epitome for the *civitas* Batavorum. By about AD 50 the stage for two centuries of further development had been set in its main outlines.

147 Cf. site 132, see p. 239–40.
148 Cf. sites 117–118 (Driel), see p. 237.
149 See note 74.
150 In addition to what was said in chapter 2, p. 25, recent (1984) excavations in the *vicus* at site 38 have produced pre-Flavian finds. See *JROB* 1984, chapter IIB, sub 2c.
151 Tacitus, *Ann.* XI, 20 and Cassius Dio LX 30, 6. For archaeological evidence, see Bloemers 1978a, 27, with further references.
152 For references and a discussion of the context, see chapter 3, p. 52–3.
153 Bloemers 1979c, 36.
154 Bogaers 1979a, Bloemers 1979b, 29.
155 See *JROB* 1982, 34; the earlier statement that definite proof is lacking (chapter 6, 185 and note 241) is now rendered out of date.
156 Bogaers 1955, 184 and 191–3.
157 Part of the explanation may be the possible presence of a previous, native, cult site (cf. chapter 8, p.239). An additional argument could be that Batavodurum (site 403) was the Batavian capital in name only, which might well be deduced from the archaeological evidence.
158 Rüger 1968, 20.

11.4 FROM CORBULO TO POSTUMUS

11.4.1 Revolt, Consolidation, and Growth until the Frankish Invasions

The revolt

Neque enim societatem, ut olim, sed tamquam mancipia haberi.[159] Thus, in the words of Tacitus, was the gist of Civilis' complaints in his speech on the eve of the Batavian revolt. Although this was surely not the way he said it, there is every reason to believe that Tacitus has left us an adequate representation of Batavian sentiments.[160] Evidently there is more to the revolt than its immediate causes, and its roots go back further than the last year of Nero's reign and the opportunities created by the confused events leading to and following his death. In fact, as was suggested in the previous paragraph, they can be traced back to at least a quarter of a century earlier.

When eight cohorts were sent to Britain in AD 43, this had serious consequences. As argued earlier, the permanent absence of all these men must have affected agricultural productivity. Due to the nature of the economy, with an emphasis on cattle-breeding, presumably this did not drastically undermine the subsistence base, but it must have caused a set-back. At least from this time onwards the Batavian troops were not relatively autonomous allies employed as *tumultuarii* when needed, but increasingly or even entirely pressganged into an organization of regular auxiliaries. Another consequence must have been a considerable social and demographic disruption. Fathers and sons were away from home for many years; casualties had to be replaced; young men in the army postponed marriage; married couples were separated. These or other, comparable, factors can only have led to a strong drop in the birth rate and, in combination with a higher male mortality, have caused a population decline which may have taken several decades to overcome.

When the effect of this development was still at its peak, new upheavals occurred. The river area became the scene of feverish activity with numerous foreign troops – where previously there had been only a few after Germanicus' day – engaged in building forts, dams, and other infrastructural works about which nothing has been recorded, such as roads and the temple at Elst. During the two decades following the events of AD 47, the population thus became subject to Roman interference in many different ways and on a scale never before experienced. Although much material may have been imported, the region's resources must have been exploited where possible. Where at first only the uninhabited forest on the ice-pushed hills at Nijmegen was used, now at least some tribal land must have been expropriated. In addition trees were felled, fish caught, gravel dug, game hunted and other resources tapped, to which there are bound to have been natives feeling that Romans had no right. With the increase of material and personnel flowing into the region came traders and, as formulated by Dyson,[161] other unofficial and usually more obnoxious members of the ruling power.

Moreover, it should be remembered that Batavian tribal society was still being moulded into a *civitas* model. Similar to the *tumultuarii* being forced into the straightjacket of regular auxiliaries, so society was transformed into a Roman *civitas*. It may well be argued that whatever progress was already made in this direction in the first half of the 1st century it needed to be brought to a satisfactory conclusion after AD 50, when the military commanders needed an effective civil organization to deal with the administration. In this respect, the building of the temple at Elst at about this time, is an ominous gesture.[162] There may have been advantages in all this for the native elite, but no doubt there were most unwelcome consequences as well; this elite group, in particular, must have been under considerable acculturative pressure. The apparent controversies among them during the actual revolt may stem from these conflicting effects.[163]

All such developments probably contributed to the general resentment which must have existed, as is shown by the immediate success of the revolt in its early stages. Perhaps this can even be supported archaeologically, with reference to the in itself not inexplicable[164] but nev-

159 Tacitus, *Hist.* IV, 14: 'For we are no longer regarded as allies, as before, but as slaves'.

160 Cf. on this specific passage, Callies 1964, 146–7, note 77. In general, e.g., Sprey 1953, Much 1967.

161 Dyson 1971, 269.

162 As noted by Bogaers (1955, 192) there is no way of telling whether the building was destroyed by the rebels in AD 69 or by Cerialis when he devastated Batavian lands in AD 70, but the former possibility seems decidedly more attractive, especially for people who started their rebellion in a sacred grove (Tacitus, *Hist.* IV, 14: *sacrum nemus*), a native, *not* a Roman symbol. Besides, why would a Roman general destroy a Roman temple

163 Julius Briganticus (Tacitus, *Hist.* IV, 70 and V, 21) and Claudius Labeo (*Hist.* IV, 18; IV, 56; IV, 66; IV, 70) were opponents of Civilis.

164 Cf. above, p. 240–1.

ertheless striking absence of Roman artefacts in settlements almost adjacent to major early-Roman sites.[165] This may seem to be rather far fetched, and in any case one would not wish to use it as hard evidence, but there is also abundant literary evidence to show that the revolt was very strongly put into a context of traditional native values and customs. Anthropology has provided us with a good insight into such uprisings, which are a very common phenomenon indeed,[166] and in his admirable analysis of native revolts in the Roman empire Dyson has shown that these fall into the same pattern.[167]

The Batavian revolt shows a number of characteristics which identify it as a strongly 'nativistic' uprising with unmistakable undertones of a 'prophetic' movement as evidenced by the important role of the Bructeran prophetess Veleda.[168] The revolt was initiated by Civilis, himself undoubtedly strongly Romanized as an auxiliary commander with 25 years of service behind him, during a ceremonial feast in a sacred grove.[169] Also, he dyed his hair red and allowed it to grow uncut until the destruction of Vetera, according to Tacitus some kind of native vow, and gives his son a truly barbarian upbringing.[170] Other nativistic elements are the customs of having women and children view the battlefield,[171] the clattering of arms,[172] the election of a leader by lifting him up on a shield,[173] and probably even human sacrifice.[174] In addition, there is evidence for the ethnographically well-attested phenomenon of the use of symbols from the dominating culture but converted into native terms or practices.[175] Thus, Roman military training was counted as an asset,[176] but the men were bound by Roman as well as native-style oaths,[177] and they did carry standards but native symbols were used as such.[178]

All these indications are ample evidence that the rebellion was indeed a nativistic revolt resulting from a longer period of stress and acculturative pressure. But it might still never have happened had it not been for three vital elements, namely, the favourable conditions created by the civil war, the presence of a charismatic leader (*praepotens inter Batavos*),[179] and the immediate cause which was the increased recruiting combined with excesses caused by cruelty and incompetence of those in charge. An additional important fact in this respect, which is explicitly mentioned by Tacitus (*Hist.* IV, 13), may also be that Vespasian's agents Primus Antonius and Hordeonius Flaccus actually incited Civilis to stage a revolt, to tie down Vitellian troops. The development of the uprising as such has been so extensively discussed elsewhere that it need not be repeated here.[180] It should suffice to note that its initial success led to the destruction of all symbols of Roman presence: not only is there literary and archaeological evidence for the destruction of all forts up to Xanten, and the dam across the Waal, but the temple in Elst was also demolished, while even Batavodurum may have been sacked.[181]

In retaliation, Batavian lands[182] were devastated by the

165 For a similar reasoning with indications for what they term *contre-acculturation*, see Gruzinski/Rouveret 1976, 200. But see also p. 412 below.

166 See, e.g., Linton 1943, Lanternari 1963, Köbben 1971 (esp. chapter 4), or Gruzinski/Rouveret 1976 (esp. chapter 5).

167 Dyson 1971, 1975.

168 Tacitus, *Hist.* IV, 61; IV, 65; V, 24.

169 Tacitus, *Hist.* IV, 14.

170 Tacitus, *Hist.* IV, 61 and *Germania* 31, 1. On further data about this vow, which seems to have been a deeply rooted symbolic act, see Much 1967, 385–8.

171 Tacitus, *Hist.* IV, 18.

172 Tacitus, *Hist.* V, 17 and *Germania* 11.

173 As happened when Brinno, the Cananefate, was chosen as a leader (Tacitus, *Hist.* IV, 15).

174 This might be deduced from the fact that the legionary commander Munius Lupercus was sent *as a gift* to Veleda (Tacitus, *Hist.* IV, 61). Compare *Germania* 9 and especially *Ann.* XIII, 57 where such sacrifices in relation to a victory are mentioned. Also, the fate of the captives given to Civilis' son for target practice, mentioned in the same paragraph of the Histories, will hardly have left them in good health.

175 A particularly horrible example is that of the Mexican Maya insurrection of 1869, where the anachronistic custom of human sacrifice was revived but in the form of a crucifixion so that the rebels might have a messiah of their own race (Gruzinski/Rouveret 1976, 201–2).

176 Tacitus, *Hist.* IV, 17: *militaris disciplinae in castris Romanorum.*

177 Tacitus, *Hist.* IV, 15: *barbaro ritu et patriis execrationibus.*

178 Tacitus, *Hist.* IV, 22: *depromptae silvis lucisque ferarum imagines, ut cuique genti inire proelum mos est.*

179 Tactitus, *Hist.* I, 59.

180 See Byvanck 1943, 220–79, Brunt 1960, or Van Soesbergen 1971 to name only a few.

181 When Batavodurum = Oppidum Batavorum = site 403 there is literary as well as archaeological evidence (Tacitus, *Hist.* V, 19 and *JROB* 1982, 34). When Batavodurum = site 403 and Oppidum Batavorum = site 417, the latter may not have been sacked but destroyed when it (as a camp of Batavian auxiliaries?) had to be cleared on the arrival of Cerialis' army.

182 In this case evidently only the area north of the Waal, the present-day Betuwe.

army of C. Petilius Cerialis, a man who had had his experiences with a native revolt in Britain.[183] He used not only military action but replaced it with diplomacy as soon as possible: a replacement of the use of force by that of power that already had its effect when Tacitus' narrative breaks off at the peace conference between Civilis and Cerialis. The attempt to become once again a client tribe, instead of a *civitas* under direct Roman control,[184] had thus failed. As a result, and quite certainly because Batavian troops and lands were as valuable or indispensable as before, the relation between Romans and Batavians was restored under its former conditions.[185]

Flavian consolidation

In view of the measures that could have followed the suppression of the revolt, it can indeed be argued that this was a very favourable outcome for the rebels. But the almost jubilant tone in which this termination is often hailed in recent literature is utterly unjustified.[186] Both literary and archaeological evidence clearly indicate that the conditions which were restored were not those of Augustus' day but the very different ones which had come into being under Claudius or even earlier and which had contributed to provide such fertile soil for the revolt. Undoubtedly, the excesses which were its immediate cause, and which may have been the result of the administrative breakdown and the additional pressure following from the troubles that started in AD 68, were remedied. But everything else changed for the worse, at least from a native point of view.

183 Tacitus, *Agricola* 17; *Ann.* XIV, 32.

184 Although his analysis takes a slightly different perspective, this is essentially the – entirely convincing – conclusion reached by Van Es (1981, 37–41). Civilis was not just a charismatic leader but an intelligent man who showed good sense at various stages of the revolt and who must have had a profound grasp of Roman policy and force. Although perhaps pushed by those behind him he cannot possibly have aimed at complete independence or even more. This despite the fact that in the wake of Julius Vindex' insurrection in Gaul (not a separation movement either, cf. Drinkwater 1983, 41) there were hotheads who entertained the notion of an *Imperium Galliarum* (Tacitus, *Hist.* IV, 59). These were, however, rapidly disavowed by their own people (*Hist.* IV, 69).

185 This can be deduced from Tacitus, *Hist.* V, 24–6 and *Germ.* 29; cf. Byvanck 1943, 279, Sprey 1953, 109, Bogaers 1955, 188, 193–4, and many others.

186 It shows, in fact, the bias of Romanists reasoning from the Roman point of view and much along the same lines as any Roman historian would have seen it.

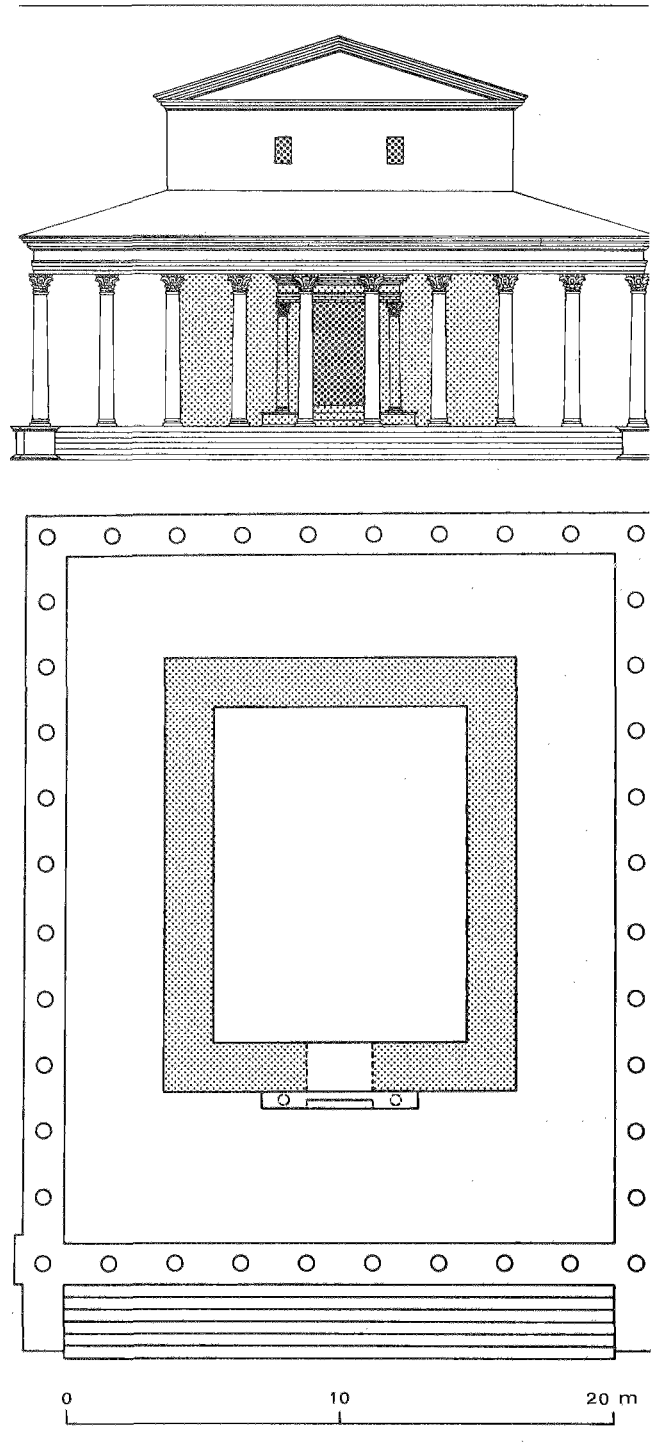


Fig. 129 Reconstruction of the second Gallo-Roman templ at Elst (site 105). After Bogaers 1955.

The leaders of the revolt, if they escaped with their lives, were surely neutralized.[187] In general, the role of the native elite as officers in the auxiliaries was also terminated: they were replaced by Italians, a situation which is typical for Flavian-Trajan times.[188] The number of Batavian troops was not diminished but, as mentioned earlier, they were reorganized and all of them sent elsewhere, first to Britain and later most of them to the Danubian provinces.[189] The largely Batavian horseguard of the emperor was, understandably, not reinstated.[190] In addition, the 10th legion was stationed in Nijmegen and the river area received its fair share of the large number of auxiliaries that were concentrated in Germania Inferior. In AD 70 for the Lower German district alone there were at least six *alae* and 23 cohorts.[191] These were all mixed, not national units, some consisting of Roman citizens and most raised in Spain, Britain, and the Danubian provinces. Particular care was taken to recruit many of the necessary new supplements from elsewhere, notably from Thracia.[192]

In addition to the direct damage as a result of the war, people in the river area were thus confronted with even more of their men being sent away and the permanent presence of many more foreign troops than before. These started to work with due speed on the repair of the burnt-down frontier forts and added some new ones in the process, which led to an even more closely spaced chain of such establishments all along the Rhine (fig. 130). A camp on the site of the former Augustan one in Nijmegen was rapidly converted into a true legionary fortress.[193] Other infrastructural works must have been repaired and further developed,[194] and the temple at Elst (fig. 129) was 'rebuilt by the Romans in a grand style very shortly after the revolt', according to the author of these words 'a splendid confirmation of the very favourable peace-treaty'.[195] The symbolic meaning of the rebuilding is indeed almost self-evident, but when viewed from a native context such an interpretation is either naïve or a cynical overstatement.

Shortly after AD 70, a new capital was founded on the bank of the Waal to the west of the largely deserted Batavodurum (site 403). The latter continued to be occupied, but covering a much smaller area and probably in direct connection to the military sites. Whether the new site (no. 399) ever developed into a fully fledged town is still rather uncertain,[196] but it is known that the work on its construction began in Flavian times. The traditional and the only logical interpretation of this is that it was decided to 'give' the Batavians a true civil capital, as was done in so many other areas where towns were founded. Although, evidently, great and noble deeds were performed and tasks accomplished from a Roman viewpoint, one can be quite certain that especially in the first years under Vespasian and Titus the modal Batavian in the river area must have been decidedly unhappy with his conditions of life. On the other hand, the enormous input of capital in the form of army pay and building programs must have boosted economic activity and been the source of wealth for increasing numbers of the local population. We shall never know how long and to what degree feelings of enmity persisted. But there is every reason to believe that already the following generation coming of age under Domitian, and surely the next one under Trajan, no longer shared their fathers' or grandfathers' feelings to the same extent, if at all. At least the archaeological evidence, to be discussed below, shows that they did profit from a relative prosperity and participated in the social and economic life developing in the consolidated *civitas* and protected by an effective system securing imperial borders.

The nature of this system of forward defence has been admirably analyzed by Luttwak.[197] It is a strategy of border defence connected with a territorial empire,[198] in which virtually all troops are located at the frontier. As

187 Civilis' fate is unknown. There is evidence that Veleda was later shipped to Italy and installed in the service of a temple there. See Dyson 1971, 265, and notes 65 and 66 with further references. Also Tacitus, *Germ.* 8 and Cassius Dio LXVII, 5 and the comment by Much 1967, 169.

188 Cf. Alföldy 1968, 101–2, 110–11, 115–6, 121.

189 Cf. Alföldy *op. cit.*, 14 and 47, with further references. A very recent overview, with new details, is provided by K. Dietz, Das älteste Militärdiplom für die Provinz Pannonia Superior, *BRGK* 65, 1984 [1985], 159–268.

190 The emperors employed regular auxiliary cavalry until Trajan instituted the *equites singulares Augusti*. By that time, however, at least half of the horsemen and perhaps even all of them were again recruited in Lower Germany (cf. Speidel 1984, 43–5).

191 Cf. Alföldy 1968, 132 and 148 f.

192 Cf. Alföldy, *op. cit.*, 103, 148.

193 Bogaers/Haalebos 1979b, 45–7.

194 Cf. chapter 3, 53. A further development are also the tile works at De Holdeurn started by the 10th legion.

195 Bogaers 1955, 194.

196 Cf. above, 265 f.; see also below, 418 f.

197 Luttwak 1976, chapter 2.

198 Cf. paragraph 11.2.2 and fig. 125.

can be deduced from what was said earlier, this strategy was introduced in Germania Inferior at least from early Claudian times onwards in a systematic way and the Flavian period merely saw its restoration and further extension.[199] As far as major threats are concerned, the defensive strategy operated in a tactically offensive manner. The *limes* as such was designed to serve as the supporting infrastructure but armed conflict was to take place beyond, not at or within, the border. This, of course, implied an early warning system and control of the foreland. The fixed defences on the frontier were not designed to endure prolonged attacks as in a siege. Smaller threats could be dealt with by intercept movements carried out by forces stationed in the frontier forts, while the legions were deployed in such a way as to ensure battlefield superiority in each sector of the frontier. At the lowest level, small-scale incursions could be thwarted by the troops from individual forts or fortlets and the border patrols.

Necessary elements in such a system are, of course, watchtowers and sometimes outpost forts, good roads, and an excellent communications network. In addition there is the use of power in the diplomatic control of the tribes living beyond the frontier. In this way, when the system functioned properly, all manner of outside threat could not only be countered but that could be accomplished beyond the frontier, thereby providing continuous, day-to-day security even for the frontier provinces and the actual frontier zones.

In fig. 130 the *limes* of Germania Inferior is indicated, from Claudius' reign onwards. [200] In its developed form, after AD 70, it is essentially one line along the Rhine, with only one possible (not indicated) and intermittently occupied (?) outpost fort (or camp) in Ermelo.[201] A second case might be the camp at Kneblinghausen, between the Lippe and the Ruhr c. 75 km east of the frame of fig. 130.[202] This camp, like that in Ermelo with two different phases and not very well investigated, is sometimes seen in relation to one or other of the Flavian endeavours in Germany, against the Bructeri and the Chatti.[203] A similar reasoning is possible for Ermelo, although the little material found there is Flavian as well as late-2nd to 3rd century. Obviously, more work is needed at both sites.

In the hinterland, three forts are known. Two of these are Vada (location unknown) and Rossum, conceivably to be identified as Grinnes, and presumably situated at a road junction near a Waal-crossing and very close to the assumed early-Batavian centre of Rossum-Lith.[204] If the identification is correct it was an auxiliary fort during the Batavian revolt,[205] but its later status is unknown: it could have been a *statio beneficiariorum consularis*. The latter has also been assumed for Cuijk (site 499/500), which was a fort only until c. AD 100.[206]

The withdrawal of the troops stationed in Cuijk may perhaps be connected to that of the 10th legion in Nijmegen in about AD 104, but in any case it is symptomatic for the general reduction of troops in Germania Inferior which had already started under Domitian.[207] He, undoubtedly as a further measure of administrative consolidation, converted the military districts of Lower and

Fig. 130 The *limes* of Germania Inferior from Claudius-c. AD 270: 1 legionary fortress, 2 fort, 3 possible fort, 4 fortlet, 5 modern border, 6 canal, 7 possible border of the province. Not all sites were occupied during the entire period.

199 As was also objected by Mann 1979, 179, Luttwak's opinion that the preclusive defence system with 'scientific' frontiers was a Flavian invention is thus surely not correct. But this is only a minor matter of historical accuracy and Mann's further comments on Luttwak's models are unjustified and stem from a partial misunderstanding of their purpose. They are not designed to describe historical particularities and not intended, which Mann (*op. cit.*, 180) seems to think, as general laws covering all situations. They are merely a heuristic device to help understand the nature of an empire and its means of ensuring security. Mann is right in pointing out that Roman frontiers were normally the result of practical *reactions* instead of *actions* and that Rome lacked institutes of strategic studies, but that does not relieve us of the task of performing such studies and thereby providing a new dimension to our understanding of historical 'facts'.

200 For further literature, see Bogaers/Rüger 1974, Gechter 1979, and chapter 2, note 31.

201 But see chapter 3, p. 68 and notes 207 and 210. For location, see fig. 6. Velsen was abandoned well before AD 70 but has been included in fig. 130 because there is a serious possibility that the occupation was only terminated in the early years of Nero's reign. Cf. note 269 below.

202 For its location (on the wrong map!), see Bogaers/Rüger 1974, *Karte* 1.

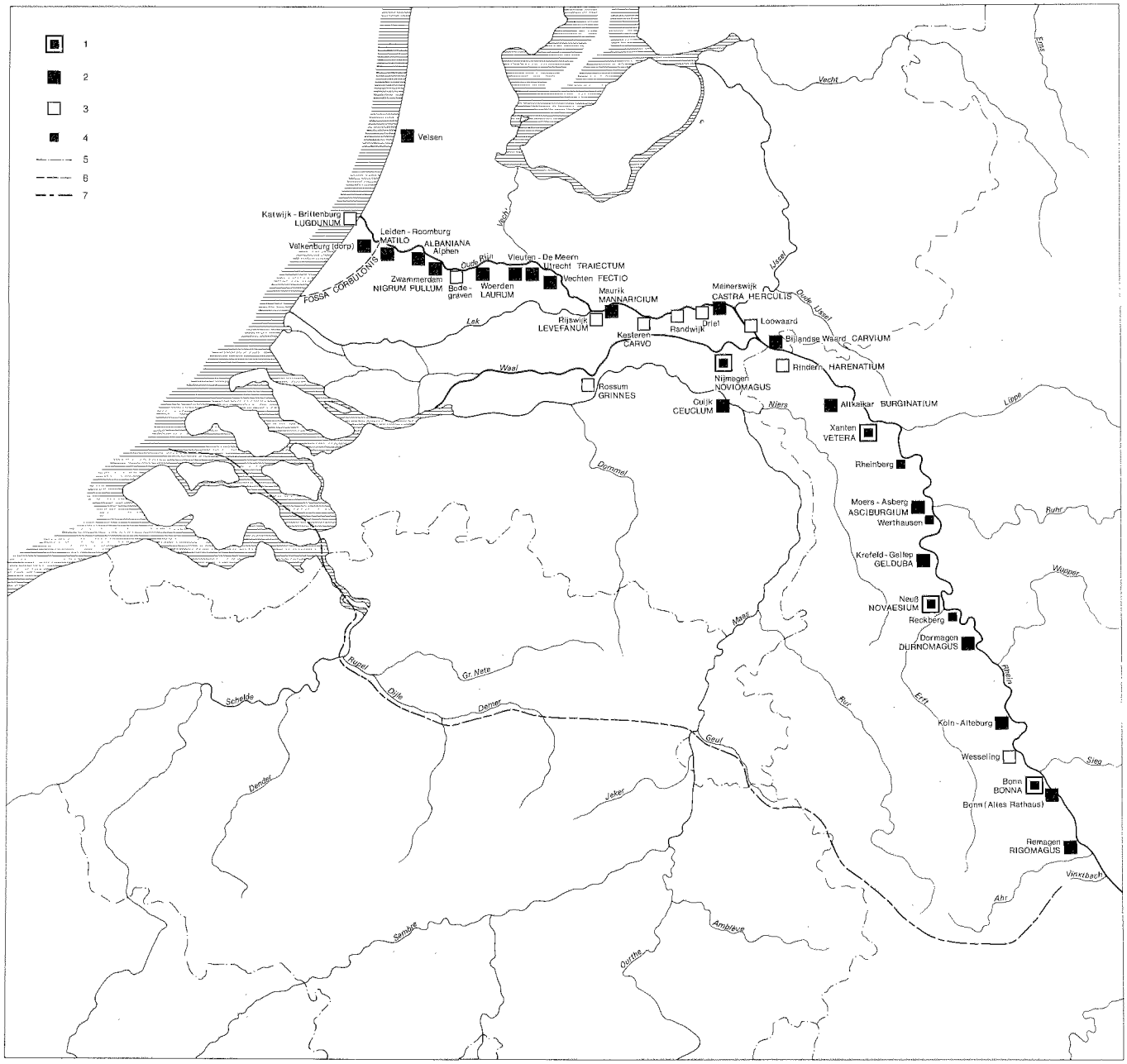
203 Cf. Krüger 1976, 397, note 153 or Von Schnurbein 1979, 7.

204 Cf. Bogaers/Rüger 1974, 74, Haalebos 1976, 203–4, and Roymans/Van der Sanden 1980.

205 Tacitus, *Hist.* V, 20.

206 Bogaers/Rüger 1974, 84–6 and above chapter 8, 258.

207 See Alföldy 1968, 149–52, esp. the table on p. 151.



Upper Germany into formal provinces between AD 83 and 90.[208] During this period, and somewhat later, some reduction of the number of auxiliary troops in the province began to take place, but substantial changes were made only after the Flavian period.

This entire phase can thus be seen as the rapid and in part compulsory transformation of the river area and other regions into a well-ordered frontier province with a secure frontier and internal peace. The latter was not achieved so much by changing the basic reasons for the revolt but rather by stamping out all thoughts of resistance by overwhelming force and presumably by proper

208 Bogaers 1960–61, 265 and Rüger 1968, 50.

and competent management. This not only prevented further excesses such as took place in the late '60s but should also have led to a fairly quick and surely profitable adaptation to the new conditions on the part of the generation coming of age after the revolt.
It is difficult to judge from the literary sources whether this involved a conscious policy to alter conditions in the frontier zone other than those already described. It is not likely that the established subsistence pattern of the native population could have been changed, especially not when so many men were in the army and, at least initially, the population was declining. But we do know that the central government did not hesitate to take regulating measures when deemed necessary, as is testified by Domitian's edict promoting the cultivation of corn by restricting the making of wine.[209] But then, the emperor had to abandon this policy rather quickly, which shows that such measures were not easy to enforce.

Further developments

In AD 98 Trajan, the experienced general and governor of Upper Germany, rose to the purple as Nerva's heir. He was well acquainted with the situation in both Germanies and as the emperor's adopted son presumably had assumed responsibilities in Lower Germany as well. That, at least, may be deduced from the fact that his accession to the throne was in Köln,[210] not the most logical place for a governor of Upper Germany.[211] Although he was personally primarily engaged in Dacia and Mesopotamia, Trajan's reign brought a number of important further developments for Germania Inferior in general as well as for our area. First, there is the fact that he revived the imperial horseguard in the form of the *equites singulares Augusti*, of which, initially, at least half the men came from Lower Germany.[212] Thus the honour of serving in the emperor's personal guard was restored to the Batavians and adjacent tribes, which can only mean that in Trajan's expert judgement they were worth it. Thus, at the beginning of the 2nd century, conditions in the river area must have been satisfactory from a Roman point of view which testifies to the effectiveness of Flavian policy as well as to changes in the native attitude.
Further evidence for this is, of course, provided by the withdrawal of the 10th legion from Nijmegen and the rapid reduction of troops in the first two decades of the 2nd century for the province in general.[213] The civil settlement in Xanten was elevated to the status of a *colonia*, Colonia Ulpia Traiana, and the town at Nijmegen, Ulpia Noviomagus Batavorum, was presumably granted the *ius nundinarum* as suggested by the Celtic name (newmarket).[214] The measure as such reflects a conscious concern that Nijmegen should function as a true central place, irrespective of whether the grant was an attempt to create a new function or the sanctioning of an established practice.
Under the favourable conditions of the 2nd century the somewhat reduced and temporarily declining civilian population can be expected to have increased again. Population growth may also have been stimulated by settlers from elsewhere, notably the veterans of the legions and auxilia who appear normally to have settled in the provinces in which they had been discharged.[215] Even more important in this respect could be that in the 2nd and 3rd centuries units recruited soldiers in the areas where they were stationed and in Lower Germany there is no evidence for the Flavian practice of bringing recruits in from elsewhere or for placing units under reliable Italian commanders, a procedure which must have died out under Trajan.[216] We might thus assume that many Batavian soldiers served at home again, but such a conclusion may not be entirely justified. There is epigraphical evidence[217] showing that Batavian units may –

209 Suetonius, *Domitianus* 7, 2 and 14, 2.
210 Eutropius, *Breviarium* VIII, 2.
211 Cf. the suggestion by Drinkwater 1983, 90, note 13. See also Bogaers 1960–61 (276–7 and note 92) who suggests that on his adoption Trajan's function as governor ended although he stayed in Germany where, sharing the *imperium proconsulare*, he was above both governors (and thus a situation was created which, according to Stein 1932, 23–4, only existed under Augustus and Tiberius).
212 Cf. above, note 190.
213 Cf. above, note 207. Of course, the reasons for the reduction of troops were not just that they were no longer necessary in Lower Germany but also that they were needed in Trajan's wars elsewhere.
214 See esp. Bogaers 1960–61, 289–91.
215 For summaries of the evidence, see Sebesta 1972, esp. 116–9 and 144–6 (auxiliaries) and Mann 1983, esp. 25–8 and 97–9 (legionaries).
216 Cf. Alföldy 1968, 104, 110–11, 116; Holder 1980, 142.
217 Two inscriptions, *L'Année Epigraphique* 1935, 163 (= *ER* III, 240, no. 1141A) and 1944, 97: Batavian officers of the Cohors III Batavorum. See also Holder 1980, 300, no. 1161A: a Batavian in the Cohors I Batavorum, recruited in about AD 88, and most recently Dietz (cf. note 189).

to an unknown extent – have retained their national character until at least the early-3rd century, if not even much later, an exceptional situation indeed.[218] In general, however, we may assume that in addition to the new settlers more Batavian soldiers served at or near home than was the case in the second part of the 1st century.

From Trajan times onwards only two legions were stationed permanently in Lower Germany.[219] The largest concentrations of troops in the empire were no longer to be found on the Rhine but in the Danubian provinces and in the east. Hadrian's concern for the frontiers is, however, well known and also had its effect in the river area where no additional works on the frontiers were necessary. In AD 120/121 he may have visited there himself.[220] The bestowal of the *ius nundinarum* on the capital of the *civitas Cananefatium*, Forum Hadriani, has often been connected with this visit, and the river area will presumably have profited from the emperor's general policy to further economic development,[221] and from government expenditure on the troops in particular.

No major events are recorded for most of the 2nd century, which in itself is sufficient proof for an undisturbed and prosperous development. The *limes* system was able to cope with high intensity threats such as those on the Danube frontier,[222] although the effort must have been a considerable drain on the imperial resource potential.[223] Intermediate-level threats, such as the Chaucian raids which affected Lower Germany in the '70s,[224] were also no real problem. But, gradually towards the end of the 2nd century, more troubles arose in the interior as well. An example is the brigand Maternus in Gaul who turned into a usurper so quickly that the implicit popular support has probably correctly been interpreted as a reflection of growing social unrest.[225] An important cause for this was the continuing drain on state finances which may well have been virtually exhausted at the time of the assassination of Commodus in AD 192.[226] This started a civil war, with another 'four emperor year' in AD 193 and ending only in AD 197 with the death of the usurper Clodius Albinus. The events must have affected Gaul and thus, although not directly, implicitly the German provinces as well.

But Severan policy resulted in amelioration of the economic situation,[227] and in active use of force[228] and diplomacy[229] in Germany; moreover there is epigraphical evidence for repairs to the frontier forts.[230] In this same period, in AD 212, Caracalla's famous edict conferred Roman citizenship to all free subjects of the empire, the significance of which will, however, have been quite minimal for most people.[231] More important, as noted by Van Es,[232] is the apparent enlistment of foreign (Frisii and Tuihanti) cavalry in the region north of the river area, evidently client tribes called upon to supplement troops no longer available inside the empire. Such northern tribesmen may well have constituted Caracalla's bodyguard. Their description as Germani[233] is strongly reminiscent of the (largely Batavian) bodyguard of the Julio-Claudian dynasty, but Caracalla's guards apparently spoke no Latin which is hardly conceivable when they came from Lower Germany.

As in so many other provinces, the further history of Germania Inferior in the 3rd century is one of continuing troubles, although the literary evidence is rather scanty. Thus, the probability of large-scale military activites in the Batavian area under Elagabalus, around AD 220, can only be deduced from the presence of the le-

218 Cf. Bogaers 1960–61, 285, note 130. In addition to the cases mentioned in note 217 one may also point to the case of the Batavian *dux* Postumus (discussed below) and to the *dux* Aurelius Ianuarius (chapter 12, note 27). Although the context is different, one might even refer to a remark by Ammianus Marcellinus (XVI 12, 45) about Batavians fighting *cum regibus*, under their own kings, in the battle at Argentoratum in AD 357. This reading is, however, a very doubtful one and it is much more realistic to translate this as 'with the Regii', one of the *legiones comitatenses* (cf. Hoffmann 1969, 68).

219 These were the 30th legion (replacing the 6th) in Xanten and the 1st legion in Bonn, both remaining there until the late-3rd century.

220 *Vita Hadriani* X, 1 and Cassius Dio LXIX, 9.

221 *Vita Hadriani* XX.

222 Cf., e.g., Drinkwater 1983, 64.

223 Cf. Bloemers 1978a, 85, and note 147.

224 *Vita Didii Juliani* I, 6–8.

225 Drinkwater 1983, 79–80.

226 Crawford 1975, 562, note 6.

227 Crawford, *op. cit.*, 566–8.

228 The campaign of Caracalla (Aurelius Victor XXI, 2; Cassius Dio LXXVIII 13, 4). See also Schönberger 1969, 173–4. Unrest beyond and at the frontier may also be deduced from a number of hoards (cf. paragraph 8.4.5 and note 271).

229 Cassius Dio LXXVIII 14.

230 Glasbergen 1947, 305; Bogaers 1976, 230–2; Bloemers 1978a, 94. See also chapter 8, note 272 and below, note 396.

231 Cf., e.g., Drinkwater 1983, 86.

232 Van Es 1981, 46.

233 Cassius Dio LXXIX 6, 1 and Herodianus IV 13, 6.

gionary legates of the 1st and 30th legions in Vechten as revealed by inscriptions.[234] But it is not at all certain that life in the river area was already seriously affected by such events. Even under the serious attack of the Alamanni in AD 233, which brought Severus Alexander to the Upper Rhine where he was murdered by his own troops at Mainz in AD 235,[235] the *limes* held and the invaders were driven back successfully by Maximinus Thrax. Repairs were made,[236] and the new ruler, the first of the 'soldier-emperors', could even entertain the notion of conquering Germany as far as the ocean (the North Sea?).[237]

There are very few direct clues for events in the following decades. Barbarian incursions may have taken place, but much of the older interpretations of what happened after AD 240 can no longer be upheld.[238] Decius seems to have had to cope with a revolt in Gaul as well,[239] but on the other hand administrative measures such as the placing of milestones indicate real political and military efforts in such different areas as the Agri Decumates[240] or the western Dutch river area.[241]

The incessant pressure on the *limes* and the barbarian incursions, in part surely evoked by the withdrawal of troops for civil wars, apparently necessitated Gallienus to move his headquarters to Köln in AD 257.[242] It is under his reign that Franks are mentioned with certainty for the first time,[243] from then on to appear more and more frequently in the sources. As a group these Franks remain somewhat elusive, but it is generally assumed that the name refers to a kind of collective designation of a number of transrhenish tribes such as the Chauci, Amsivarii, Chamavi, and others. The majo of these were undoubtedly rather warlike tribal gro at the periphery of the empire which, by the mid- century, had been subjected for two centuries to a va ing degree of control, direct contact, and – in the gen analytic terms discussed in paragraph 11.2 – informat flow from the Roman state. This led to processes of ternal social change and a more hierarchically organi society.[244] Perhaps to some degree initiated by inte warfare and population movements in Germania Lib but certainly accelerated by the increased recruitmen transrhenish soldiers for the Roman army in the 3rd c tury,[245] these groups were now able to muster eno military force on a permanent basis to be a continu high intensity threat. As shown by the development the first half of the 3rd century the *limes*-based systen forward defence was, in principle, capable of counter such a threat. But it could not cope when sufficient m tary force was lacking, and the use of power was imp sible with adversaries who had a clear perception of true extent of the available forces. Moreover, it could cope with too many major threats in many different s tors of the frontier at the same time, such as posed by Franks and Alamanni, the Goths, or the Persians.

Evidently, although his initial efforts were not at all successful,[246] Gallienus was bound to fail in the end is so well summarized in the judgement passed on reign a century later by Eutropius: *Gallienus ... im rium primum feliciter, mox commode, at ultimum pernic gessit.*[247] The Franks, whom Gallienus initially s ceeded in defeating, must also have invaded and pas

234 *CIL* XIII, 8810 and 8811. The deduction is from Alföldy 1967a, 54–5.

235 Herodianus VI 7, 9 and Zonoras XII, 15.

236 Herodianus VII 2, 6–8. See also Schönberger 1969, 175, for epigraphical evidence.

237 Herodianus VII 2, 9 and *Vita Maximinorum* XIII, 3–4.

238 Notably those cited by Van Es 1981, note 139, which were already, and quite correctly, objected to by Bogaers 1964, 50–1.

239 Eutropius IX, 4, cf. Von Petrikovits 1978, 170 and Drinkwater 1983, 88.

240 *CIL* XIII, 9103, 9111, and 9123; see Schönberger 1969, 176. In this area, on the eastern bank of the Rhine, there is even a milestone (of disputed significance: Schönberger, *op. cit.*, 177–8, Von Petrikovits 1978, 174) dated to AD 269/70 and presumably evidence of a display of loyalty to the Gallic empire.

241 *L'Année Epigraphique* 1965, 118. See Bogaers 1964 Bloemers 1978a, 85: league-stone from Rijswijk dated to 250.

242 Zosimus I 30 and 38; see Von Petrikovits 1978, 171.

243 De Boone 1954, 13–4: there is every reason to beli that the action against the Franks by the later emperor Aure nus, tribune of the 6th legion at Mainz, recorded in the *Hi ria Augusta, vita Aureliani* VII, 1, did not take place bel Gallienus.

244 For an overview, see e.g. Steuer 1982, chapters 5 and

245 An early example are the Frisii and Tuihantes (in *Cuneus Frisiorum*) and the Germanic *Numerus Hnaudifridi* Hadrian's wall (e.g., Frere 1978, 208, 212; compare chapter note 92).

246 For overviews, see e.g. De Boone 1954, 32 f. or Von trikovits 1978, 171–5.

247 Eutropius IX, 8: Gallienus ... administered the emp at first with luck, later bearable, and in the end calamitously

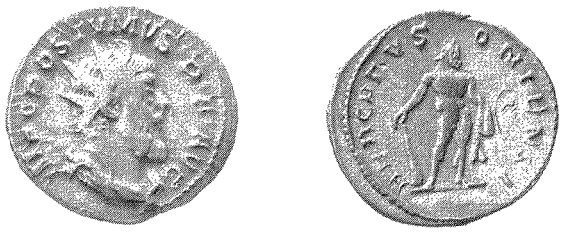


Fig. 131 Antoninianus from Postumus. On the reverse the legend HERC(uli) DEVSONIENSI, his special protector (cf. note 256). Photo Koninklijk Penningkabinet, Den Haag.

through the river area and general conditions of life there cannot have been very favourable. The dramatic invasions of the Franks and Alamanni in AD 259 or 260 and other disasters such as the capture of Gallienus' father Valerianus by the Persians, can only have added to the disarray. Although as a frontier zone the river area must have been harassed to some degree before then, the first major calamity can thus be dated to the 6th decade of the 3rd century.

It is uncertain whether this was also the final one. Presumably in AD 259 Latinius Postumus,[248] a *dux* or army commander (or governor of Lower Germany?) intercepted a raiding party, probably Frankish, carrying valuable plunder. When this was claimed for Gallienus' son in Köln Postumus' troops revolted and proclaimed him emperor. But unlike so many others in those days, Postumus did not become a usurper striving for control of the empire. Instead, he formed an independent 'Gallic Empire' in the Gaulish and Germanic provinces, with his residence in Köln, and extended it to Britain and Spain as well. Without the troubles of the whole empire on his shoulders, he was thus able to carry through effective military and administrative reorganizations.

There are a number of very good reasons to assume that Postumus himself (fig. 131) was a Batavian,[249] and the nucleus of his forces may even be identified as Batavians form a reference in which the *rebelles Gallicani* are discussed in the context of the Gaulish empire as a *rebellio Batavica*.[250] He[251] is most likely to be the Batavian *alumnus* under whose reign Batavian lands were settled by Franks.[252] This conclusion by implication shows what must have happened in our study area.

The Frankish invasion which caused Gallienus to come to the Lower Rhine must have caused large-scale disruption in the river area and at least part of its inhabitants to be killed or to flee. But the Batavian 'nation' as such survived as a social unit, as is evident from its later role in the army, although some (other?) people must have been moved, perhaps not very much later under Constantius Chlorus,[253] to new locations in northern France as *laeti*.[254] It is probable that a Batavian population remained in the part of their *civitas* south of the Waal. This can be deduced from the fact that the Franks who were apparently so vigorously repulsed by Constantius Chlorus are explicitly reported to have occupied only the land encompassed by both branches of the Rhine, thus the area between the Waal and Lower Rhine.[255] Further confirmation of this conclusion is provided not only by other, less precise, references to this occupation[256] but also by the same passage which men-

248 For a short account, see Von Petrikovits 1978, 174 f.

249 See the discussion by De Boone 1954, 36.

250 *Panegyrici Latini* VI (VII), 4: *Panegyricus Constantino Augusto*.

251 Not Carausius, who was *Menapiae civis* (cf. Aurelius Victor XXIX, 20).

252 *Panegyrici Latini* VI (VII), 5: *Panegyricus Constantino Augusto*. See De Boone 1954, 36, 42.

253 Cf. De Boone 1954, 61–2.

254 These are recorded in the *Notitia Dignitatum*, *Occ.* XLII; the area of Bayeux-Courtances, Arras, and Condren-Noyon. See also chapter 12, 435.

255 *Panegyrici Latini* VIII (V), 8: *Panegyricus Constantio Caesari*.

256 See De Boone 1954, 15 and 57 f. De Boone (p. 36) also called attention to the fact that Hercules Deusoniensis, the special protector of Postumus, is almost certainly a deity related to the toponym Deuson, *Deusone in regione Francorum* (Hieronymus, *Chron.* 2389 and Cassiodorus, *Chron.* 373), where the *magister peditum* Severus annihilated a large body of Saxon raiders in AD 370 (Ammianus Marcellinus XXVIII, 5 and XXX 7, 8). Perhaps (cf. De Boone) this Deuson is indeed related to the small stream of the Dieze, a tributary of the Meuse in the province of North Brabant, or to the village Diessen there (Stolte 1961, 22), but in any case (cf. also Von Petrikovits 1978, 208) it should be near Toxandria which is in the general neighbourhood of where the 1st-century Texuandri are localized (see fig. 122) and where also the early-Medieval *pagus* Taxandria is situated (cf. Stolte, *op. cit.*, 20). All this again provides a connection with the southern part of the Batavian *civitas* and perhaps even, considering Postumus was a Batavian, an explanation for his apparent devotion to Hercules Deusoniensis.

tions in addition that the Frankish conquest extended further southwards along the coast and included the Scheldt region. The total occupied area thus covered only part of the *civitas Batavorum* but entirely the lands of the Cananefates and Frisiavones (as indicated on figs. 3 and 4 in chapter 1). It is hardly coincidental that these tribes are precisely the only two who are lacking in all late-Roman sources in which all of their neighbours (the Menapii, Nervii, Tungri, Batavi, and also the Frisii from north of the Rhine) are still mentioned.

In how far Postumus was in direct control of events in the river area is difficult to judge. It has been argued that he allowed the Franks to settle along the frontier as his allies, to defend it.[257] For general strategical reasons, this conclusion is in fact inescapable because the Gallic empire could not have survived if those Franks south of the Rhine had been in the van of a movement against it. Moreover, Postumus is explicitly reported to have enlisted many Germans and Franks.[258] Precisely what the balance of power was, and the extent of Postumus' true options, remains, however, obscure.

The Gallic emperor's murder at Köln in AD 268, at the hands of his own troops, ended the administrative, military, and certainly economic conditions that had already been rapidly deteriorating since the mid-3rd century. The Gallic empire existed a few years longer but the recorded events[259] show that neither his successors nor the legitimate Roman emperors in the following years were able to control events or even run a proper administration. In the river area Batavi and Frankish tribes (such as the Chamavi?), must have maintained some kind of uneasy balance.

11.4.2 The Development of a Frontier Zone

Introduction

Between the hard-pressed Germanic Batavians of the mid-1st century AD following Civilis in a native revolt against their oppressors, and the harassed Roman Batavians of the mid-3rd century turning to Postumus, one of their own sons and as emperor of a large part of the western Roman world the last one to protect them to some degree against German invaders, there is a world of difference.[260] The previous paragraph has presented the literary evidence and a number of deductions and interpretations concerning historical developments and underlying processes. But this evidence spans a period of two centuries and specific information about the river area is only available for the first and, to a lesser degree, for the last few decades.

Most of this period can thus indeed be described as 'an age without history'.[261] In principle, this can be taken as a result of the proverbial process where no news is good news, but then it has to be remembered that all the news that has reached us has been transmitted by a few very privileged and very Roman authors. They have left us the general outlines, but what happened in a peaceful and well-controlled border zone and what that meant for the population there can only be investigated by interpretation of the archaeological evidence. And this interpretation should not proceed from a historical, let alone a Roman viewpoint, but from an assessment of what the native and Roman societies were like, the context of their interaction, and the processes thus set in motion.

In this paragraph, a number of such processes will be examined, based on data assembled in previous chapters. It is thus essentially an interpretation of a number of observed changes, resulting from a process of acculturation. However, changes need to be observed before they can be interpreted, and whether they are seen depends on the questions asked as well as on the availability and quality of data. For both these reasons, only a limited number of issues will or can be examined here.

Initial impact of the army

A number of consequences of the establishment of a *limes*-based forward defence system in the mid-1st century AD have already been mentioned above. A major change was, of course, the permanent stationing of troops, normally auxiliaries, in frontier forts along the Rhine. There is little evidence that auxiliary units were still stationed with the legions,[262] although there are also reasons to assume that some troops, and notably cavalry units, did indeed stay with legions.[263] Of these legions, two were stationed in Xanten and one each in Bonn and Neuss.

This is, in fact, a rather curious state of affairs because

257 Van Es 1981, 49.

258 *Historia Augusta, tyranni triginta* VI, 2 and *vita Gallieni* VII, 1.

259 For a summary see Von Petrikovits 1978, 175–8.

260 Plus, of course, a few superficial parallels of which Postumus was undoubtedly aware and which he exploited as political propaganda. Cf. Van Es 1981, 48 and note 144.

261 Drinkwater 1983, chapter 4.

262 Cf. Gechter 1979, 127.

263 Cf. Alföldy 1968, 144–5; the question remains, however, what 'with' means: in the same fortress or in forts close by.

the strategical redeployment evidenced by moving the two legions stationed in Köln to Bonn and Neuss was not effectuated with respect to Xanten. The reason for this is not, as has formerly been suspected,[264] that the Batavians were charged with the defence of 'their' section of the frontier. Most of their units had been sent to Britain and there is enough evidence for auxiliary forts all along the Batavian part of the *limes*. Perhaps this is simply a case of old-fashioned traditional thinking by those in command,[265] and there may have been many legionaries not actually in Xanten but serving in detachments stationed in the river area for various purposes, such as to garrison some of the forts,[266] construction work such as the temple at Elst, or major enterprises such as the *agger* at the Rhine-Waal fork which the commanding officer in Lower Germany, Pompeius Paulinus, had his troops build in AD 55.[267] But this does not explain why one legion was not moved from Xanten further down the Rhine; in fact, it makes it even more difficult to understand.

Part of the reason could be that the cost of stationing and provisioning a whole legion in the delta was judged to be too high. After all, as far as we know everything had to be set up there from scratch, whereas upstream there was already an infrastructure available at the sites mentioned and in Köln even a colony was founded in AD 50. But this cannot have been a compelling reason so that, unless further research in Nijmegen or the river area proves some of the current assumptions wrong,[268] those in charge must have felt that the delta was adequately guarded without a legion. As a matter of fact, we do know that the region was unusually heavily guarded. For example, the installations in Velsen were probably occupied until the early years of Nero's reign,[269] thereby controlling the coastal area in front of the *limes*. Even more important are the indications for a line of forts in the rear, namely, one certain case in Cuijk (site 499) and the two forts in Grinnes and Vada which, at least in AD 70,[270] are described as *cohortium alarumque castra*. As mentioned earlier, Grinnes could be the site at Rossum, where a pre-Flavian fort is possible but uncertain.[271] Vada's location is unknown, but both sites were located at least south of the Waal.

As for the forts on the Rhine, we know that a whole chain of them existed[272] but very little about their sizes or garrisons. Alföldy has calculated that after the known withdrawals under Claudius and Nero there were at least 8 *alae* and 12 *cohortes* left in Germania Inferior on the eve of the revolt.[273] This number should probably be further augmented as far as the cohorts are concerned, in view of the many forts. The maximum number at that time for the *limes* downstream of Xanten is already 19, of which 13 can be considered fairly certain.[274] By contrast, there are only two auxiliary forts (Moers-Asberg and Remagen) plus a fleet-base (Köln-Alteburg) known upstream, where the legions were stationed (perhaps with *auxilia*).

Unfortunately, all this is of little help in establishing the number of troops stationed in the Batavian area. There are simply too many uncertainties to give even remotely reliable estimates, apart from the fact that some units were transferred elsewhere and there were temporary task forces on special duties. With quantification being thus impossible, a discussion of the impact of the troops on social and economic conditions in the region becomes a difficult matter. The limited information about Batavodurum (site 403) shows that trade and industrial activities developed there,[275] as finds and features indicate virtually entirely attributable to military personnel or Gallo-Roman immigrants, or both. While it was un-

264 Cf. Van Es 1981, 100.

265 Cf. the remarks of Ritterling in Stein 1932, 27. The habit of deploying legions in multi-legion camps was only explicitly forbidden by Domitian (Suetonius, *Domitianus* 7, 3).

266 Such as is suspected for the pre-Flavian occupation at Zwammerdam (Haalebos 1977, 87) and repeatedly hinted at for the first fort of Valkenburg (De Weerd 1977). As long as excavations in Nijmegen, and notably on the Kops Plateau (site 417), are lacking it is always possible to situate a legionary detachment there as well, although (Batavian?) auxiliaries would seem to be a better proposal as argued above.

267 Tacitus, *Ann.* XIII, 53. See also chapter 3, p. 52–3.

268 See also paragraph 8.4.1, p. 249 and note 98.

269 Cf. Morel/De Weerd 1980, 477 f. for a rather vague discussion, but with relevant older literature.

270 Tacitus, *Hist.* V, 20.

271 Cf. Bogaers/Rüger 1974, 74 and Haalebos 1976, 203–4.

272 Cf. above, p. 399 and note 150; also note 273 below.

273 Alföldy 1968, 142.

274 These are the following sites: Altkalkar, Nijmegen-Kops Plateau, Cuijk, (Rossum), (Vada), (Duiven-Loowaard), Meinerswijk, (Driel), (Kesteren), (Wijk bij Duurstede/Rijswijk), Vechten, Utrecht, Vleuten-De Meern, Woerden, Bodegraven, Zwammerdam, Alphen a/d Rijn, Leiden-Roomburg, and Valkenburg.

275 *JROB* 1981, 32–3 and 1982, 34–5. A publication by J.H.F. Bloemers is in preparation.

doubtedly the administrative centre of the *civitas*, there is very little evidence that the site was also a true central place in the socio-economic sense of the word. Literary evidence indicates that the Batavian elite lived elsewhere[276] and archaeological evidence shows that there was no widespread use of Roman material which could testify to intensive social and economic contact.[277]

Admittedly, the potential of the current data-set to register such evidence is limited,[278] and the material available at present overaccentuates the true state of affairs, but compared to the evidence for later periods its minimal quantity is nevertheless significant. It is conceivable, as suggested earlier,[279] that this scarcity represents a kind of cultural resentment on the part of the native population. This may be true, but it is also conceivable that the major reason was simply one of supply. The pottery production in Nijmegen and at the sites of forts (such as at Cuijk, site 499) was limited and there is no evidence for an industry capable of supplying the whole *civitas*, if there was indeed a demand. The military was probably supplied from elsewhere and to a certain – and gradually increasing? – degree by local manufacturing, but for widespread use of wheel-turned pottery on native sites there may not have been enough.

Convincing as this may be, it can only be part of the reason, as is shown by the research in Frisian territory north of the Rhine. There, in the region around the Claudian-Neronian fort at Velsen, conditions applying to the eastern river area should be even more clearly visible. In reality, the number of finds from that period in the region is surprisingly high.[280] As Brandt has shown, however, the peculiar composition and condition of the imported pottery from native sites indicates a particular, selective, process of dispersal. His interpretation, in the context of native exchange patterns, offers an intriguing model. But it is not relevant for the present discussion because Batavians evidently, and understandably, had little use for Roman potsherds. But the evidence does show that already under Claudius there was a very di ferent handling of Roman artefacts in a native conte beyond and within the frontier, which is true even Brandt's model itself should not be appropriate:[281] any case the Frisii were keen on having material whi the Batavi cared much less about.

To return to the river area: the provision of the troo there with equipment and pottery cannot be assumed have been a real problem even in the absence of a deve oped local industry. They brought their own gear wi them on arrival and could obtain many materials (e.g for building the earth-and-timber forts) locally, the vo ume of whatever was needed to replace or replenish the stocks being relatively small. As has repeatedly bee demonstrated,[282] it could easily be transported as a rel tively unimportant addition to other, more bulky ca goes. Apart from the still limited transport of sto building material in this period,[283] these can only ha concerned food.

As shown by the evidence for the Flavian period ar later,[284] and thus implicitly for the phase under discu sion, the necessary corn was probably largely importe As mentioned earlier, in the native economy arab farming was second to stock-breeding. Undoubtedl the fertile soils of the stream-ridges in principal pr duced good harvests. But with a traditional economy f cused on stock-breeding and because so many men su denly were absent after AD 43, it is hardly conceivab that the population could have increased arable produ tion even if they so wished. Also, the well-datable poll diagrams of the Kops Plateau and Meinerswijk do n show a strong increase in indications for the expansi of arable land,[285] and are thus evidence in support of th conclusion.

The required total amount of food imports was not ne essarily very large. The quantity would depend on ho many soldiers were involved, but at least some food such as meat and perhaps cheese, must have been avai

276 Cf. Tacitus, *Hist.* V, 23, 3; see also above, p. 398 and note 144.

277 See above, p. 240–1 and 400–1.

278 See chapter 8, p. 232.

279 Above, p. 400–1 and note 165.

280 Brandt 1983, 135.

281 If the interpretation of the evidence itself should not be correct there is also a difference, because then the Frisii apparently had much more Roman pottery than the Batavi, which would provide strong support for the idea that the latter were averse to its use. But there is no reason at the moment to reje the interpretation.

282 E.g., Von Schnurbein 1982, 132–4 or Fulford 1984, 13

283 See also chapter 6, 184 and above, p. 399 and note 155.

284 See below, p. 424 f.

285 Teunissen/Teunissen-van Oorschot 1980; Teunisse a.o. 1985.

able locally.[286] It would also depend on the type of the troops concerned, for it is very likely that the auxiliaries used more elements of the native diet than the legionaries, who were recruited entirely from the Mediterranean in pre-Flavian times.[287] Most of the auxiliary units, though not all of them, were Gaulish or Germanic troops. It is remarkable that an increase in cereal production in the river area can only be associated with legions: the sudden increase in Nijmegen at the time of the Augustan campaigns mentioned earlier, and the increase after the arrival of the 10th legion in the river area as a whole (discussed below). The implicit causal relation in this observation is perhaps more logical than the assumption that no legion was stationed in the river area because there was not enough food locally. Had there been compelling reasons to station a legion there, the food problem would have been solved.

Apart from the troops and their direct needs, the changes in the mid-1st century, perhaps even slightly earlier, may have also led to the expropriation of tribal land. The question of a military territory has already been touched upon in foregoing chapters.[288] As far as archaeological data are concerned, there is no real evidence for the existence of large coherent military territories. As demonstrated in paragraph 6.5.1, in this respect tile-stamps are of no value, certainly not in the river area. The distribution of settlements as such does not provide any clues either, because there are native sites everywhere, even in the two areas which would first come to mind: the banks of the Rhine[289] and the ice-pushed ridges south of Nijmegen.[290] There is one example of a settlement on the Rhine which may have been moved in the mid-1st century, but the reliability of this evidence is very minimal indeed.[291] The lack of sites on the Nijmegen hills can be attributed to the rather infertile soils there and to the deficient state of research as well as to difficulties in surveying the lower-lying areas.[292] The only potentially relevant observation regarding the distribution of settlements is the near absence of villae along the Rhine. Strictly speaking, this can only be observed after AD 70 or, more correctly, in the 2nd century, but its relevance can be extended into pre-Flavian times. Above,[293] the distribution of villae was approached from another angle, namely, their concentration along the Waal and also the Meuse and their apparent relation to larger centres. But their near absence along the Rhine is nevertheless remarkable, even though villae in the vicinity of forts and fortresses are found elsewhere in Germania Inferior and other provinces.[294] Apart from this perhaps not very meaningful observation (see below, note 371), and in the absence of relevant inscriptions or boundary markers,[295] possible evidence for military territories is lacking.

In a too often neglected paper Vittinghoff[296] has made an important contribution to the question of military territories by showing that many aspects of the concept of a *militärisches Nutzland*,[297] a large and coherent area for military use and under military administration, are not as acceptable as sometimes assumed. There are, apparently, no good reasons to assume a military administration,[298] nor large military territories with a permanent status as such. In Vittinghoff's opinion, military and related installations were situated in selected areas on *ager publicus* and used by the army *ohne dass damit zugleich hoheitliche Verwaltungsfunktionen über Personen und von anderen Besitzern genutztes Land eingeschlossen wären*.[299] In principle, a provincial governor or, on his orders, a le-

286 The results of the archaeozoological analysis (cf. chapter 1, note 8) have to be awaited for possible further proof. At least in Velsen, the troops ate beef obtained from the small indigenous species of cattle (Brandt 1983, 135).

287 Cf. Mann 1983, 25.

288 Chapter 6, 190–2 (in the context of a discussion about the significance of military tile-stamps) and chapter 8, 239, 260.

289 As the discussion of population numbers in paragraph 11.3.2 has shown, the banks of the major rivers are the probable location of quite a few missing (eroded) settlements in addition to those surviving (see Appendix 3 or fig. 62). An excavated, fine example of a native site near a fort is Wijk bij Duurstede-De Horden (Van Es 1981, 101–3 and 1982, esp. 153).

290 This area especially after AD 70 and the arrival of the *legio X gemina*.

291 Site 132; see above, p. 239–40. Bloemers (1978a,97) has suggested a similar forced evacuation for a few settlements along the Rhine in the coastal area.

292 See paragraphs 7.2 and 3.2.2 plus fig. 9.

293 See p. 281.

294 A number of instances have been assembled by Von Petrikovits 1979, 232–3.

295 The *cippus* of the *legio X gemina* from Nijmegen (*ER* II, 139) is apparently dubious (Rüger 1968, 53, note 250).

296 Vittinghoff 1974.

297 See Rüger 1968, 51 f., which has been very influential in recent literature, and the important paper by Von Petrikovits 1979. A recent overview of other literature in Sommer 1984, 13–4.

298 See also chapter 8, p. 268 and note 169; compare Von Petrikovits 1979, 236 and 242.

299 Vittinghoff 1974, 117.

gionary or auxiliary commander could always expropriate land for military purposes by a *de facto* measure that could be reversed when no longer needed. No large, coherent, and well-defined areas were necessary.[300]

Despite Vittinghoff's detailed arguments, some of his conclusions perhaps need modification. For example, the striking continuity in the area originally reserved for troops on the Hunerberg in Nijmegen has often been observed.[301] There is much to say for a military area all along the northern edge of the ice-pushed ridge in Nijmegen from the Valkhof area on the Waal over the Hunerberg and Kops Plateau to (after AD 70) the tile-works at the Holdeurn (site 433). But then this is only a small strip of land, some 6 km long, in a formerly (nearly) uninhabited area. It is possible that there was a much larger *territorium legionis* covering the entire area between Nijmegen and Cuijk in the west and Rindern and Qualburg in the east, or a smaller one between Nijmegen and Rindern,[302] but this is pure speculation. As long as a legion was stationed in Nijmegen, an extended area reserved for use as training grounds, such as that from the Xanten legion(s) in Alpen-Veen,[303] can be expected although not necessarily on the Pleistocene hills. The rather infertile hills may have been used by the army as well as natives without the necessity to assume a well-defined military *territorium*: it all depends on the opinion one holds about the true nature of such territories.

For the Holocene area, however, this is a different matter. This was a densely settled region, largely deforested by the end of the Iron Age, and used by the native population for agricultural purposes. Even without Vittinghoff's arguments it is inconceivable that a huge part of these lands, and the entire Betuwe, should be *militärisches Nutzland*.[302] On the other hand, troops in the forts did need grazing land (*prata*), as is testified from va parts of the empire.[304] It is reasonable to expect parts of the stream-ridges along the Rhine and espec the (uninhabitable) flood-basins were indeed ex priated to serve as *prata cohortis*, thereby contributi the Batavian resentment which is evidenced by th volt. The later absence of villae along the Rhine ma an indication of this, when interpreted as evidence such private enterprises could not be established t because the land was already used by the army. Bu only area for which there are reasons to presuppose tary land are the banks of the Rhine where the forts located and the *limes* road must have been constru All we can say about this zone is that it must have at least a few hundred metres wide and definitely enc passed native settlements, which, according to Vitt hoff, would mean that it was not continuous.

Although even this minimal estimate, when true volved hundreds of hectares of agriculturally valu soils and is sure to have upset at least part of the pop tion, the question remains whether it was large eno From a historian's point of view, Vittinghoff has ar that while the army was involved with animals needed grazing lands as well as functionaries to su vise the animals, it did not engage in arable farmir cover its own needs locally.[305] As the pollen evid from the Kops Plateau has shown, this is not ent true even for an Augustan army on campaign, altho much food was imported.[306] But the consumption of tle and quite probably other animals was not necess covered by army production either. On the cont many of the animals are indeed likely to have been tained locally from the native population.[307] A *pequarius*[308] can then be seen as a soldier in charge

300 *Op. cit.*, 113.

301 E.g., Bloemers 1979a, 27. On the other hand, it should be remembered that the discontinuity between the Augustan and Flavian occupation is not yet completely certain. On other sites, such as in Meinerswijk between periods 1 and 2, there may have been continuity. The example from Cuijk (sites 499/500) shows that an originally military area could be used by civilians even though the site as such remained militarily significant as shown by the (assumed) *statio* and the (demonstrated) late-Roman fort. See also Petrikovits 1979, 242, on the civilian use of former forts and fortresses.

302 As in Rüger 1968, 37, fig. 1.

303 In this area, a large number of practice camps have been discovered by air photography and partly excavated. See esp. Scollar/Andrikopoulou-Strack 1984 and Hinz 1984.

304 Rüger, *op. cit.*, 51 f.; Vittinghoff, *op. cit.*, 113 f.; Vor trikovits, *op. cit.*, 230 f.

305 *Op. cit.*, 120: *prata* means only grazing land and no f tionaries involved with arable land are attested.

306 Kops Plateau: above, p. 393.See also below, p. 42 addition, there is a reference perhaps to military arable (*agros vacuos et militum usui sepositos*) by Tacitus (cf. note although the further context refers more to grazing land below), and there is epigraphical evidence in the form of a scription of the *legio I adiutrix* on a plough discovered in Kr. Alzey-Worms (Von Petrikovits 1979, 232).

307 See note 286; the famous – probably Tiberian – de purchase from Tolsum (*ER* II, 396).

308 *CIL* III, 10428.

more limited group of primarily draught-, carry-, or riding-animals, with additional herds of slaughter cattle and other animals depending on local circumstances. There may be vast differences in this respect between different areas or in the same area during different periods. An evaluation of the situation in the river area, and developments therein, may be possible in the near future when the archaeozoological research has been completed.

Apart from the above considerations, there is an additional argument for assuming only limited territories for military use in the Batavian area. This is the fact that we know from literary and epigraphical sources that there was a zone immediately beyond the Rhine which was kept clear of native settlements and reserved for military use.[309] This is even specified, namely, the apparently rather extensive use for grazing *pecora et armenta militum*, the flocks and herds of the soldiers. This is confirmed by the inscription from Menden, which records the extension of the *prata* of the 1st legion beyond the Rhine, perhaps as late as the first half of the 3rd century AD.[310] This shows that there is every reason to assume that, throughout the middle-Roman period, a zone beyond the Rhine was maintained undoubtedly for security as well as economic reasons, and also as a way of avoiding too much interference with native tribal property. As pointed out by Von Petrikovits,[311] the extension of the *prata* as such also shows that the zone beyond the Rhine was not a continuous one. Unless one is prepared to accept the unlikely proposition that one *prata* was enlarged at the expense of another unit's land, the conclusion must be that there was ample empty, or rather 'not military', land in between the areas reserved for military use.

That the area in between was not entirely empty is also shown by the few settlements north of the Rhine in our region (see Appendix 3 or fig. 62). The measures recorded by Tacitus (note 309) were directed against whole tribes settling in military lands and do not at all imply that nobody was allowed to settle (or continue to live) there. The people from the military zone beyond the Rhine were apparently considered inhabitants of the empire and in close contact with the army.[312] In addition to evidence for this from elsewhere, the military tile-stamps on two clearly native settlements across the Rhine from the Betuwe should be mentioned in this respect.[313] As mentioned in paragraphs 3.4.2 and 6.5.1, both sites were located along an undoubtedly frequently patrolled land-route to the north. But they are not military sites themselves.

From the little and usually indirect evidence there is about land for military use, we may conclude that from about the mid-1st century onwards this included at least a small, not necessarily entirely continuous, strip of land along the Rhine, presumably a few limited parcels around existing forts, and probably somewhat larger areas north of the river. At least after AD 70 the (small?) territory on the Pleistocene hills at Nijmegen must have been enlarged, conceivably also over some of the Holocene river area and notably the present-day Ooijpolder and Duffelt. But if the treaty with the Batavians was indeed renewed on the old conditions and favourable for them from a Roman viewpoint, this is not very likely. In any case the palynological evidence indicates that after the arrival of the 10th legion the hills were once again cleared and used primarily as grazing land,[314] which shows that there, at least, we may postulate a *territorium legionis* (of unknown size) with some confidence.

Socio-economic structure and development

During the quarter of a century preceding the revolt, economic conditions in the river area remained comparable to the earlier situation as far as the archaeological evidence is concerned. The various and for the population decidedly negative changes discussed earlier, no matter how much pressure on social and economic life they may have caused, cannot have brought about fundamental changes in such a short period of time. This is confirmed by the archaeological, palynological, literary, and other evidence we possess. On the other hand, the revolt itself should not be considered a caesura. Instead, it was a native reaction to pressure which led to a use of force and further, presumably much more conscious and precisely aimed pressure.

As both literary and archaeological data show, the almost complete destruction of the Roman superstructure

309 Tacitus, *Ann.* XIII, 54–55. See also Bloemers 1978a, 97–9.

310 Von Petrikovits, in Bogaers/Rüger 1974, 26–8 and esp. 1979, 239 f.

311 Von Petrikovits 1979, 239–40.

312 Von Petrikovits, *op. cit.*, 240.

313 Sites 1 (Ede-Veldhuizen) and 7 (Bennekom-Achterstraat). See chapter 5, p. 95 and 6, p. 187, fig. 50.

314 Teunissen/Teunissen-van Oorschot 1980, 272.

during the revolt had only a positive effect on that structure: it was rapidly restored, ameliorated, and expanded in a thorough Flavian manner. These measures have already been referred to in paragraph 11.4.1, and are reflected by an ambitious building program that included the new foundation of a *civitas* capital at site 399 in Nijmegen. As mentioned in chapter 8,[315] it is questionable whether this settlement ever developed into a fully fledged town even though it later became a *municipium*. Nevertheless, the limited excavation data, stray finds, and the size of its cemetery (site 398, Hees) indicate a considerable population and size in the 2nd century. The Flavian foundation can be interpreted in direct connection to the reinstatement and perhaps further reinforcement of the Batavian *civitas*. At a supra-regional level, the administrative situation was also normalized by Domitian's formation of the province of Germania Inferior.

As argued earlier, the decline of the native population should also have turned into an upward trend again in this period, although this is very difficult to demonstrate archaeologically. The clear increase in the dispersal of Roman pottery from the late-1st into the 2nd centuries might be taken as an indication,[316] but an interpretation in purely economic terms is much more likely as a main cause of this development. The troops stationed in the river area, and other immigrants, not only contributed to a general increase of people living in the region but undoubtedly also to the growth of the native population. Ironically, the only pertinent literary evidence indicates that soldiers were interested more in handsome young Batavian boys rather than girls,[317] but the practice of soldiers (illegally) having native wives is known to have been widespread and tolerated.[318]

The best evidence for population growth apart from the large cemetery of Ulpia Noviomagus is the number of settlements. In paragraph 11.3.2 it was concluded that for the eastern river area in the mid-1st century (and by implication during approximately a century before the revolt), the number of settlements must have been around 400. For the Middle-Roman Period, when the wide distribution of wheel-turned wares allows a more direct estimate based on archaeological and geological data, a maximum number of about 500 settlements h been inferred in chapter 8.[319] In contrast to all oth phases, the chance of a grave underestimate of t number of settlement sites is not very large in this cas and the original number was, if at all, only slight higher than this maximum estimate. If the avera number of inhabitants per site remained constant and the increase in the number of sites is valid for the enti *civitas*, this would mean that there was a population i crease of about 25 % from the 1st to the 2nd and earl 3rd centuries, discounting soldiers. When the popul tion of Noviomagus and a few other larger settlemen (*vici*, see paragraph 8.4.2) is taken into account, even somewhat higher increase could be acceptable.

The basic assumption of a constant number of inhabi ants per site is, however, very difficult to substantiate i the absence of enough sufficiently analysed excavatio data. The growth of a settlement such as Rijswijk-D Bult[320] might be used to reject the assumption but th site need not at all be a typical one in this respect becaus it developed into a villa. Conversely, it can be argue that the changing economy – to be discussed below – an the relative security could have led to a dispersal of fo merly more concentrated population groups whic would result in more but generally smaller rural settle ments. Indications for a suddenly increasing number c truly isolated farmsteads are, however, lacking. A well investigated site such as the villa settlement at Drute (site 214; see fig. 78) may have housed a few families an at present we can only assume that its total populatio was not very different from an average late-Iron Age o early-Roman site.

On the whole, with an increase in the total number o sites and the accretion of at least some of them, a popula tion growth in the order of magnitude of one-fourth is a acceptable proposal.[321] This would bring the 2nd-to ear ly-3rd-century population of the *civitas Batavorum* t about 50,000 people (in reality less when c. 5500 me were taken permanently into the army) not counting th army units. Based on Alföldy's figures,[322] these ma have amounted to 21,000–22,000 soldiers for the entir province, one-third of whom should have been statione in the present-day Netherlands and for the Batavia

315 See p. 265 f.
316 See chapter 4, fig. 23.
317 Tacitus, *Hist.* IV, 14; see also Much 1967, 213–4.
318 See, e.g., Sommer 1984, 30–3. The practice was legalized by Septimius Severus in AD 197.
319 See paragraph 8.4, p. 246.
320 Bloemers 1978a, 46–54.
321 It is more than that when the inferred population declin in IB is taken into account.

stretch of the *limes*, roughly two-thirds of this, that would mean approximately 5000 men. As fig. 130 shows, there are more than enough forts for this number which agrees with the circumstance that some or probably many of these forts did not house entire or regular-strength cohorts.[323] From the approximately equal number of troops posed by the Batavians and the number of troops stationed in the *civitas* in the 2nd–3rd centuries it may not be deduced that all new personnel was recruited locally. As mentioned in paragraph 11.4.1, Batavians were still being sent to their own national units and they also served in other units elsewhere. But many of them would indeed have served in their own province.[324]

The total number of people living in the *civitas* can thus be estimated at roughly 50,000, while in Flavian times, when the increase had yet to begin, that number was not very much less due to the presence of more troops including the 10th legion at Nijmegen. At that time, the eastern river area was thus by far the most populous and must have profited the most from the Flavian investment program when distrust or resentment did not interfere. Recent estimates for total army costs amount to somewhat over a hundred million denarii a year[325] for the empire as a whole. According to MacMullen's pay figures and Alföldy's unit numbers,[326] it appears that under Domitian a minimum of 10% of this total was spent in Lower Germany, and in the 2nd century still at least 5.5 million denarii. With one legion and the large number of auxiliaries (a total of ten of normal strength?) we can safely assume that under Domitian the Batavian area accounted for one-fourth of the total, which amounts to at least 2.5 million denarii yearly. In the 2nd century, with only the auxiliaries left plus, for some time, a detachment of the Xanten legion in Nijmegen, the army pay spent annually on troops in our area can hardly have been more than 1 million denarii and probably somewhat less than that.

Even though the figures mentioned above are only estimates, they do indicate quite clearly the relative amount of state expenditure and then only for the army. If Hopkins' estimates of the empire's gross tax revenue are correct,[327] the cost of the army was somewhat less than half this revenue. This means that in Flavian times no less than 1% of the entire income of the whole Roman empire was spent in the *civitas Batavorum*, in the 2nd century still around 0.5%, or somewhat less if the tax income increased, which is in fact probable. From the approach used by Fulford for Britain and Hopkins' average tax contribution of 15 sestertii per individual at a 10% tax rate,[328] it follows that in Flavian times the tax product of some 670,000 people was expended in the Batavian area, and later that of 270,000 which is still at least five times its total population.

These figures demonstrate the obvious in that they show that the area did not pay for itself. They are more properly put in context at a supra-regional or rather imperial level, as was done by Drinkwater who demonstrated that the entire expenditure in the two German provinces is approximately equal to the revenues from the three Gauls.[329] Although this does not prove a true 1:1 relationship because on the one hand wealth was accumulated from the Germanies also and on the other Britain was probably economically dependent throughout the 2nd century,[330] it illustrates the mechanism of tax-exporting and tax-importing provinces already referred to in paragraph 11.2.2. Nevertheless, under average conditions we should expect that as a general principle conscious ef-

322 Alföldy 1968, 156–7 and 162.

323 E.g., Bloemers 1978a, 125. On the other hand, the interpretation of some of the indicated sites as forts is still to varying degrees hypothetical.

324 Alföldy 1968, 104.

325 MacMullen 1984, 572; Hopkins 1980, 117 (the same amount calculated in sestertii). Figure valid for the reigns of Domitian up to Septimius Severus.

326 MacMullen 1984, Appendix, and Alföldy 1968, 151: In the early-Flavian period, 6 *alae* + 2 *cohortes mill.* + 1 *cohors mill. eq.* + 12 *cohortes eq.* + 7 *cohortes quing.* + 4 *legiones* = 6×418 BP (BP = 1 unit 'base pay') + 2×418 BP + 468 BP + 12×234 BP + 7×209 BP + 4×6622 BP = 34,571 BP, at 1 BP = 225 denarii is nearly 7,800,000 denarii. A similar calculation for Domitian's reign results in a figure of 33,541 BP, at 1 BP = 300 denarii is more than 10 million denarii. For the 2nd century, the figure is 18,410 BP of 300 denarii is over 5.5 million denarii. Alföldy's figures are taken to be the minimum estimate of the number of auxiliary units.

327 Hopkins, *op. cit.*, 116 f.

328 Fulford 1984, 130 f. There is much to be said against this approach, and notably the fact that the Batavians are known to have paid no taxes (which must be accepted at the very least for the entire 1st century), but the calculations only serve to put expenses in perspective.

329 Drinkwater 1983, 65. Evidently, this pertains only to the 2nd century and not to the enormous expenses in Flavian times.

330 Cf. Fulford 1984.

forts were made to make areas pay for themselves as far as possible. And other circumstances should have worked in the same direction, such as the technical limitations to the transfer of matter and energy under the then prevailing conditions and also the boost to local economic systems which must have resulted from the input through the army.

As demonstrated above, in our area this input, when army pay is taken as a yardstick, was considerable from the mid-1st century onwards and reached truly gigantic proportions in Flavian times. But in the 2nd century it was reduced to about (very) roughly one-third of the Flavian level and that reduction was quite sudden as well. It occurred within a decade at the beginning of the century and must have had a considerable impact in the region. That this was indeed the case can be deduced to some extent from the analysis of regional developments discussed below. But perhaps there is a much clearer illustration possible, namely, the development of the *civitas* capital Noviomagus.

As argued in chapter 8,[331] the regional centre is in many ways a curious town. Despite the deficient state of research, there are several reasons to suspect that it was an ambitious project in Flavian times, but that it probably lacked a number of facilities (such as an amphitheatre) which should normally have been present. Also, we know for certain that within the perimeter[332] in the first half of the 2nd century there was a pottery industry, which is entirely incompatible with a dense occupation. Also, and even though some tactical advantages are obvious and there could be other reasons,[333] it is nevertheless remarkable that the town was given up around AD 270 in favour of site 403 (Valkhof). Perhaps it is also no coincidence that the excessively wealthy burials recently discovered at the town's cemetery in Hees (site 398) date to the Flavian period.[334] There is nothing even remotely comparable to the amber, rock-crystal, and other precious artefacts from these graves in the huge collection of later finds from the cemetery assembled by Brunsting.[335] While the available evidence does not allow us to reject the identification of site 399 with Noviomagus there is, in the present state of the evidence, every reason to suspect that Flavian investments did not lead to a further development in all respects. Neither Trajan's granting of the *ius nundinarum* nor the later elevation to the status of *municipium*[336] succeeded in bringing about an independent socio-economic development into a complete Roman town and central place for – and supported by – the *civitas*. Military stamps, not only from the 10th legion but also 2nd-century specimens,[337] are evidence for continuing involvement (in whatever form) of provincial authorities. Thus we arrive at the hypothesis that Noviomagus, while started in high spirits under the Flavians, remained 'incomplete' as the result of a significant drop in state expenditure in the region. The implication is that the town was to some extent artificially maintained instead of self-supporting in later times.

That there are indeed reasons to think so can, apart from the aspects mentioned so far, also be deduced from a tentative analysis of the regional settlement system as discussed in an earlier paper,[338] and based on the application of the so-called 'rank-size rule' as developed in geography. Essentially, this is an empirical observation of regularity in settlement-sizes. In its basic form, it states that the size of the nth settlement is $1/n$ the size of the largest settlement in the system. Thus, assuming that the largest settlement has a value of 100 and the system conforms to the rule, then the second settlement would be $100/2 = 50$, the third $100/3 = 33$, and so on. This re-

331 See paragraph 8.4.2, p. 265 f.

332 Excavations have been limited and no wall, ditch, or other bordering structure was known before September 1985, when excavations by the IOGA (U. of Nijmegen) for the first time revealed the existence of a wall and ditch.

333 Bogaers 1979b, 62.

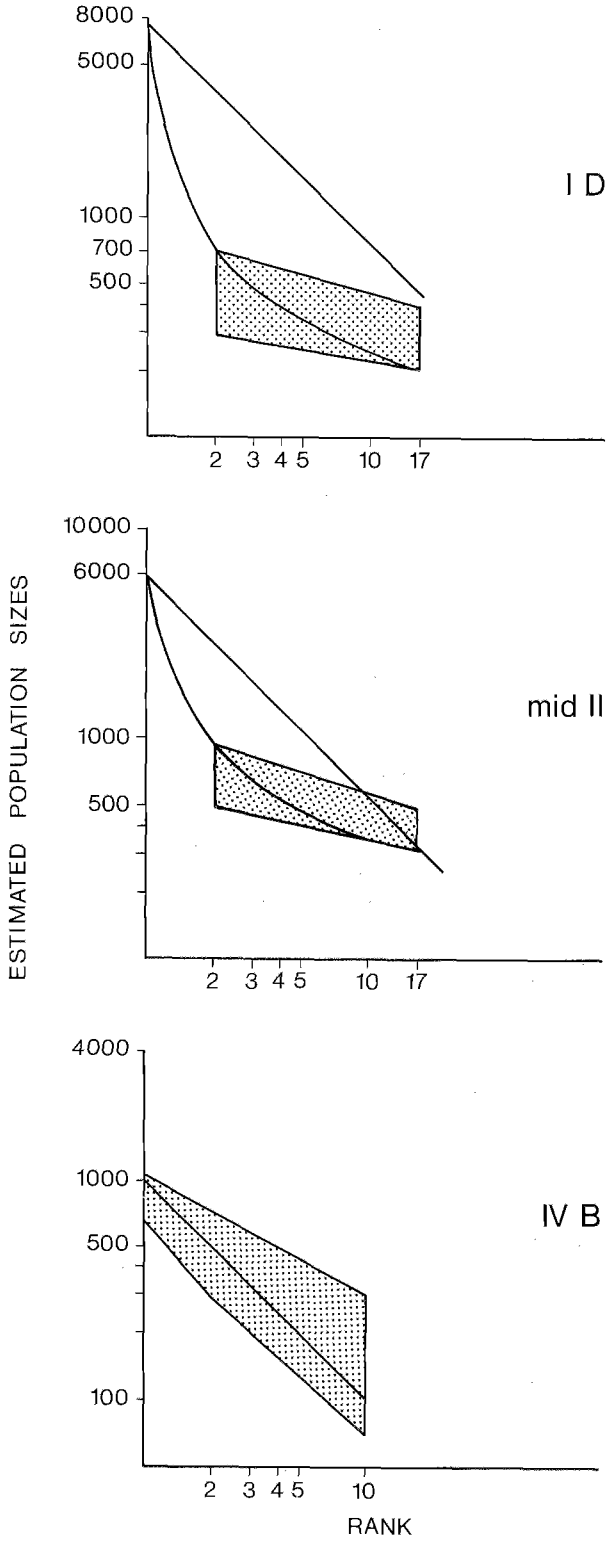
334 Bogaers/Haalebos 1984.

335 Brunsting 1937. A later sarcophagus from site 397 might belong to the cemetery (see above, p. 298) and there is a rather wealthy burial from the early-3rd century as well (Brunsting, *op. cit.*, 184, no. 25), but comparably rich burials are only those from Esch (Van den Hurk 1973, 1975, 1977, and 1980) which date from the later-2nd and early-3rd centuries.

336 Bogaers 1960–61, or 1972a, 312–18. See also note 393. It is perhaps quite significant that of the three or four known town councillors one or two had an unknown occupation while the two remaining individuals were traders. Drinkwater (1983, 199), assuming there was only one such case, a Treveran *decurion*, attributed this to the exceptional situation after the revolt and the short-lived *Imperium Galliarum*. Nevertheless, his conclusion that traders were not normally town councillors remains valid. The evidence for Nijmegen can thus also be taken as an indication of an exceptional situation, showing that the town badly needed such people and that things were not going well early in the 3rd century.

337 See chapter 6, 187–8, fig. 50; also Bogaers 1979b, 59.

338 See Willems 1983a, 114 f. for some additions not repeated here.



◁ Fig. 132 Hypothetical rank-size distributions of settlements in the area of the *civitas Batavorum* during the Flavian period, the mid-2nd, and the later 4th century. The estimated range of variation is shaded.

sults in a lognormal distribution which, if the values on the axes are transformed to a logarithmic scale, appears as a straight line with a gradient of 45° to the horizontal. Several explanations of this regularity have been advanced.[339] Their rationale is based on Simon,[340] who demonstrated that lognormal distributions are the end-product of a large number of stochastic growth processes. This view is criticized by Johnson,[341] who argues that a linear distribution is not necessarily a limiting case (a systemic steady-state) or that, conversely, non-linearity need be indicative of system change as is implied by, for example, Berry's work on economic development and Crumley's hypothesis on urbanization.[342] He suggests that lognormal rank-size distributions are the product of highly integrated settlement sytems, while non-linear distributions are produced by a relatively low degree of integration.

There are two non-linear distributions: a convex and a concave (or 'primate') distribution, the first one having one or more too large settlements while the second is characterized by one dominant, or primate, settlement. Both these distributions are in some way a product of boundary definition.[343] Thus, an almost classical interpretation of a primate distribution is its relation to a colonial system, but this only holds true as long as the system is defined by the boundaries of the colony. The primate settlement is (at a higher level) also part of a larger scale system which may well have a linear or convex rank-size distribution.

A more serious 'cause' of a primate distribution – for analytical purposes – is the deletion of the middle range section (the second, third, and further settlements) from an otherwise linear distribution, either because they have not been located or because they are outside the boundaries of the region under examination. On the other hand, effective control from a centre may have

339 A review is given by Richardson 1973. See also Johnson 1977, 1981.
340 Simon 1955.
341 Johnson 1977, 497 and 1981.
342 Berry 1961; Crumley 1976.
343 See esp. Johnson 1981.

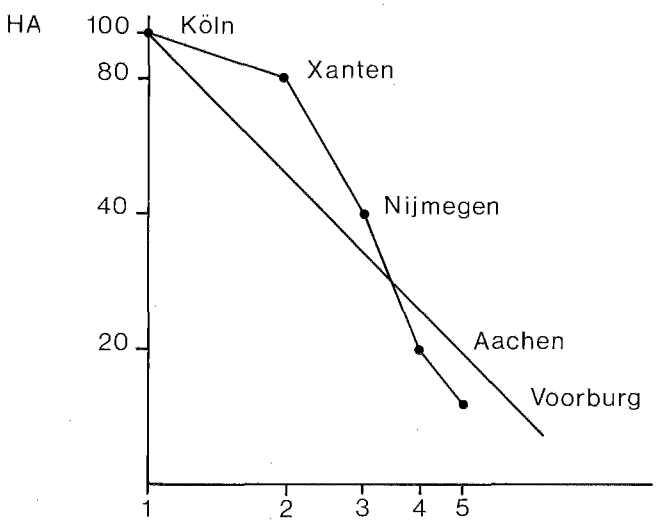


Fig. 133 Rank-size distribution of towns in Germania Inferior in the 2nd century AD. All except Aachen (cf. note 347) are *civitas* capitals.

suppressed the development of such middle-range settlement, a situation which again reminds us of the colonial situation. The resulting primate distribution is normally connected with dendritic settlement systems (fig. 134) in which lower centres are tributary to only one higher level centre.[344]

Estimates of settlement sizes in the Batavian area, whether expressed in hectares or numbers of inhabitants, are very difficult to give as will be evident from the sites discussed in previous chapters. The three curves in fig. 132 can therefore only be regarded as highly tentative,[345] but fortunately that is not a serious impediment to further use. The shape of the curve is determined primarily by the size of the first versus those of the largest other settlements and there is no way in which the primacy of the most relevant curve, that for the mid-2nd century, can be changed into something approaching linearity.[346]

In the 2nd century we are thus dealing with a marked primate settlement distribution. Because there is not the slightest chance of missing middle range settlements these are indeed absent. The primacy of the system could, in principle, result from the fact that Nijmegen was differentially articulated with the larger scale system of the province and thereby with that of the empire, which would have increased its relative size. Interestingly enough, however, the provincial curve as such is neither linear nor concave either (as could have been expected for a frontier province). As fig. 133 shows, it is a convex distribution,[347] indicative of low system integration and as such of the rather artificial nature of the province from a socio-economic point of view. In fact, however, it may well be the result of the pooling of separate systems (constituted by the *civitates*) as described by Johnson.[348] Each of these systems, except perhaps that of the Cananefates with its tiny capital at Voorburg-Forum Hadriani, has a primate distribution even more marked than for the Batavian area.

Another possible approach to understanding the observed primacy is to view it as reflecting 'the political administration of an economy in which competitive forces, necessary to a regular commercial central-place hierarchy, are minimized'.[349] The central place is actively promoted in this case, and the development of middle-range centres effectively suppressed. Trajan's inferred granting of the *ius nundinarum* to Noviomagus, hence Ulpia Noviomagus, could be a splendid illustration of such a process.

344 See Smith 1976a, 34 f. and 1976b.

345 An account for their composition is given in Willems 1983a, 117–21.

346 This holds good even when the composite primate site Nijmegen is redefined as site 399 and a very pessimistic estimate of its population is employed, e.g. 3000 or 4000 (a quite reasonable estimate forwarded by Bogaers 1979b, 62, arrives at 5000 inhabitants). There are still no middle-range sites nearly large enough to fill the gap: without counting soldiers it is very questionable if there is another site even reaching 500 inhabitants.

347 Data on settlement sizes from Bechert 1982, 100 f. Note that the presence or absence of Tongeren, with a size slightly smaller than Xanten and a disputed status as to which province it belongs (Bogaers 1972a, 326 f.), only makes a difference in the *degree* of convexity, not in the shape of the curve. The indicated towns are the *civitas* capitals except for Aachen, whose administrative position is uncertain (see Von Petrikovits 1978, 118, Bechert 1982, 115, 117 f.; also Rüger 1968, 99 f.). Although larger than Voorburg, it was apparently not a chartered town but a *vicus*.

348 Johnson 1981, 167–9.

349 Smith 1976a, 32; cf. Johnson 1981, 173–6.

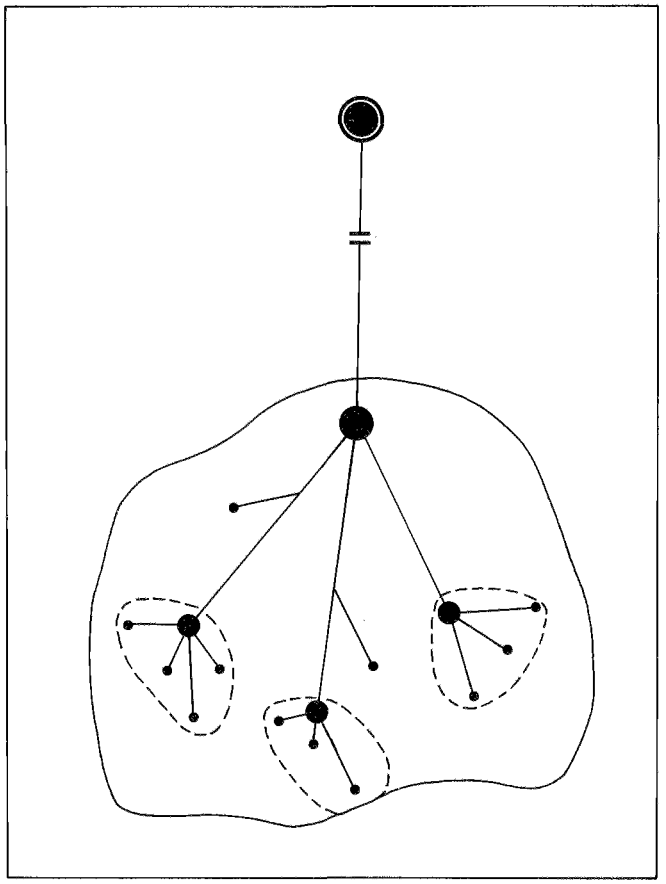


Fig. 134 Model of a dendritic settlement system. After Smith 1976b.

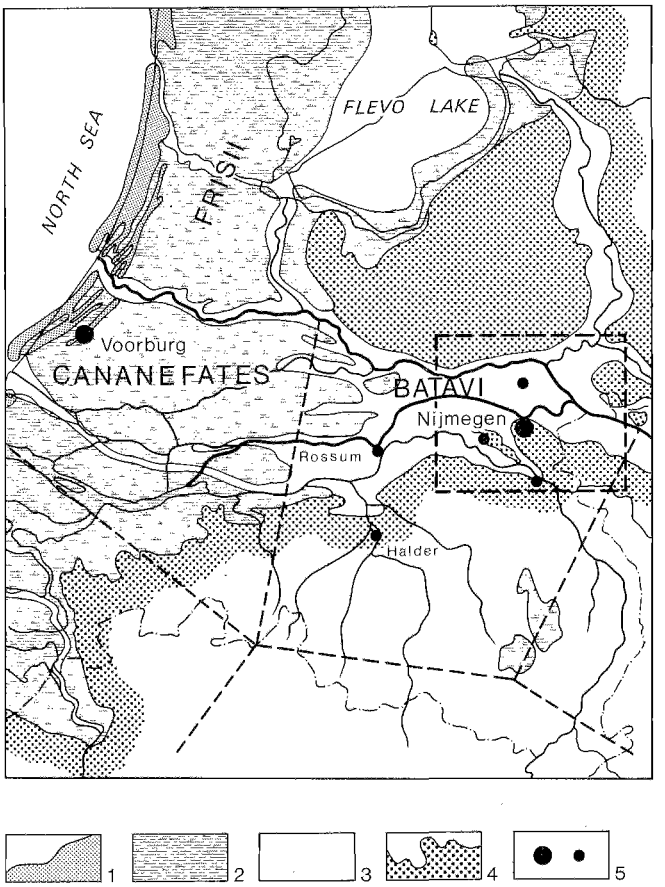


Fig. 135 The administrative structure of the *civitas Batavorum* in the 2nd century AD and geological context: 1 coastal dunes, 2 marine clay deposits and peat, 3 Holocene fluvial deposits, 4 Pleistocene deposits, 5 *civitas* capital and secondary centre (*pagus* capital?). Interrupted lines: theoretical boundaries of the *civitates*, rectangle: eastern river area.

The above discussion has, of course, wider implications than showing the 'artificially maintained' character of Noviomagus or, for that matter of the province. When the provincial rank-size curve is correctly interpreted, Germania Inferior appears not as an integrated socio-economic system but as the product of a decision at the core for administrative, political, and military reasons binding together a number of weakly linked individual systems. The same weak linkage is also a characteristic of the relation between the secondary centres in a dendritic settlement system. A model of such a system is presented in fig. 134, and its characteristics have been analysed by Smith (cf. note 344). Important is especially the flow of goods which is vertical, to and from the primate centre, and only very limited between different areas (horizontal). The central place thus controls prices for rural goods and the distribution of commodities in the countryside, and there is no true competition in service functions. One of the consequences of this system is that rural producers near the primate centre are at a considerable advantage.

As indicated elsewhere,[350] there is evidence for such a dendritic settlement system in the Batavian *civitas*. On fig. 135,[351] the regional centre and a number of possible

350 Willems 1983a, 116.

351 Fig. 135 is part of a slightly improved version of fig. 3 (cf. fig. 123).

secondary centres have been indicated. Those in the area covered by the present study, Elst (site 105), Wijchen (site 315), and Cuijk (site 500) were discussed in chapter 8.[352] They are interpreted as *vici*, and their location in different landscapes and in a half circle around Nijmegen almost by itself suggests an interpretation as secondary centres with their own hinterland. There is thus every reason to consider these different regions as *pagi*. Although there is yet only one certain piece of evidence for a *pagus* in Lower Germany, there is no reason to reject their existence.[353] The relative proximity of the three *vici* to Nijmegen already suggests that the remaining parts of the *civitas* should have had their own centres. One such area is the western part of the *civitas*, bordered by the moors of the perimarine area (Holland peat). Especially because of its central and strategic position the site of Rossum (= Grinnes?), already often mentioned in different contexts above, is a likely candidate for a secondary centre there. Although its hinterland is less of a geographical unit than the others, its position is such that it could have functioned excellently as a central place for the areas north and south of the Waal.

The fifth region is constituted by the Pleistocene sand area south of the Meuse, in fact the drainage basin of the tributaries of the Meuse there. As repeatedly argued above,[354] this region must be considered part of the *civitas Batavorum* and presumably bordered on the Texuandrian area on the plateau around the present-day frontier between Belgium and the Netherlands (see fig. 122).[355] The unmistakable centre for this region is the *vicus* of Halder,[356] in the immediate vicinity of the findspot of the altar dedicated by Flavus (fig. 127).

Especially in Halder, but also in Cuijk and Wijchen, there is evidence for small-scale industries. The altar dedicated to Magusanus Herculis, the temple at Elst, and two smaller Gallo-Roman temples at Cuijk are indications for religious centres. Administrative involvement may be deduced from fragments of a monumental inscription found in Cuijk and mentioning Trajan, and the military stamps found on all sites except Halder could point in the same direction. All of them are, in fact, not just favourably but truly excellently situated in relation to traffic over land or water and obvious locations for such establishments as *stationes beneficiariorum consularis*, *mansiones*, or *mutationes*, or combinations of these.[357] With the possible exception of Elst, none of the presumed *pagus* centres exceeded 10 ha in size.

Although wealthy individuals could, and in fact did,[358] live in all parts of the *civitas*, the dendritic nature of the system may also be deduced from the relative wealth in the region around Nijmegen. It is difficult, as long as recent overviews for the central river area and the Province of Brabant are still lacking, to substantiate this quantitatively. But it is nevertheless a long-recognized fact that the quality and quantity of Roman artefacts is superior in the eastern river area.[359] In addition, there are other data such as the fact that in the 2nd century AD the only region where native pottery may have ceased to be used entirely or at least used to a far lesser extent than elsewhere is the eastern river area.[360] The number of villae is also largest here, and includes the only certain *villa urbana* (site 377, Mook-Kloosterberg I) and the only one with a mosaic (site 239, Ewijk-De Grote Aalst). But the best indication is probably the variety and quantity of Roman artefacts, notably pottery, found also at relatively simple native settlements.

Change

The Batavian area thus appears to have had a little balanced and non-integrated socio-economic system with

352 Above, p. 271.

353 Cf. Rüger 1968, 101–2, or, for Britain, Rivet 1975, 112–3 or Crickmore 1984, 13 (with further references) and 45 f. The inscription from Lower Germany (*ER* II, 33; see Van Es 1981, 235, fig. 180) mentions a *pagus Catual[inus?]*.

354 E.g., p. 371, 396–7, or 410.

355 In principle, it may also have included some of Plinius' (*Nat. Hist.* IV, 106) *Texuandri pluribus nominibus*. The Texuandri as such presumably belonged to the *civitas Tungrorum*.

356 On Halder, see Willems 1977 with further references. The particular kiln discussed in this paper is, by the way, not very likely to be from the mortaria-manufacturer Adiutor as was tentatively suggested. As mentioned by Peacock (1982, 94–5), mortaria are normally produced by a different kind of industry. According to J.E. Bogaers (pers. comm.), the distribution of the stamps points to a more southerly located production centre.

357 Cf. above, p. 261.

358 As evidenced, e.g., by the graves from Esch (see note 335) located several km southwest of Halder.

359 See, e.g., Modderman's (1949, 78–9) conclusions about the difference between Over- and Neder-Betuwe and Bogaers (1955, 185–6) remarks on the subject. The current investigations in the other areas (cf. chapter 1, p. 14) do support the same conclusion (pers. comm. W.J.H. Verwers and C. Blommesteijn).

360 Cf. chapter 6, 179–80 and notes 200 and 201.

distinct elements of artificiality and obvious connotations to colonial systems. It differs clearly from more integrated systems, such as, for example, the often discussed situation in southern Britain, which has been described as a 'transport based' as well as an 'administered' or 'solar' central place system.[361] Both at *civitas* and provincial levels we are dealing with administratively bounded but essentially non-interconnected units, each through its centre connected to one higher level centre. Units can be added or disconnected relatively easy in this situation, as perhaps they were.[362] What determined the organization along the frontier was thus largely the result of administrative and military measures as perceived to be necessary at the core, and from the primary motivation to ensure imperial security and thus to be in complete control.

To reach this goal it is understandable that in economic terms input exceeded the output at a regional level, because at the level of the empire the gains in terms of security were very valuable indeed. On the other hand, the whole complex of developments in the 2nd part of the 1st century did, of course, lead to various and fundamental changes. One important change is that in the rural economy, which, as outlined before, was originally dominated by cattle-breeding. There are several indications that this essentially Iron Age system started to change after AD 70.

One important clue is, of course, the appearance of settlements which can only be described as villae. On the basis of literary evidence, it is sometimes assumed that a kind of villa system with large landowners already existed before the revolt.[363] However, although it appears that Civilis had his home in the Betuwe and undoubtedly, being *praepotens inter Batavos*,[364] that home was an important settlement, the nature of tribal society as discussed earlier is not compatible with such large private landownership. Civilis may have controlled much land and many people through his large *clientela*, but it is difficult to see him leading such a strongly nativistic revolt when he was a Romanized landlord residing in a villa. His residence itself is also not likely to have been very different from others in its material aspects.[365] In addition, the term villa has of course not the same meaning as in modern scientific parlance.[366] Apart from these arguments, there are no settlements classified as villae which are datable to pre-Flavian times.

The evidence on villae as such was presented in chapter 8 and need not be repeated here.[367] Site 214 in Druten (see fig. 78) is a good example of the kind of Romanized settlement with native roots that started to operate in the Flavian period, but there are wealthier and also later founded villae. Although the villa developed further in the early-2nd century, this quick start of apparently rationalized arable production (there are no indications to assume a specialization in stock-breeding in the excavated plans or faunal material) is surprising. It can only be understood when it is realized that there was not just a demand for the products but that the native economy was not yet geared to arable surplus production and that too much compulsion was not very feasible so soon after the revolt. Instead of enforced changes with dubious results, a much more reasonable assumption is that some more Roman-minded – and such people are even attested to during the revolt – and more enterprising types started afresh on a new site. There is every reason to believe that, even if they were not actually induced to it, such individuals surely received official support and encouragement. This may explain the rather strong degree of affiliation with the army observed on these sites,[368] as evidenced by the presence of military tiles or fine wares (fine Nijmegen ware[369]) and quantities of terra sigillata. Although the excavation evidence is not nearly large enough to be quite certain in this respect,[370] there may be a significant difference in the origins of villae, one kind being founded by native entrepreneurs on new sites soon after AD 70 (the 'Druten type') and one resulting

361 See Hodder 1975 and Hingley 1982 for evaluations. Theoretical issues are cogently presented in Smith 1976a.

362 Consider, e.g., the original status, still somewhat uncertain, of the *civitas Cananefatium* in relation to that of the Batavians and the possibility of a later addition of the *civitas Tungrorum* to the province.

363 E.g., Bogaers 1955, 191, and Thompson 1965, 26, note 1, on the basis of Tacitus' (*Hist.* V, 23) mentioning the *agros villasque Civilis*.

364 Tacitus, *Hist.* I, 59.

365 On the kind of settlement to be expected theoretically, see Nash 1984, 101. The lack of differentiation between settlements discussed in chapters 8 and 10 provides archaeological support.

366 A similar use of the term must be the 'villa' of Cruptorix in Frisian territory, where 400 soldiers died during the Frisian revolt of AD 28 (Tacitus, *Ann.* IV, 73).

367 Above, p. 272–81.

368 See esp. the discussion of various classes of artefacts in chapter 6 and also paragraph 8.4.3.

369 Chapter 6, 170–1.

370 Cf. paragraphs 6.5.1, p. 191 and 8.4.2, p. 278.

from more gradual adaptation and acculturation processes and growing out of native settlements in the 2nd century (the 'Rijswijk type').

Although the surface material collected at various villa sites does not allow generalization, we may even go one step further than this on the basis of the Druten and Rijswijk excavations. The fate of the Druten establishment is very clearly linked to that of Nijmegen.[371] It started after AD 70, its owner grew wealthy and rapid improvements in the facilities can be observed which led to its developed form early in the 2nd century. In principle, it may thus have been a one-generation achievement by the original owner. But already after the mid-2nd century there are changes and the site was abandoned before or around AD 200. When viewed as a commercial enterprise dependent on deliveries to the army, this fate is understandable although its early demise remains unexplained.

In contrast, the villae of the Rijswijk type, most of them much more villa-like architecturally than the type site to judge from the materials found on eastern river area sites, were built after some internal developments in native society. Most or all of them were also production sites functioning by means of surplus production in a more or less commercial manner. But especially because of their native roots they can also be seen as status symbols, to display the wealth as well as the 'Romanness' of their owner. Although epigraphical evidence is needed to prove this, it is entirely conceivable that some of these owners acquired their wealth by other means (military or administrative career, trade) than agrarian production.

The above, rather hypothetical, conclusions do indicate that 'changes in the economy' must not be treated simply as economic change but put in the context of a *socio*-economic development. As in every society, there were Batavian entrepreneurs willing to change fast and ca able of successfully doing so. But there was never any r volutionary change in the economy and Batavian lan were never able to provide the necessary food for all t soldiers stationed there. In addition to the degree of o served change, which will be discussed below,[372] oth information clearly shows that at least part of the arm food was always imported. A good illustration of this ir port is provided by a recently excavated Roman barge the former Rhine channel near the fort at Woerden-La rum.[373] The barge must have sunk in about the la quarter of the 2nd century and the remains of its car were found on its floor. This consisted of wheat (prob bly *Triticum dicoccum*) but especially important are t weeds found among the wheat (a.o. the exotic *Orla grandiflora*). These show that the cargo must have cor from a limestone/loess area and therefore in all probab ity from the 'villa belt' stretching from Belgium over t southernmost part of the Netherlands into the Germ Rhineland, but in any case it did not come from the riv area.

A second important clue, which fits into this picture ve well indeed, is provided by the epigraphical evidence f the presence in Nijmegen of M(arcus) Liberius Victor Nervian *negotiator frumentarius*: a private trader, dealii in corn, and originating from Gallia Belgica. He dedica ed an altar there to the Matres Mopates (fig. 136), qui probably to thank them for the safe arrival of a tran port.[374]

As mentioned before,[375] there are several lines of reaso ing and different kinds of data which lead to the concl sion that the native economy was rather strongly direc ed towards stock-breeding. The changed circumstanc from the mid-1st century onwards did not lead to muc economic change but, instead, created a fertile soil for native revolt. But in the Flavian period, the increase

371 One may 'predict' from this, and the following hypothesis, that similar settlements will not be found beyond an economically feasible distance from Nijmegen, because profit had to be made. For the excavation report on Druten, see Hulst 1978.

372 There is no point in calculating theoretical production capacities. As mentioned in paragraph 7.2.2, there was about 300 km² of prime arable land in the river area alone and much more if the lesser quality soils are included. Even taking moderate yield figures (see, e.g., Fulford 1984, 130 for figures and further references), this is more than enough to feed the army in the 2nd century. But what is theoretically possible and what actually happens are two very different things as is also shown by the subrecent and recent land-use data from the river ar (above, p. 221). An illuminating study in this respect is Hass 1981 (esp. chapter 10), in which a distinction is made betwe maximum, critical, and optimum carrying capacity of an ar and the relation to its population level.

373 Data mentioned in Bogaers/Haalebos 1983, 306.

374 *CIL* XIII, 8725 = *ER* II, 191. On the important role *negotiatores* in the two Germanies, see Middleton 1979, 85 and fig. 2; also Von Petrikovits 1978, 143 and Hassall 197 42 f.

375 See chapter 8, p. 225–8 and 284–5, chapter 10, p. 375- above, p.394 and 412.

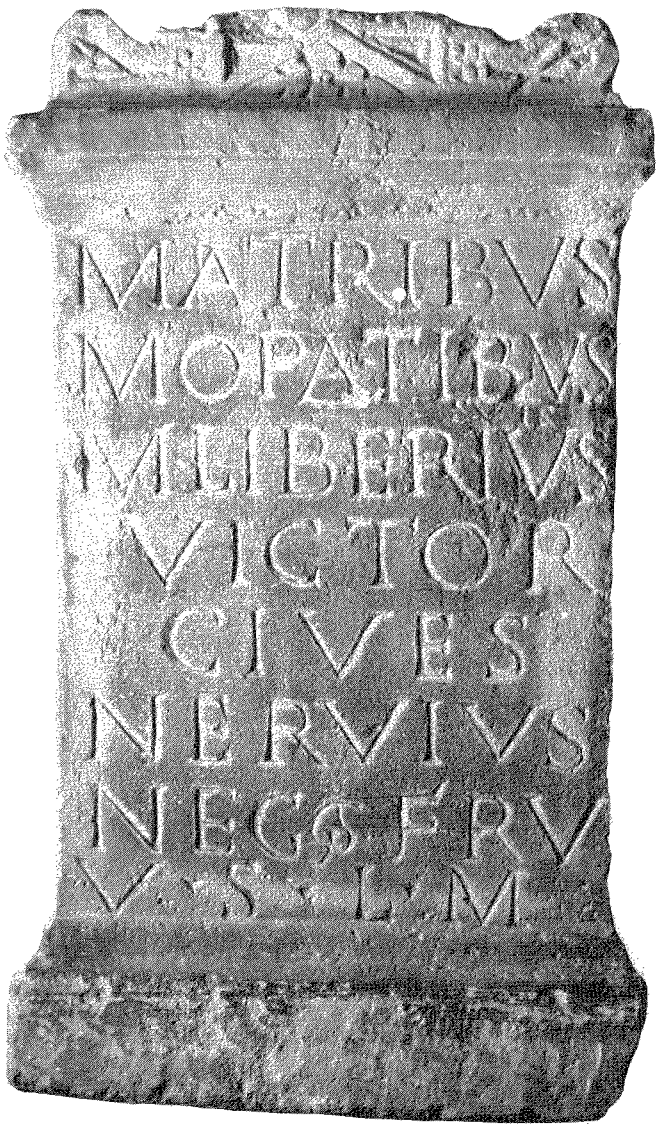


Fig. 136 Altar from Nijmegen (site 399). The text should be read as follows: MATRIBVS / MOPATIBVS / SVIS / M(arcus) LIBERIVS / VICTOR / CIVES / NERVIVS / NEG(otiator) FRV(mentarius) / V(otum) S(olvit) L(ibens) M(erito): 'To his Mastres Mopates Marcus Liberius Victor, Nervian citizen, corn trader, willingly and deservedly fulfilled his vow'.

number of soldiers, the building activities, the imports and trade associated with the army having so many soldiers with money to spend, the undoubtedly increased pressure to maintain and support a smoothly running *civitas* organization, soldiers having alliances with local girls, returned Batavian recruits or foreign soldiers remaining in the area settling as veterans and Roman citizens, and many other interrelated factors started to bring about change. At first, this change is archaeologically almost imperceptible. The scarcity of Flavian, and certainly early-Flavian, artefacts on settlements can be interpreted as an indication of aversion on the part of the native population and as proof of the fact that they had not yet much to offer in return for such artefacts. But already during the Flavian period the inclusion of Roman material in native burials as well as the increased archaeological visibility of graves indicate changes in the native system of norms and values governing disposal-of-the-dead practices.[376] Although a peak is reached only in the 2nd century and even then strong native traditions are still evident, this must be the result of social change. Even in the absence of a proper study on changes in the native burial program both the differences in wealth between graves and the spatial configuration of graves in excavated cemeteries do show that society became far less egalitarian and more differentiated (presumably hierarchically) in the 2nd century.

Despite local entrepreneurs and the obvious opportunities as well as perhaps pressure resulting from the new situation, native society as a whole was thus slow to change. The post-hole swarm type of settlement, and with it the specific stock-breeding economy, did not disappear until the mid-2nd century. Earlier, this was connected to the increase of villae demonstrating ever more arable farming. The relation of both developments is indeed obvious, and in the mid-2nd century the number of villae must have reached its maximum level while the last post-hole swarms were abandoned or, as in Ewijk (site 232), their former sites now occupied by natives living in regular houses with a byre. But this evident relation should not be interpreted in merely economic terms, as one development causing the other.

As mentioned in paragraph 11.3.2, the stock-breeding type of tribal economy is not only a subsistence strategy. It is also a way of life.[377] The role of herds of cattle in some societies as a form of social prestige and a necessary

376 See paragraph 8.4.4.

377 Above, p. 394 and note 116.

element in various kinds of social relations is well known from ethnography.[378] For example, the status of a powerful individual can be related to the number of cattle he owns, fines for deviant behaviour and breaking specific social regulations need to be paid in cattle, and in the usually patrilinear societies a bride-(or 'progeny') price needs to be given to the wife's descent group.[379] These are not commercial transactions, but part of the mutual social obligations between groups. Not surprisingly, the same phenomena such as paying fines in cattle and cattle as a bride-price are also specifically attested for Germanic society.[380] Both anthropological and historical data thus indicate that basic changes in the native economy imply some rather basic changes in society as well. There is, of course, nothing to prove this, but the owner of the Druten villa in its first years may well have been regarded as a kind of Romanized quisling while his later, 2nd-century, counterparts were respected and powerful individuals who, by that time, could use a villa instead of a huge herd as a socially accepted status symbol.

But in the absence of tangible evidence it is not useful to pursue this line of reasoning much further. That the economy did change is, however, a fact which can be observed by other means as well. A very important clue is given in the pollen diagrams. It is very remarkable that the strongest human interference in the natural landscape, as demonstrated by the generalized regional curve for the relation between tree and non-tree pollen (fig. 137), does not occur in the 2nd century, but long before. Deforestation reaches its peak at the very end of the Iron Age. The trend of increasing deforestation during the Iron Age is reversed and forests start growing again, noticeably during the 2nd century and markedly during the 3rd. At first this may seem rather curious, especially when a direct relation is assumed between population density (and human activity) and pressure on the natura vegetation. But the nature of the economy is, of course of major importance.

As mentioned in paragraph 7.2.2, the river area prope consists of vast areas which are very suitable as grazin land. The flood-basins are, in addition, uninhabitabl and totally unsuitable for arable. The Iron Age defores tation can therefore be attributed to the clearance of th stream-ridges for habitation, arable, and pastures wit additional and rather severe reduction of the moi alder-ash forests in the flood-basins by grazing cattl The reduction of herds would then lead to a prompt re turn of these forests with their rapidly growing trees. A was also pointed out by Groenman-van Waateringe in recent paper, normal pollen diagrams give *relative* value which means that an absolute increase and condensing c woods alone is enough to cause the relative decrease c indicators for human activity even if they remain in real ity as numerous as before.[381] This effect is due to the fac that the dispersal of pollen is obstructed by forests an all the more when they are less open. To this can be add ed that a substantial number of diagrams are from sam ples taken in the flood-basins, which are very suitable fo this purpose.

That this explanation is indeed the only one possible i indicated by the archaeological evidence which show that there may have been a population increase of abou 25% into the 2nd century, and certainly not a decline This indicates that as a general conclusion we can sa that while stock-breeding was reduced, arable farmin may well have increased or at the very least remained a the same level from Flavian times onwards. The stream ridges must have been used very intensively, which i rather clearly visible from the distribution of sites as we (Appendix 3; fig. 62). When the explanation for the lac of culture indicators given above is accepted,[382] this ca

378 See also Nash's description of a warrior society and Thompson 1965.

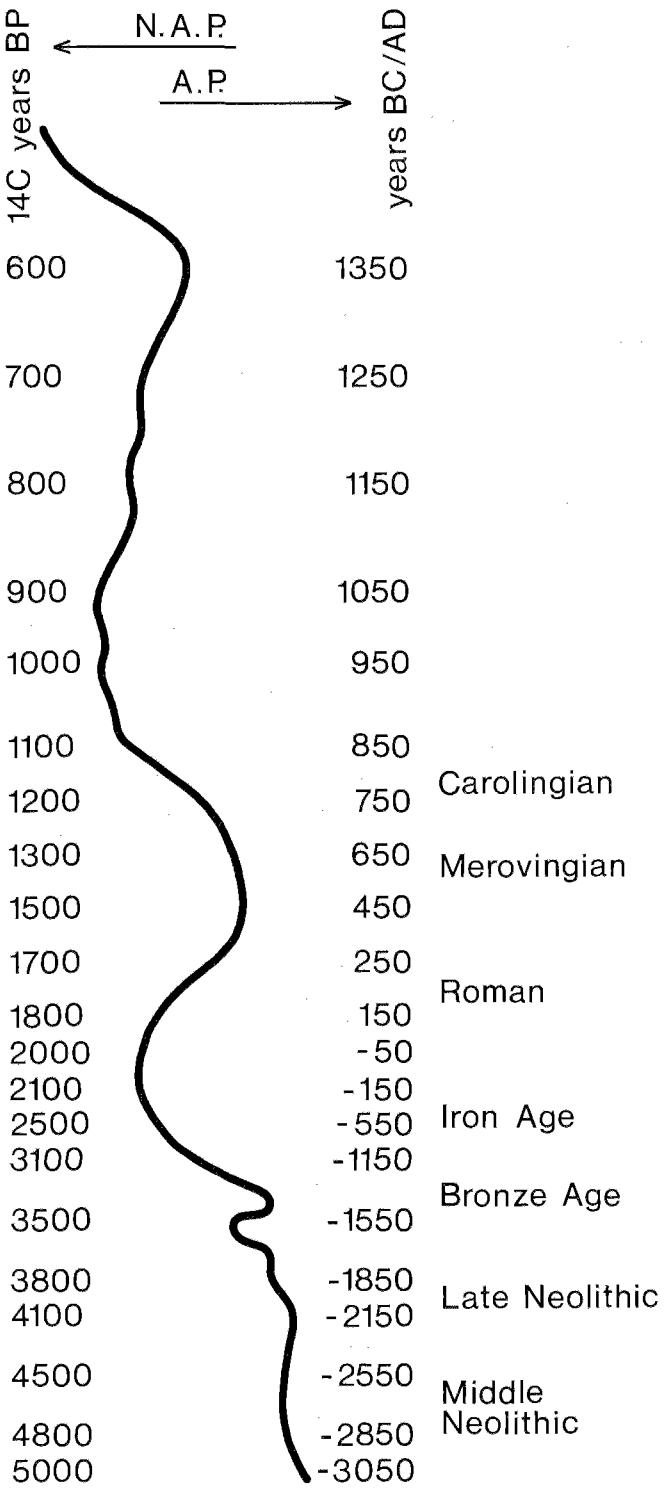
379 See, e.g., Kloos 1972, 67, 165, with further references.

380 Fines: Tacitus, *Germ.* 12 (see also Much 1967, 217–8), bride-price: *Germ.* 18 and Much 1967, 284–5. Much mentions the fact that in Germanic society a girl was literally called 'cattle yielding', cf. the Homeric Greek concept ἀλφεσίβοια. An interesting parallel, although taking the opposite viewpoint, is offered by the Zulu saying that 'cattle beget children' (Kloos, *op. cit.*, 67). Another context is the social or ritual gifts to chieftains mentioned by Tacitus (*Germ.* 15), again in the form of cattle. Also, cattle (and sheep) were used to appease feuds, even homicide (*Germ.* 21).

381 Groenman-Van Waateringe 1981, esp. 369.

382 Teunissen's pollen zone E (note 383) also includes th Late-Roman Period and the earliest Middle Ages, and it i only then that the true (although still relative) minima for th culture indicators are reached. An additional element whic should not be forgotten is the possible influence of the Duin kerke II marine transgression phase. Its influence, especially i the eastern river area, is difficult to evaluate (cf. paragrap 7.2.1), but the flood-basins may have become less suitable fo grazing cattle than before, especially from the late-3rd to 5t centuries.

383 Teunissen 1982, fig. 11, zone E.



◁ Fig. 137 Curve of the relative frequencies of tree (AP) v. non-tree (NAP) pollen, based on C14-dated pollen diagrams from the eastern river area. After Teunissen 1982, fig. 11.

also be deduced from the short characteristic of the regional pollen zone beginning in the 2nd century as given by Teunissen[383]: 'Strongly declining non-tree pollen, rapidly increasing *Alnus*, moderately represented *Fagus* and *Carpinus*, minimum for grain, *Plantago*, and *Rumex*'.

Further developments
and the role of the frontier

As a conclusion from the foregoing arguments, we may picture the Batavian *civitas* in the 2nd century as a peripheral and still rather dependent border area, although socially as well as economically increasingly tied into the whole complex of Gallo-Roman society. The original socio-economic system had, by the middle of the century, changed into a more hierarchically organized society with evident differences in wealth and social standing being expressed in the usual (Gallo-)Roman manner. The *civitas* superstructure, the administrators, traders, artisans, and the like, located in Nijmegen and to some degree in the various local centres in the different parts of the Batavian area, may have been supported by the food produced in the area itself, especially when much (or most) food for the army was imported. The constant and reliable financial input provided by army pay and largely spent in the centres or in and around the forts can be regarded as a major stimulus. It enabled rural producers, and especially those in the eastern river area around Nijmegen, to acquire the whole range of Gallo-Roman artefacts and some of them to grow truly rich.

The people buried at the rural cemetery of Esch,[384] the owner of the Ewijk villa (site 239), and perhaps some epigraphically attested individuals,[385] belonged to this category. The regional centre became the base of wealthy (to judge from the fact that they were *decuriones*) traders,[386] and there is evidence for artisans producing luxury items, such as mirrors.[387] On the whole, however, the evidence assembled in chapter 5 and the available data from Nijmegen do not create the impression of the great prosperity which is so evident for the 2nd and ear-

384 See note 358.
385 See chapter 8, note 186.
386 See chapter 8, notes 158 and 186; also above, note 336.
387 Cf. Lloyd-Morgan 1981, 95.

ly-3rd century northern Belgic *civitates* and also in the southeast of Germania Inferior, in the German Rhineland.[388] In the early-3rd century, *civitates* such as those of the Ubii and Treveri became the scene of feverish economic activity and there is ample archaeological evidence of the great fortunes that must have been made. As observed by many writers, the original input through state expenditure and the very favourable geographical location from a supra-regional perspective, must have boosted the economy to unprecedented heights. While there is evidence that for the empire as a whole taxation in money became increasingly difficult into the 3rd century and the volume of trade diminished,[389] this is not true for the Rhineland which became an exporting region.[390]

Developments in the Dutch river area could well be a reflection of this social and economic florescence, although a rather pale one when viewed in perspective. On a supra-regional scale, the river area may have profited most from the trade with Britain, which also seems to have reached a peak in the early-3rd century.[391] Perhaps in this period the regional economic system became more balanced, in the sense that its superstructure was more directly related to the regional capacity to support it. As suggested earlier, Ulpia Noviomagus seems to have been a town that never quite made it as such. Also, although there are no good reasons to assume that the town itself was abandoned before about AD 260–270,[392] and may even have become a *municipium* only under Caracalla,[393] the finds from its main cemetery at Hees (site 398) show that the number of burials there may have decreased noticeably from the beginning of the 3rd centu onwards.[394] The possibility of an increasingly less p mate and more lognormal regional settlement rank-s distribution, suggested by fig. 132, should also be me tioned. But it should be realized that the assumed diff ent situation of the later-4th century – to be discussed the final chapter – may have been due to the rather su den changes of the late-3rd century and not to a me gradual change starting considerably earlier.

Although we know little about civilian building acti ties[395] or other display of wealth as in the Rhineland, least the military sector was invested in on a considera scale. Mostly during the late-2nd century (e.g., Valke burg 6, Zwammerdam 3, Meinerswijk 5) but perha also slightly later (Utrecht, Vechten),[396] many or all the forts along the Rhine were rebuilt completely stone. Septimius Severus' and later Caracalla's pay- creases to the army,[397] although in view of the inflati and debasement of silver coinage of limited importan may, initially at least, have provided a stimulus to lo economic life.[398] The quite favourable Severan pol towards the military in general is evident from many d ferent indications,[399] and this may have been quite ben ficial for the river area as a whole.

This is because the population must have changed character rather drastically during the seven or eig generations after AD 70. The intimate association w the army is already evident from the fact that so ma young Batavians were enlisted each year, to keep up t level of approximately 5 000 recruits we must assu were continuously in the army. The rather exceptio

388 See, e.g., Von Petrikovits 1978, 81 f., 89 f., 167 f. or Drinkwater 1983, 219 f.

389 See Hopkins 1980, 112–6.

390 Cf. Von Petrikovits 1978, 83, 143–4; Bechert 1982, 187 f. See also note 399 below.

391 See, e.g., Hassall 1978, 44.

392 Cf. Bogaers 1979b, 62.

393 Cf. Bogaers 1960–61, 309–10. The altar from Colijnsplaat dedicated by Hilarus, *d(ecurio) M(unicipii) B(atavorum)*, in AD 227 (Bogaers 1972b), provides the only precise *terminus ante quem*.

394 See Brunsting 1937, 198–202 for a summary. Brunsting interpreted the relatively underrepresented 'Niederbieber' pottery as evidence for a rather early end of the cemetery's use (c. AD 240). When the settlement was abandoned only towards AD 270 (cf. note 392) his alternative, a steadily decreasing use during the 3rd century, is a much more likely interpretation. A similar conclusion was already forwarded in 1937 by H. von Petrikovits in his review of the publication (*BJ* 142, 363–5).

395 Until very recently it was even unknown if Noviomag was ever walled although, by analogy with the late-2nd or e ly-3rd century defences around Forum Hadriani (e.g., Boga 1972a, 326), one could assume it was. See note 332 above.

396 Haalebos (1977, 290) assumes that the renewals w probably not all contemporaneous and part of one large-sc program. See also note 230. Relatively late activities can also deduced from tile-stamps (sites 298 (Wijchen) and 433 (H deurn); see fig. 50) and an inscription (site 422, Berg en D mentioning the title Severiana Alexandriana for the 1st a 30th legions. See also Bogaers 1972.

397 Birley 1981, 43; MacMullen 1984, 572.

398 Cf. note 227.

399 See esp. Von Petrikovits 1978, 90 and p. 407 above. Al it can hardly be a coincidence that all river-barges found so f Kapel Avezaath, Druten, Zwammerdam (cf. Van Es, 19 120), and recently Woerden (Bogaers/Haalebos 1983, 305– with different cargoes, date to the late-2nd and 3rd centuries

fact that at least some of them still served with their own national units stationed elsewhere has already been mentioned. Perhaps it was a special (2nd–3rd century) distinction. But many of them must have served not so far from home, as was the usual practice. In addition, there are the foreign soldiers and, as clearly indicated by epigraphical evidence for legionaries and auxiliaries alike,[400] those who lived long enough to become veterans normally settled in the region where they had served.[401] Moreover, some of the soldiers on active service had wives and children, a practice to some extent officially recognized under Hadrian and legalized by Severus in AD 197.[402]

All this can only have led to an increasing amalgamation of population groups, a classical frontier 'melting-pot' situation, in which by the 3rd century there can only have been very few if any people left without more or less direct relations with the army. Thus, we may confidently assume that the original Batavian warrior tradition was smoothly carried on and transformed (as was the nature of their society and to some extent even the genetic composition of the population) into a 3rd-century tradition of military service with sons following in their fathers' footsteps.[403] The relative prosperity of this militarized frontier population was increased in various ways. For example, if they were not exempted from taxation because they were Batavians, many would have been because of their military status.[404] In the late 2nd and 3rd centuries especially, many or most of the legionary recruits also came from the frontier areas and when conditions became difficult, the system of sons entering their fathers' profession was even made compulsory.[405] The increased demand for soldiers could not, however, be covered entirely from the frontier population, as is evidenced by increased recruitment, especially of Thracians, to the legions as well as auxiliaries. Also, there is the evidence of increased recruiting beyond the Rhine, although the irregular units raised there began to be important on the Lower German *limes* only after the mid-3rd century.[406]

The inferred character of the late-2nd and especially 3rd-century population of the river area can also be deduced from the dispersal of military artefacts in the countryside. To these may be added (apart from the more obvious worship of male gods identified with Roman equivalents such as Mars or Hercules) a number of Germanic warrior goddesses who are particularly characteristic of the area. Most notable among these is Vagdavercustis (and the related Viradegdis or -decdis or -decthis).[407] Others are probably Haeva and Hurstrga.[408] The amalgamation of burial practices mentioned in paragraph 8.4.4 is perhaps also significant in this respect, as well as the observation that typical native cemeteries such as at site 396 (see fig. 69) seem to have already ceased to be used before the end of the 2nd century.[409] A recent study by Gechter on fibula-distributions is of

400 Cf. note 215. The numerous inscriptions assembled by Byvanck 1935, chapters 3 and 4, illustrate this for Nijmegen and the river area.

401 As pointed out by Hopkins (1980, 124), it is often forgotten that the number of veterans after 16 or 25 years of service is quite small, according to his figures only respectively c. 6% and 3% of the men originally enlisted. In view of the age at discharge given by the enlistment at c. 17 years of age (normally perhaps around 20 or even later, see Alföldy 1968, 98–9), plus 25 years of service, and general life expectancy with the additional occupational hazards, this does not seem to be very exaggerated. Mann (1983, 59) estimates the legionary survivors at 'rather less than half'.

402 Edict of Hadrian allowing legionaries' sons to inherit (see Mann 1983, 65, and note 594). Septimius Severus' measure recorded by Herodianus (III 8, 4–5).

403 Soldiers' families are evidenced by the epigraphical record, both for auxiliaries (Alföldy 1968, 201 f., no. 119 = *CIL* XIII, 1892, and 215, no. 162) and legionaries (Mann 1983, 95: *CIL* VI, 3335 and XIII, 8278).

404 On the exemption from taxation of soldiers and veterans, see Birley 1981, 45–6.

405 Cf. Mann 1983, 67: the method presumably applied to auxiliaries as well.

406 Above, p. 407 and note 232; Alföldy 1968, 79 and note 450 on p. 78.

407 See Much 1967, 385 and Von Petrikovits 1978, 157. Vagdavercustis was a goddess of Batavian soldiers and is attested to from sites 69 (*ER* II, 280 = *CIL* XIII, 8805) and 450 (*ER* II, 1513 and 1514 = *CIL* XIII, 8702 and 8703), and the 3rd Batavian cohort in Pannonia (*ER* III, 1141A, see note 217). Also from Altkalkar (*ER* II, 988 = *CIL* XIII, 8662), Köln (*ER* II, 448 = *CIL* XIII, 12057), and Old Penrith (*ER* II, 1516 = *RIB* I, 926). The related Viradegdis a.o. from Vechten (*ER* II, 297 = *CIL* XIII, 8815).

408 Haeva is mentioned on the inscription from site 128, close to the fort at Arnhem-Meinerswijk (*ER* II, 275 = *CIL* XIII, 8705) and could be incorporated in the name of the frontier fort Levefanum (Rijswijk-G.?) which has been interpreted as Haevae Fanum (see Van Es 1984, 281, with further literature); Hurstrga on the altar from Kapel-Avezaath in the Nederbetuwe, south of the presumed forts at Rijswijk and Maurik (*L'Année Epigraphique* 1958, 38; see Bogaers 1960–61, 287–9).

409 See also paragraph 8.3.3 and notes 77–78.

particular interest.[410] By innovative use of the evidence he showed that the low-profile decorative fibulae (see, e.g., fig. 71, 1–3) are characteristic for the population, associated directly with the army, along the frontier and obviously the result of a tendency to personal adornment of a prosperous and distinctive group of people as well as tokens of social status. Although this is more difficult to prove, the absurd footwear of the late 2nd–3rd centuries presumably points to the same thing.[411]

It may be that a substantial part of the frontier population developed into a socially distinct, caste-like, group.[412] In so far as their families did not already own much land, former soldiers surely acquired it. In the 3rd century veterans regularly received their *praemia* in the form of land allotments.[413] This is perhaps another circumstance which may help explain the above-mentioned increasing density and acreage of the river forests at that time, because such people were not very likely to have engaged in ancient Batavian agricultural practices.

Perhaps it is not inappropriate to say that the Dutch river area as a whole remained a kind of reserve for breeding soldiers throughout its entire history as part of the Roman empire. From the fierce barbarian warriors of Augustus' day to the self-perpetuating Romanized soldiers' caste of the 3rd century which remained an important group even in the late-Roman army,[414] literary and epigraphical evidence as well as the archaeological evidence suggest this. The Batavians can, in many respects, not be considered unique. A sedentary militarized population had developed along most of Rome's northern frontiers in the 3rd century. But even at a time when the socio-economic periphery of the 1st and 2nd centuries had developed into an area of central importance, the supra-regional spatial differentiation clearly shows that as far as Lower Germany is concerned most of the action took place in the German Rhineland. The main role of the Rhine-Meuse-Scheldt delta is more that of a reliable shield for Gallia Belgica and an imp and carefully supported channel to and from the F land. Also, the local aristocracy may have been mc terested in pursuing a military career than engag administrative activities or agricultural productio example, they allowed merchants to run their *civi* and the palynological evidence clearly shows that opments in the 2nd century did not lead to a massi ricultural exploitation of the countryside.

The differences between the frontier population an in more southerly areas are thus rather obvious. T the tribute flows and dominance-dependency rel channelled through a native aristocracy owning and acting as patrons for the rural peasant sc brought about a true integration in the social an nomic system of the empire.[416] That there was large difference from the population north of the fr is, of course, evident. There was obviously a great c contact, as evidenced by the archaeological rec through trade, diplomatic relations, and sometime tary action (or, in the opposite direction, r Especially from the 3rd century onwards it wa stimulated through the German soldiers employed army. The more or less gradual change from a egalitarian tribal into a more stratified social org tion is also amply documented by the archaeologic literary record.[418] To name just one example, the opment of a settlement like Wijster shows growth a as the emergence of an elite group, manifest in t creasing importance of one household.[419]

Such settlements were, in fact, very different from inside the empire.[420] They are true villages with cleated and closed structure and it may be argue they represent a spatially bounded combination c ments which is found in a dispersed form south *limes*. Thus, the large and wealthy household mig comparable to a villa of the Rijswijk type with th

410 Gechter 1984.

411 Van Driel-Murray, in press. Especially notable are the bizarre sandals (*op. cit.*, fig. 4) which have been found on some sites (villae, towns) in the hinterland as well (pers. comm. C. van Driel-Murray).

412 Gechter, *op. cit.*, 8. The anthropological notion of caste may indeed fit most aspects of the situation rather well (Kloos 1972, 207): endogamous and normally localized group with inherited membership, associated with a specific profession, and part of a hierarchy of other similar groups.

413 Mann 1983, 67.

414 Cf. chapter 2, 22 and below, chapter 12.

415 Cf. above, note 336.

416 For further theoretical discussion along these line the concept of integration, see Slofstra 1983.

417 An overview of the evidence for the directly re area, the northern Netherlands, is given by Van Es 198 f. For a broader overview, see Kunow 1983 (with extensi erences to most of the relevant modern literature).

418 See, e.g., Steuer 1982.

419 Van Es 1967; Willems/Brandt 1977.

420 See also chapter 8, p. 283–4, and note 205.

rounding farms equivalent to the dispersed native farmsteads.[421] What developed in spatial isolation, through direct input, in the protected environment of the empire can thus be seen as developing in spatial context, through indirectly stimulated internal developments, outside the *limes* as well. This mitigates the observed disparities somewhat and illustrates the effects of the material and information flows across the difference of potential (see also above, note 17) created by the *limes* construction.

Nevertheless, the imposed superstructure and acculturation process south of the Rhine did cause substantial distinctions, and the frontier itself may have been in many respects more of a barrier than is sometimes assumed. Why else should such a dense chain of forts have been built? An interesting conclusion in this respect is that proposed in paragraph 8.4.3, on the basis of the absence of mortaria even immediately north of the Rhine.[422] The absence of a Romanized kitchen there is all the more remarkable when it is considered that settlements so close to the Rhine – few in number as they are – may quite probably have been situated on military land and their occupants formally considered subjects of the empire. In general, the number of ordinary Roman artefacts in normal settlement debris also drops off steeply north of the Rhine.[423] That more precious objects, in different contexts, travelled much further is a fact so obvious that it need not be discussed in the present context. There is a lot to be said about the phenomenon as such,[424] but the items concerned were in no way a part of daily life except as prestige goods for the emerging Germanic elites.

The increasing organizational and information processing capacity of transrhenish societies was undoubtedly an important structural cause (as opposed to other, 'incidental' or 'historical' events) of the increased effectiveness of their traditional raiding operations. As outlined in paragraph 11.4.1, these did affect the river area from the late-2nd century onwards, but without causing any major damage. Two of the three Severan hoards (see note 427!) were in fact found north of the Rhine (sites 22 and 26) and their significance is rather limited. It is quite probable that the early-3rd century saw some raids and, as a result, perhaps quite sizeable campaigns by the army. Such activities may, however, have had a positive rather than a negative impact on the river area in that they enhanced the prestige and conceivably the wealth of its militarized population. Less favourable for that population was, of course, the withdrawal of troops; but the auxiliary units from Germania Inferior do not seem to have been transferred elsewhere, which means that in our area there were probably no major changes.[425]

To what extent the river area was affected by raids and in how far life may have become unpleasant there in the fourth and fifth decades of the 3rd century is impossible to estimate on the basis of the vague archaeological and historical evidence. In the second part of the century the then sharply declining non-tree pollen shows that cultivation of the land in general was now indeed reaching much lower levels than before. Shortly after AD 250, when a league-stone was erected near Rijswijk (cf. note 241), conditions must have deteriorated rapidly and when Gallienus was forced to move his headquarters to Köln in AD 257, the first group of 'Franks' may have been raiding the river area. But the general coin evidence indicates that habitation continued for more than a decade longer,[426] and most of the forts presumably held out to at least c. AD 260, if not c. AD 270.[427] The settlement of Rijswijk must also have been inhabited until approximately the latter date.

421 The argument is derived from Van Es 1982, 152.

422 Above, p. 290; a similar observation has been made for the transrhenish area in the Province of North Holland (Brandt 1983, 138).

423 See, e.g., Van Es/Verlinde 1977 on the poverty of Roman finds in an area actually well within a 60 km radius from the frontier. The numerous finds from the *terp* area on the Frisian coast may be an exception (van Es 1960, 1965–66).

424 See Kunow 1983.

425 See Alföldy 1968, 160–3.

426 Bloemers 1978a, 113 and notes 308–309.

427 Valkenburg may have come to its end in AD 240, but there is no compelling reason to reject c. AD 260 (cf. Bogaers, in Bogaers/Rüger 1974, 40). For Zwammerdam this can actually be demonstrated, as well as for Utrecht and the *civitas* capitals at Voorburg and Nijmegen (Haalebos 1977, 65 f.), and also Vleuten-De Meern (Bogaers/Rüger 1974, 55) and Maurik (Haalebos 1976) have coin evidence for a longer occupation. As noted in the latter paper (p. 203), the Zwammerdam evidence shows that an earlier end of the coin series is not necessarily indicative of an earlier end of the occupation. This observation is strongly supported by numismatical analysis, which has shown the dramatic increase of very debased issues. As King (1981, 89) observed, only reasonably good silver coins may have been saved. In this way, the 'Severan' hoards mentioned in paragraph 8.4.5 could in fact date to the 260s, which definitely makes more sense.

Similar arguments could be forwarded for a sizeable number of sites in the eastern river area (see chapter 4), but surface finds do not constitute hard evidence. But there are, as outlined earlier, good reasons to assume that under Postumus' reign at least the area north of the Waal was settled by Franks and, because he was presumably a prominent member of the Batavian soldiers' caste, this may have been effectuated without too much bloodshed. The fort at Qualburg was probably built in his reign (site 460), while many of the others may still have been held by his countrymen even without pay, simply because it was their only hope of survival. They may also have been actually supported by the Germans and Franks enlisted by Postumus.[428] The Gallic emperor – who of course chose his seat in the metropolis at Köln – must have had other things to look after than the fate of Batavia. But he did succeed in creating a fairly effective military and administrative organization, and the provisioning of strongholds along the Lower Rhine could not have been impossible.

Apart from the coins, indications for Postumus' activities are exceedingly scarce. In a recent article attention was called to the possibility that an official of his court was buried at Veldhoven in the Province of North Brabant (approximately on the southern limit of the Batavian *civitas*, see fig. 135).[429] Two symbols of the Köln-centred Sabazius cult associated with such individuals were found here. The nature of the social or economic circumstances during the Gallic empire cannot, however, be deduced from this. We can take it for granted that the insecurity which must have sharply increased in only a few decades thoroughly shook up the relatively prosperous communities. And it undoubtedly diminished the wealth and prestige of the military caste which ceased to exist in the region, at least in its previous form, and in some instances quite probably physically. That most land was lying waste can, apart from the palynological evidence, also be deduced from the general circumstances; famine, spread of diseases among people packed in cramped quarters or simply suffering from undernourishment, and many other factors must have severely reduced the population both south and north of the Waal. Within a couple of decades, the *civitas Batavorum* as it had developed after two centuries of internal change and external patchwork must have partly ceased to exist – although it evidently did survive, as will be discussed in the next chapter.

428 Cf. notes 257–8 above.

429 Verhoeven/Ector 1984.

12 BATAVIA. THE RIVER AREA FROM TH LATE-3RD TO 6TH CENTURIES

12.1 ROMANS AND FRANKS. HISTORICAL DEVELOP MENTS

12.1.1 The Civitas Batavorum: End and Aftermath

Although obscure in detail, the broad outlines of deve opments in the late-3rd and early-4th centuries can b monitored reasonably well. From the literary evidence may be deduced that the river area suffered from in creased raiding which had a far-reaching impact afte the mid-3rd century and probably led to some Frankis tribes being allowed to settle in the area between Wa and Lower Rhine.[1] For the northwestern provinces as whole it is true that 'while Gallienus left the state to it fate, the Roman empire in the west was saved by Pos tumus',[2] but precisely how this worked out for the nor thern frontier *civitates* is uncertain. The archaeologica and palynological evidence shows decrease in populatio and desertion of settlements during the third quarter c the 3rd century, but the evidence from the frontier fort is very difficult to interpret.

There is evidence that under Postumus a fort was bui at site 460 (Qualburg), occupied by the *numerus Ursa riensium*,[3] and tile-stamps from this unit have also bee found, without a direct archaeological context, at sit 403 (Nijmegen-Valkhof; see fig. 138) and further to th west at Wijk bij Duurstede.[4] But whether or not th frontier forts along the lower Rhine were held until th end of the Gallic empire or deserted in about AD 260 de pends on chance discoveries of a few coins or the lates products of the terra sigillata from Trier and Rhein zabern, and on the interpretation of such finds. Th numismatical evidence from a number of forts an coastal sites, in several cases even with a slight peak fo the Gallic emperors and barbaric imitations, and th Zwammerdam evidence showing that the absence o such coins does not imply an earlier end of the occupa

1 Above, p. 409.

2 Eutropius IX, 11, 1: *Ita Gallieno rem publicam deserente Ro manum imperium in occidente per Postumum ... servatum est.*

3 Von Petrikovits 1937 and 1971, 181 and List 1.

4 Nijmegen: Wynia 1979, 65; Wijk bij Duurstede: *ER* I 1082 = *CIL* XIII, 12507. The latter tile may have originate from the probable frontier fort at Rijswijk-G. (Levefanum? see Van Es 1984. Perhaps a more likely original findspot (se also fig. 143, 9 below) is Maurik, which is also very close t Dorestad.



Fig. 138 Stamp of the [N(umerus)]VRS(ariensium) on a *tegula* from Nijmegen (site 403). Scale 1:1.

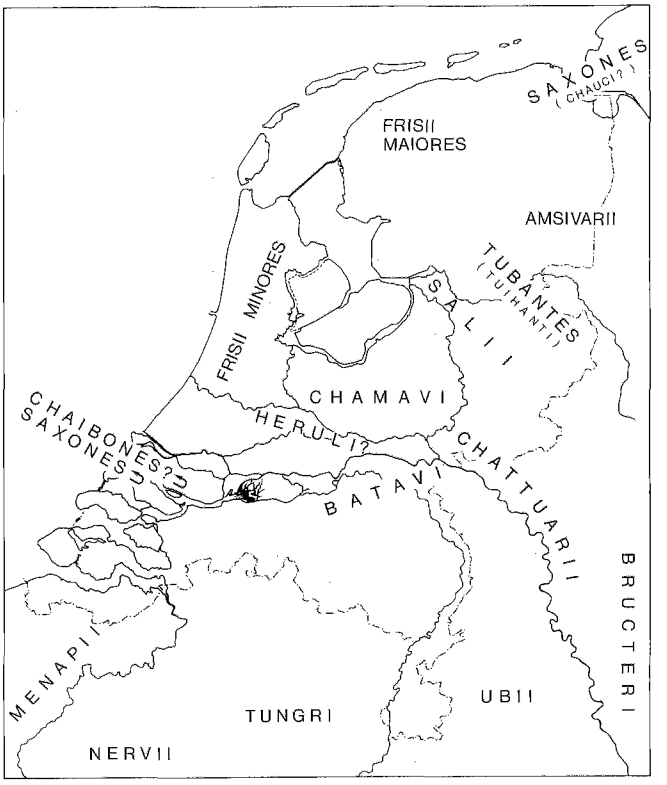


Fig. 139 The approximate location of population groups in the early-4th century.

tion do at least allow the conclusion that it is most probable that the *limes* as a whole was held until the end of the Gallic empire or shortly after.[5]

But already before that time there were Franks south of the Lower Rhine and it is entirely conceivable that the second line, which perhaps always existed along the Waal,[6] grew more important to control these people even if they were officially Postumus' allies.[7] Although a historical date is always problematic in relation to archaeological evidence, it is rather likely that the final abandonment of the military installations as far as they were still functioning should be connected to the large-scale invasions after the death of Aurelianus, in AD 275 and 276.[8] Although the invaders were apparently repulsed again from Gaul by Probus, this did not include the river area as can be deduced from the lack of pertinent references and also the fact that this is explicitly reported for Constantius Chlorus, who cleared Batavia as well as the coastal region from invaders.[9] In reality, this enterprise may have involved more the pacification and at least formal restoration of Roman administration (i.e., of the entire *civitas Batavorum*) than the actual removal of Frankish settlers.[10]

The clearing of Batavia took place in AD 293, on the eve of the campaign against the British usurper Carausius. Of the intervening two decades little is known which pertains directly to our area. Probably in AD 286, Maximianus is reported to have defeated two Germanic tribes, one of them the Heruli.[11] In view of their later association with Batavi in the auxiliary units mentioned in the *Notitia Dignitatum*, there is every reason to assume

5 See Haalebos 1976, esp. 208; see also fig. 141 and, for the interpretation of the coins, King 1981. See also Van Es 1981, 121.

6 See above, p. 411.

7 An example (in addition to Nijmegen?) could be Rossum (Grinnes?), another perhaps nearby Kessel (see *NKNOB* 1976, 189 or *JROB* 1977, 47–8).

8 Cf. Von Petrikovits, in Bogaers/Rüger 1974, 17–8.

9 For an overview of the relevant sources, see De Boone 1954, 47–9 and 57–9.

10 In fact, this is also reported, e.g., *Panegyrici Latini* VI (VII), *Pan. Constantino Augusto* 5,3. On the continued existence of the *civitas*, see also chapter 8, p. 312 and notes 298–300.

11 *Panegyrici Latini* X (II), *Pan. Maximiano Augusto* 7 and XI (III), *Pan. genethliacus Maximiano Augusto* 5.

that this tribe settled in the river area.[12] As noted by Hoffmann,[13] the original unit was not formed much later than AD 290 (and definitely under Diocletian) and may indeed have been raised among the Batavi and Heruli in the river area very shortly after Constantius pacified the region in AD 293. At least from this time on, we must assume that the former *civitas Batavorum* was again part of the reorganized empire under the tetrarchy.

Organizational change

The various reorganizations need not be extensively discussed here.[14] Especially important in the present context is the fact that the province of Germania Inferior was transformed into Germania Secunda, itself part of the *dioecesis Galliarum*,[15] and probably encompassed four administrative units, the *civitates* Agrippinensium, Traianensium, Batavorum, and, as an addition, Tungrorum.[16] Under Diocletianus, the process of changes in taxation was also virtually completed. The taxation in money, which had been an important factor in economic development in earlier centuries[17] and was increasingly replaced by rather irregular requisitions during the 3rd century, was reorganized into a system based largely on taxation in kind. When Germania II was indeed an enlarged successor of the former province, this may well be related to the taxation policy because there is a strong tendency all through the observable reorganizations to create interdependent but as much as possible self-sufficient units at all levels. Thus, the addition of the *civitas* Tungrorum in the rear can be seen as necessary from an economic point of view.

The same can be said when the changed strategy of imperial security is taken into consideration.[18] From G lienus onwards the continuous need to repulse major vasions had already led to the formation of mobile regic al field reserves. Under the tetrarchy these were ma tained only as the relatively small field armies (*comitat* of Diocletian and his colleagues, while the increased s of the army went into the frontier troops still compos of legions with auxiliaries and also cavalry vexillatio The main reform concerned the defence of the fronti and the hinterland. In general, this was not the resto tion of the former *limes* connected with a strategy of f ward defence but the creation of a deeper zone of defe ce more resilient to major attacks. This included t building of heavily fortified strongholds at easily c fensible and optimally situated locations for commu cation and traffic, road forts to block easy access into t hinterland, fortified settlements and granaries, and t like. Settlements thus had to rely on their own defen in time of need, at least for a while, which is another dication for the trend towards self-suffiency.

The defence-in-depth system was, from an organi tional point of view, brought to its logical conclusi under Constantine with the creation of a central fi army of *comitatenses* by removing units from Dioc tian's static border troops, the *limitanei* (then call *ripenses*).[19] This must have occurred before AD 325 a probably goes back to Constantine's campaign agai Maxentius in AD 312.[20] Although there were obviou different developments in various parts of the empire response to local circumstances,[21] the decades around 300 thus saw the effective transformation of a forwa into a defence-in-depth strategy which, at the level

12 From their later role in various sources (for references, see De Boone 1954, 52) it can be concluded that they were not truly exterminated. They are also mentioned twice between the Saxons and Franks. As noted by Hoffmann (1969, 157), the unit was not formed from the *laeti* living in northwestern France in the 4th century.

13 *Op. cit.*, 156–7. The rejection (note 258) of Heruli on the Lower Rhine is unjustified. It does not follow from the dismissal of Schmidt's arguments cited there. Hoffmann does not take De Boone's argument into account (cf. note 12) nor the sources mentioned there.

14 For a concise overview, see Von Petrikovits 1978, 213–22.

15 As indicated in the Verona list (*Nomina provinciarum omnium* 8).

16 Cf. Von Petrikovits 1978, 217. On the debate about the size of Germania Inferior and the *civitas Tungrorum* as a later addition, see Bogaers 1972a, 326 f. As noted in chapter 11 (p. 410) the *civitas* of the Cananefates no longer existed as a res of the late-3rd-century warfare, but presumably also due to undations caused by the Duinkerke II marine transgressi phase (cf. Bloemers 1978a, 113). Perhaps there is literary e dence for this as well, in a passage where the area around t Scheldt and between the branches of the Rhine is described *'paene ... terra non est'*, as 'hardly any land' (*Panegyrici Lat* VIII (V), *Pan. Constantio Caesari* 8, held in AD 297).

17 See e.g. Hopkins 1980.

18 See also Luttwak 1976, ch. 3.

19 The debate on this army reform now seems to have be settled in favour of Van Berchem's conclusions (cf. Luttw 1976, 187–8 or Jones 1973, 97 f., 608 f.

20 Jones 1973, 97.

21 Which is the central theme in Mann's (1979) criticisms Luttwak 1976.

the empire, was a reliable policy to ensure security. For border regions, with increased exposure to barbarian raids, this was not so beneficial and the obvious desire to attain as much protection as possible is undoubtedly the major reason for a return to more preclusive defence systems wherever and whenever possible.[22]

Another element in the overall strategy was, of course, diplomacy based on power whenever possible and, depending on the situation, alternated with the use of force such as in pre-emptive strikes across the frontier or backed by sometimes large payments to local leaders. Apart from the wealth from booty and to some degree trade, this should also have strengthened the position of Germanic leaders and the developing hierarchical structure of Germanic society. Even more important may have been the practice to allow Germans into the empire, to settle depopulated lands as *laeti* or to enlist them in the army,[23] and not from weakness but as deliberate policy. The sources contain many references comparable to the measures taken by Constantius Chlorus mentioned above, indicating that barbarian invaders settling in conquered lands were first defeated and then allowed to stay. In addition, there were groups allowed inside the empire under a treaty and there was the renewal of a client system with groups outside.

Population groups and size

To return to the river area, the historical evidence indicates that from shortly before the beginning of the 4th century, the region must have been under fairly effective control again. At least part of the territory was settled by immigrants, perhaps by Franks who had been there since Postumus' reign and new groups such as the Heruli. What happened to the remainder of the former (Batavian) population is uncertain. It is often assumed that they settled in Gaul as *laeti*, in the regions where they are recorded by the *Notitia Dignitatum*.[24] But that is in fact rather unlikely, and there are two good reasons for rejecting this conclusion. First, *laeti* were normally barbarians established in a large group inside the empire, preferably with a fort nearby, and under military control, supervised by a *praepositus* or *praefectus*.[25] This is also reported for the *laeti Batavi*,[26] but in that case it is rather astonishing. Why would skilled soldiers and Roman citizens be treated in a fashion normally reserved for barbarians, and then quite unnecessarily? Their prominent position is not only evident from what has been said earlier about 3rd-century developments. It is also illustrated for the period under the tetrarchy when once again a Batavian rose to a high position, namely, Aurelius Ianuarius, *vir perfectissimus* who was *dux* of the province Pannonia Superior in AD 303.[27] In addition, we have the evidence from the auxiliary double unit of Batavi and Heruli of whom the latter are nowhere attested as *laeti* and which, as correctly noted by Hoffmann,[28] is therefore very unlikely to have been raised from such groups.

The obvious solution to this problem is to accept that the auxiliaries were raised among the mixed population of the river area. That leaves the *laeti Batavi* unexplained, but there are in fact two possibilities in this respect. First, the *Notitia Dignitatum* was presumably composed around AD 400 and revised for the last time in AD 423, and the settling in Gaul may have been much later than c. AD 300. Second, when a relatively early date of settling is assumed, it is entirely conceivable that the settlers in Gaul were the earlier immigrants in Batavia, for example, the Franks who had settled there under Postumus.

Although far less precise than in the 1st century, the troops raised do also give an impression of population size when it is accepted that they were originally raised in the river area. The *Notitia Dignitatum* lists five Batavian units, two *auxilia palatina* and three *vexillationes palatinae* (cavalry units). For the original enlistment the junior units should be deleted (*Batavi iuniores*) and one of the *vexillationes* is mentioned twice.[30] To the remaining two original units, one infantry and – as was to be expected from Batavians – one cavalry, should be added the *auxilium Herulorum*. The *auxilia* may have num-

22 Cf. Luttwak 1976, 132.

23 For an overview, see MacMullen 1963a.

24 E.g., Jones 1973, 620; locations in *Not. Dig. Occ.* XLII, see ch. 11, note 254.

25 E.g., MacMullen 1963a, 554, Jones 1973, 620.

26 And for two other Roman (!) groups: the Nervii and Lingones.

27 *ER* II, 1329 = *CIL* III, 10981. See Waas 1965, 103. As shown by that study (p. 7 f.), barbarians were only able to reach such high positions from Constantine onwards. Aurelius Ianuarius, however, was not a barbarian but a Roman. His position is rather similar to that held by Postumus before his usurpation.

28 Cf. note 12.

29 Hoffmann 1969; Jones 1973, Appendix 2.

30 See Hoffmann, *op. cit.*, 98 (later date of *iuniores*) and 26–7 and 119–20 (double mentioning of the *equites Batavi iuniores* who were for some time stationed in the east).

bered between 600 and 800 men, a *vexillatio* about 500,[31] which would bring the total number of men drafted to approximately 1900. This amounts to roughly one-third of the troops enlisted in the mid-1st century and, if the relation to population size were more or less constant, indicates a total population of between 10,000 and 14,000 people,[32] with 6000–9000 of them descendants of the early-3rd-century population.

The reasons why the above calculations could be wrong are so evident that there is no point in discussing them. It can only be remarked here that if the original population still occupied most of the area south of the Waal with the immigrants to the north and west, as was also deduced by De Boone,[33] then the relative sizes of these population groups are consistent with those of their territories. The figures of population size, taking those of the 1st century as a starting-point and taking into account the population growth of 25 % in the 2nd,[34] imply a population decline between c. AD 200 and 300 to a minimum of 12 % and a maximum of 20 % when the immigrants are not counted, 20–28 % when they are. The latter figures compare very well indeed with the reduction in the number of settlements to about 25 % of the former level calculated in chapter 8 for the eastern river area.[35] Although this cannot be used as proof because there are too many assumptions involved in both calculations, the basic independency of both data sets (site counts and number of soldiers drafted) shows that at least the order of magnitude of the observed changes may be correctly estimated. This despite the fact that the 25 % remaining sites is actually a rather vague figure because it is probably an underestimate for the eastern river area but, on the other hand, it refers to the entire 4th century.

Further developments

The continued importance of the river area, even if only from a strategical point of view, is quite evident from the fact that it was apparently necessary to pacify and trol the region before the final blow could be dealt British usurper Allectus in AD 296.[36] At a supra-re level, the continued importance of the whole n western frontier zone as a new centre of socio-ecor development instead of a periphery is apparent fro well as further enhanced by, the fact that the new Constantius Chlorus chose Trier as his seat. After came *augustus* of the west and shortly thereafter wa ceeded by his son Constantinus I, there was appa increased raiding and several invasions, but these dealt with and answered by vigorous retaliation search-and-destroy parties across the frontier. Th river area was probably affected by these events c deduced from the fact that the Chamavi, who live rectly north of the area,[37] were also involved.[38]

The remainder of Constantine's reign seems to been relatively peaceful and, as elsewhere, the n western border was secured by new fortifications, frontier as well as in the hinterland.[39] Presur caused by the trouble after his death, Frankish inva are again reported shortly after AD 340. These stopped, but only by some treaty with Constans.[40] may have included permission to settle, but it i known where or which Franks were involved. A uncertain,[41] but nevertheless a possible interpretat that Salii were involved. These are reported to moved into Batavia before the days of Julianus,[4 had (later?) illegally settled also in Toxandria where were properly subdued by the young *caesar* in AD 3 The uncertainty of this historical reconstruction course, due to the catastrophic events followin usurpation of Magnentius in AD 350. The resulting war depleted the frontier of troops, which the F and Alamanni were not slow to take advantage of. sumably from AD 352 onwards,[44] Franks poured Gaul and conquered many towns including even, 355, the occupation of Köln.

31 Hoffmann, *op. cit.*, 4; Jones, *op. cit.*, 681–2.
32 Cf. chapter 11, p.390 and 395–7.
33 *Op. cit.*, 52.
34 Cf. chapter 11, p. 416.
35 Above, p. 416, below, p. 450–1.
36 That this importance of the river area was realized on the other side of the channel as well could be indicated by the antoniniani of Carausius with Hercules Deuseniensis on the reverse (*RIC* V (II), 800). See chapter 11, note 256.
37 Cf. De Boone 1954, 20, 76, Bloemers 1983, 184–5, and Heidinga 1984, 209 f.
38 *Panegyrici Latini* IV (X), *Pan. Constantino Augusto*
39 Cf. below, p. 445; see Von Petrikovits 1971, 182–4.
40 As can be deduced from Libanius, *Oratio* LIX, 130– other relevant sources see De Boone 1954, 80–2.
41 Cf. Von Petrikovits 1978, 186, 196–7.
42 Zosimus, *Historia Nova* III, 6; De Boone 1954, 82, 9
43 Ammianus Marcellinus XVII 8.
44 See De Boone 1954, 84 f. or Von Petrikovits 1978, for detailed accounts.

These events must have thoroughly disrupted the not always peaceful – but at least relatively stable – situation in the river area that had existed since Constantius Chlorus pacified the region. Julianus, 'the Apostate', recaptured Köln in AD 356. After the battle of Straßburg-Argentoratum against the Alamanni in AD 357, participated in by the Batavi and Heruli (their unit then still in its original and undivided form), the *caesar* apparently needed to campaign in Germania II, the need being demonstrated by his encounter with a Frankish raiding party at the Meuse, far to the south between Jülich and Reims.[45] This was followed by the above-mentioned expedition against the group of Salii in Toxandria, who were probably allowed to stay but on Roman conditions: again an example of an often repeated practice. In the same passage, however, Ammianus also reports that the Chamavi, who had invaded the river area, were partly defeated as well and that a treaty was made obliging them to return across the Rhine.

This difference in treatment has, quite plausibly, been explained by De Boone as the result of another relationship with the Roman empire which could date back to a treaty between the Salii and Constans shortly after AD 340. But this need not be true, because the necessity to achieve security in the river area must have been a powerful motive in itself. After all, neither the *civitas Batavorum* nor even that of the Traianenses can be assumed to have still existed as such.[46] The Chamavi presumably continued to make trouble and Julianus also had logistic problems but in the end they did return home across the Rhine and the free passage along the river was secured.[47] In AD 359 the *caesar* himself accompanied a fleet of 600 corn ships from Britain up the Rhine and a whole program of structural measures was carried out.[48] These involved the restoration of three fortifications along the Meuse and seven along the Rhine, in addition to a number of granaries. The troops received assistance in this from the subdued barbarians who had to supply building material.

Aftermath

It may be concluded that, by AD 360, the entire river area up to the Rhine was again under Roman control, and to an extent which was not even accomplished by Constantius Chlorus.[49] But what remained there as a population is uncertain. The most likely interpretation is that the former inhabitants were largely if not totally replaced by Salii, now living as *foederati* not only in Batavia proper but also in the southern part of the river area (south of the Waal) as well as south of the Meuse (into Toxandria), and thus effectively the entire area of the former *civitas Batavorum*.[50] That was a rather comfortable position compared to that of the people beyond the Rhine who, from the late 350s onwards, had to suffer under a vigorous and remorseless Roman policy. The exploits of the bloodthirsty expeditionary force sent into Chattuarian lands across the Rhine from Xanten in AD 360 are but one example.[51] There are many more,[52] all distinctly horrible, and on top of that came requisitions of material and food as mentioned above,[53] provisions which could no longer be obtained in sufficient quantities in Gaul, at least not by normal methods.[54]

45 Ammianus Marcellinus XVII 2. The obvious location for this is the Constantinian fortification of Maastricht (see Panhuysen 1984, 52–65), but there is as yet no (published) evidence which allows the conclusion that the occupation of the site was interrupted in the mid-4th century as is implied by Ammianus' words.

46 On the Salii, see De Boone 1954, 91 and Böhme 1974, 197; also Hoffmann 1969, 158–9: Salii were enlisted at that time under their own name, the Chamavi not (but see note 57 below). On the end of the two northern *civitates* of Germania II, see above, chapter 8, notes 298 and 300.

47 A detailed discussion of the various references by Julianus, Ammianus Marcellinus, Libanius, Eunapius, and Zosimus in De Boone, *op. cit.*, 89–95.

48 Julianus, *Epistula ad Atheniensis*, p. 280 A; Ammianus Marcellinus XVII 9, 1 and XVIII 2, 3–6.

49 For archaeological evidence see chapter 9 and below, 451–7.

50 See Böhme 1974, 200–1, Van Es 1981, 58–9 and Blok 1979, 18, although strictly speaking there was a *deditio* (Amm. Marc. XVII 8,4). But in view of their role on the frontier, the absence of a *praefectus* and the voluntary surrender, they should be considered *foederati* and not (as Ammianus repeatedly says often unreliable) *dediticii* or *laeti*. The Salii were thus in effect put in very much the same position as the early Batavi some time after Caesar's Gallic war.

51 Ammianus Marcellinus XX 10, 1.

52 On the genocidal activities of Julianus and Constantius, see Ammianus Marcellinus XVI 11,6; 11,9; XVII 1, 4–7; 12, 4–9; 13, 8–15; XVIII 2, 17; 2, 19.

53 Also, e.g., Ammianus Marcellinus XVII 1, 11 and 10, 4; Eutropius, fragm. 12.

54 Ammianus Marcellinus XVII 3. The bulk of the grain was, of course, coming from Britain as is testified by the evidence cited above; but that must have been expensive. Julianus reorganized the tax collection system and obtained a reasonable yield (see Jones 1973, 119–20).

In AD 360 Constantius II demanded troops, among whom the Batavi, for his eastern army. The unit at that time probably still consisted for a considerable part of Batavians, but there is no way of verifying this.[55] By then there were no, or hardly any, Batavians left to recruit from and the later Batavian units, split into *seniores* and *iuniores*, had little or nothing to do with the river area. The troops recruited here – and there is conclusive evidence for that – were Salii, together with other population groups and Chamavian prisoners of war.[56] In AD 358–9, these constituted the double unit of the Tubantes and Salii, which was formed at that time.[57]

The active policy of brutal action across the border was continued by Valentinianus I, in particular against the troublesome Alamanni, and was complemented by diplomatic action, bribing, or the assassination of tribal leaders, all spelled out in detail by Ammianus. In addition, the fortification program was continuously developed further or reinforced, a hardly surprising measure in view of the resentment created beyond the borders. Apparently the seaborne Saxon raids on Britain were accompanied by trouble in the river area,[58] but what happened is unknown. An encompassing building program to increase the strength of the defence-in-depth system in the Rhine and Danube sectors was initiated in AD 369.[59] In general, trouble with the Franks in this period was rather minimal compared to developments elsewhere, and for our area the defensive system did wo out as intended. The Saxon raid which was halted in 370 at Deuson, in the region south of the Meuse occ pied by the Salii,[60] is a good illustration. The invad were stopped by local troops and then they were defe ed – through particularly nasty tactics indicative of t spirit of those days –[61] by troops brought up from t hinterland. A similar event, though more dangerous a damaging due to the effects of the usurpation of Magn Maximus, occurred in AD 388.[62] But again, despite t destruction in Germania II, the system worked, an few years later the Frank Arbogastes, the powerful s preme commander of the western army,[63] followed t usual policy by invading Frankish land (across the Rhi from Köln).

The episode does show, however, that the Franks w able to unite under their kings to form ever larger mies: a phenomenon reflecting also the process of soc change among such groups as the Chamavi, Bructe Amsivarii, and the like. This process was undoubte stimulated by the harsh Roman policy towards its neig bours in the second part of the 4th century, which c only have been highly disruptive for Germanic socie The same development can be observed in the growth a stratified society and the formation of large socio-pol ical units such as the Alamanni, Quadi, Burgundi, Goths. Conversely, Roman society in the northwest t

55 Hoffmann 1969, 81 and ch. 6, note 258 argues against the national character of the unit, but his arguments are based on evidence from the Concordia cemetery of Venezia, datable to AD 394/5 (*op. cit.*, 101). Not surprising, the 7(!) buried soldiers of the *numerus Batavorum* do not show a Batavian origin, although this cannot be excluded either in some cases.

56 Zosimus, *Hist. Nova* III 8, 1, cf. Hoffmann 1969, ch. 6, note 277.

57 *Notitia Dignitatum*, *Occ.* 5. See Hoffmann 1969, 158–9. His explanation for the sudden reappearance of the Tubantes (or Tuihanti) in the sources is, however, very unsatisfactory. In early sources they are mentioned in connection with Tencteri and Usipetes (e.g., Much 1967, 393–4). The name survives in present-day Twente, a region next to present-day Salland which is connected with the Salii, both areas constituting the modern Province of Overijssel. The location of the tribe, which is also attested epigraphically (*cives Tuihanti*, belonging to the *cuneus Frisiorum*, erected an altar at Housesteads: *RIB* I, 1594), is discussed by Bloemers 1983, 185. See also fig. 139. The Tubantes thus probably moved south with the Salii. A second possibility is that they are Zosimus' (III 6, 1–3) somehow mixed up 'Kouadoi', which would be more understandable if this was supposed to mean Chauci instead of, as co monly assumed (Byvanck 1931, 500; De Boone 1954, 93- but also note 619) Chamavi, despite the suggestion Kouadoi Chamavi entailed in Eunapius, fragm. 12.

58 *Panegyrici Latini* II (XIII), *Pan. Theodosio Augusto* with a brief reference to 'what the Rhine or Waal have se (*quae Rhenus aut Vachalis vidit*), confirmed by Ammiar Marcellinus XXVII 8,5 and an inscription from AD 369 which Valentinian is called Francicus Maximus (*CIL* 1175 = *ER* II, 468).

59 Ammianus Marcellinus XXVIII 2, 1. For details, see V Petrikovits 1971, 184 f.

60 Cf. chapter 11, note 256.

61 After a peace treaty the returning invaders were a bushed and slaughtered to the last man. Ammianus Marce nus (XXVIII 5, 1–8) finds it necessary to defend the act, be aware that the commanding general, *magister peditum* Sever was surely a war-criminal by any standard.

62 Gregorius of Tours, *Hist. Francorum* II, 9, citing fror chronicle by Sulpicius Alexander. See De Boone 1954, 110-

63 See Waas 1965, 83–7.

came more Germanized, not only by new settlers but also by the very considerable and steadily increasing role of Germans in the army.[64] In fact, the socio-cultural 'potential difference' (see chapter 11, note 17) across the frontier decreased rapidly during and especially towards the end of the 4th century, to be replaced by more purely political and military differences.

The situation is rather well exemplified by Arbogastes, who was *de facto* ruling the west from AD 388 until he was defeated by Theodosius in AD 394.[65] His role was taken over by Stilicho, under the infant-emperor Honorius who had succeeded his father Theodosius in the west in AD 395. Apparently Stilicho immediately inspected the Rhine sector and concluded a number of treaties. From the various sources it can be inferred that a relatively stable situation was maintained. Relevant for our area is that the Salii were apparently able to cultivate their lands in peace.[66] Presumably as *foederati* they were almost certainly tied into the economic system of Germania II and produced some surplus to meet their obligations, but nothing is known about the area and inferences from the scarce information about more southern regions are very risky indeed. It may be that the relations with Roman society were largely limited to providing troops and to supporting units stationed in the river area in their function of securing river transport, as well as by delivering food to those troops. Transport and trade going through the area are likely to have stimulated the economy to some degree, perhaps also trade with transrhenish groups such as the thoroughly subdued Chamavi (cf. note 47).

When Stilicho withdrew both the field army and also border troops for his – successful – campaign against the Visigoth invaders of Italy under Alaric,[67] everything remained as it was, which testifies to the relatively stable situation. As it turned out, when the Vandals crossed the Rhine at Mainz in AD 406 the resulting invasions and destruction ushered in the end of central Roman authority. But in our area, or for that matter in all of Germania II, everything still remained as it was.[68] None of its towns were sacked, as were so many in Germania I and Belgica and, in fact, even Rome itself. Apparently the Franks, who had been the unsuccessful defenders at Mainz,[69] at peace with their transrhenish counterparts, had become enough of a force themselves to prevent their territory from being invaded and laid waste.

12.1.2 The Frankish Kingdom

The usurper Constantinus III may initially have been helpful in restoring some degree of order, but the same source also clearly indicates that this was not effective and that the population in the northwest rapidly formed independent socio-political units, defending themselves because nobody else could.[70] The fact that Britain went its own way from Constantinus III onwards also meant that the Dutch river area lost much of its strategic value from a supra-regional point of view. In how far the region was affected by events during the remaining years of Honorius' reign is impossible to establish. In any case in the 420s Franks invaded and settled across the Rhine which can only have been in the *civitates Traianensium* and *Agrippinensium*,[71] where they were subdued again by Aetius in AD 428 or 432, but in all probability they remained there as *foederati*.[72]

The further developments in Gaul are not directly relevant here.[73] In the present context it is especially important that the Salii also started to move southwards. In about AD 447,[74] Aetius gained a victory over them, presumably in northern France, where they had moved under their king Chlogio (or Cloio). Only four years later the general had to call upon (a.o.) Franks, 'once Roman soldiers',[75] to help defeat the Huns who had invaded Gaul. The description of the Salian warriors reminds us in some ways (tall, ginger-haired) of that of the Batavians, although their war tactics were different.[76] Apparently Chlogio succeeded in establishing a kingdom reaching as far as the river Somme in those days.[77] It is possible, though wholly uncertain, that the king was

64 Waas 1965, Von Petrikovits 1978, 220 f., 237 f., Bloemers 1983, 188 f.

65 Orosius, *Hist.* VII 35, 12.

66 Claudianus XXI, *De consulatu Stilichonis* I, 222. For an overview of the sources, see De Boone 1954, chapter 7.

67 Claudianus XXVI, *De bello Pollentino sive Gothico*, 421–2.

68 Cf. De Boone 1954, 121, Von Petrikovits 1978, 274.

69 Orosius, *Hist.* VII 40, 3.

70 Zosimus, *Hist. Nova* VI 3, 5, and 10.

71 Prosper, *Epitoma chronicon*, 428; Iordanes, *Getica* 34, 176.

72 Merobaudes, *Pan. in Aëtium* II, 5.

73 For a short overview, see Von Petrikovits 1978, 278 f.

74 Byvanck 1947, 406; the interpretation was confirmed by De Boone 1954, 141 and also by more recent research (cf. Von Petrikovits 1978, 279). The episode is recorded in Sidonius Apollinaris, *Carmen* V, 206–18.

75 Iordanes, *Getica* 36, 191.

76 Sidonius Apollinaris, *Carmen* V, 238 f.

77 Cf. De Boone 1954, 142.

descended from a royal family to which belonged an executed king called Theudemer, son of a certain Richimer, who may have been the otherwise well-known *comes* Richomeres, who was definitely of royal blood.[78]

Whatever the actual situation may have been, it is evident that the social structure of the Salii had developed into a hierarchical organization encompassing a quite sizeable group of people. Chlogio's endeavours can be interpreted as an attempt, and a successful one, to consolidate his political power (and thus implicitly the structure of society) by expansion, in order to create a larger and more viable polity. But the roots of this development must be sought in the former *civitas Batavorum* where the Salii, as allies of the empire and in close contact with Gallo-Roman society in which their own elite undoubtedly participated (even though this cannot be conclusively demonstrated historically), further evolved from rank into stratified society.[79] Around the mid-5th century the changed circumstances then led to the next step, that of state formation.

Of course the Salii were not alone in this. In AD 454, after the murder of Aetius, there were renewed invasions from Frankish groups east of the Rhine into Germania I and Belgica.[80] A few years later they conquered the town of Köln,[81] which became the seat of a Frankish king. This meant the genesis of another polity, that which the Anonymus Ravennas two centuries later calls *Francia Rinensis*.[82] Which of these two kingdoms was in control in the river area is unknown. Nor is it very relevant, because in the second part of the 5th century the kings of both polities, Childeric in Tournay and Sigibert in Köln, were not yet the heads of true states but the dominant rulers among a number of lesser kings, *reges critinos*, who had some degree of independence.[83] Although the truth of this cannot be established, Gregorius of Tours indicates that among the Salii these kings were all descendants of one lineage, the Merovingians.[84]

The river area population, whichever group of Franks this may have been,[85] was in any case a peripheral group again with respect to the people in northwestern France and Belgium or the German Rhineland. Thus, under very different social and political circumstances than before, the late-Iron Age situation, with the delta region as a periphery of a Belgic core area, had returned after more than five centuries.

Epilogue

From an analytical point of view, the turn of the 5th into the 6th century is a convenient end for a discussion centred on developments in the eastern river area in the Roman Period. Contacts with the south were to some degree maintained, but the region around the Nijmegen-Noita of the Anonymus Ravennas (see fig. 140) was at best a rather insignificant and rather depopulated outer zone of the Frankish state which was founded by Childeric's son Clovis (Chlodwig, Chlodowech) through the unification of the various polities along the Rhine and in Gaul.[86] On the other hand, it was not less than that: the eastern river area may be regarded as the northernmost region with ties to the new state. This is shown by the events recorded in connection with a raid of the Geti (Jutes?) in the Dutch delta area in about AD 523, mentioned by Gregorius of Tours[87] and in the Beowulf epic. Under their king Hygelac (Chochilaichus) they traversed Frisian country into the *pagus* of the Chattuarii,[88] which must have been Frankish territory and situated in

78 This is suggested by Gregorius of Tours (II, 9), but quite evidently he did not know (cf. De Boone 1954, 127 and note 912) if or which family ties existed. On Richomeres, who was the uncle of the exiled Arbogastes mentioned above, see Waas 1965, 119–22.

79 See Fried 1967 for these concepts.

80 Sidonius Apollinaris, *Carmen* VII, 372–3.

81 *Liber Historiae Francorum* 8.

82 Cf. Zöllner 1970, 31–6, Ewig 1980, 10.

83 E.g., Ewig 1980, 13, De Boone 1954, 144 f.

84 *Hist. Francorum* II, 9: Zöllner 1970, 37, Ewig 1980, 13. Meroveus was related to Chlogio and father of Childeric.

85 The answer depends on whether one is prepared to assume a whole migration under Chlogio, with all or most Salii moving south (e.g., Blok 1979) and other Franks moving in: Chattuarii from the southeast and Chamavi from the north (see below, note 87). Presumably, however, they still controlled the delta to some extent. In any case Childeric defeated the Saxons on the coast (*Hist. Francorum* II, 19), between the Scheldt and Meuse/Waal. He was himself defeated there by the Goths under Euric, in which context mention is made of 'trembling barbarians on the Waal' (Sidonius Apollinaris, *Epistulae* VII 3, 3).

86 See, e.g., Zöllner 1970, 44 f.

87 *Hist. Francorum* III, 3; also in *Liber Historiae Francorum* 19.

88 Hattuarii, Attoarios, in *Beowulf* Hetware. For further comment, see also Zöllner 1970, 82, Blok 1979, 24–5, Halbertsma 1984, 325–6.

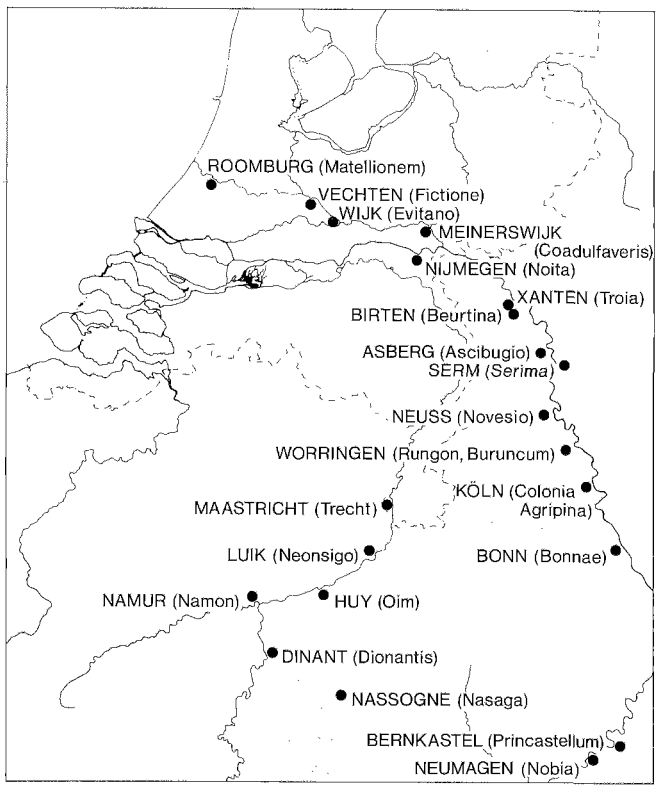


Fig. 140 The *Francia Rinensis* of the Anonymus Ravennas. Largely after Ewig 1980, 11.

the east of the delta.[89] When returning with booty the invaders were attacked by a strong Frankish army under Chlodowech's grandson Theudebert, if the Beowulf is to be believed in coordination with an attack by a *Frescyninge*, a Frisian king.

The event shows at any rate that a nucleus of Frankish influence always remained in the eastern part of the delta, the west being controlled by the Frisii, and that this was important enough to react immediately to a threat such as posed by the Scandinavian raiders. Perhaps a parallel to the late-Roman defence-in-depth system is not entirely out of place in this context, when the Nijmegen region is seen as a first outpost to counter attacks from overseas or, for that matter, from the Frisians.[90] But conditions were, of course, quite different in the 6th century and the Frankish state was a very viable and expanding structure.

The internal developments and those along the northern periphery, which inevitably led to the integration of the whole Dutch delta into the Merovingian/Carolingian empire, are not, however, part of the present study. They represent the start of a new cycle of development in which the river area, from the 6th century into the 7th and at an increased pace after the defeat of the Frisians in AD 720, profited from the then again favourable geographical situation which stimulated economic development and the emergence of new centres.[91]

12.2 BATAVI AND SALII. CHANGES IN THE RIVER AREA

From the previous paragraph it is evident that the various subjects related to the habitation of the eastern river area should be considered for three separate developmental stages. The first stage is the period between the Gallic empire and the invasions of the mid-4th century, which terminated what was left of the *civitas Batavorum*. The second covers the phase in which the area was occupied by Salii, as *foederati* living in relative peace and prosperity in a frontier zone which, though not protected, was effectively controlled up to the northern banks of the Rhine by the central authorities. It is difficult to decide when the third stage began. If the end of effective control from the Roman state is taken as a criterion, AD 406/7 is as good as any other date. The end of Honorius' reign (AD 423), with usurpations and other confusing events in the northwest during which nobody could know what the outcome would be, is a good but not a better alternative.[92] At any rate not much changed

89 Their area is commonly identified as the region immediately to the (south)east of Nijmegen, in the north (or north) of the former *civitas Traianensium*. It is possible that they occupied the area around Noita (fig. 140), with Chamavi to their north and west (e.g., in the Betuwe?).

90 Venantius Fortunatus (in *Carminium*) calls Chilperic 'the terror ... of the Frisians'.

91 Two of these are Dorestad and Meginhardiswich. 'Dorostat' is mentioned by the Anonymus Ravennas (I, 11 and IV, 24), but in his enumeration of towns along the Rhine both places are lacking. Instead, Evitano and Coadulfaveris are mentioned (cf. fig. 140), presumably the former frontier forts Levefanum and Castra Herculis and associated with, if not the administrative nucleus (*castrum*) of both trade centres (cf. Van Es/Verwers 1983, 44 and above, chapter 9.3). For important references to the river area in the Early-Middle Ages, see also Heidinga 1984, chapter 3.

92 Cf. chapters 1, 27 and 4, 77.

initially and the Salii undoubtedly guarded their territory as closely, or even better, as when they were still under Roman supervision. They must gradually (?) have started acting on their own behalf. This led to the expansion southwards and perhaps eventually to the settling of other Franks (Chamavi, Chattuarii) in the region.

Archaeological evidence for such a three-stage development is not lacking, as will appear below. But archaeological evidence for historical reconstructions, welcome as it is, is not the major objective in the present context.[93] It is very unfortunate, therefore, that the level of chronological control is rather restricted where the majority of sites is concerned.[94] There is thus very little opportunity for detailed inferences about most sites or about regional habitation patterns. On the other hand, the information about the region as a whole, incorporating all datable material from all types of sites and not only settlements, does allow more general conclusions about the structure and development of the area. But it is quite obvious from the limitations of the presently available information that one of the main priorities of the ERA-project in the near future must be the investigation of late-Roman/early-Merovingian rural settlements in the river area proper, between Rhine and Meuse.

12.2.1 Ups and Downs until the End of the Batavian Civitas

As argued at the beginning of this chapter, the *limes* may finally have collapsed only in about AD 275. But whether this is true or not, and whether it was a single event or a more gradual abandonment of the frontier forts, is important but not crucial. The evidence as such shows that the Rhine frontier was abandoned at or shortly before a major, historically attested barbarian invasion and the evidence from rural settlements, in the river area as elsewhere, confirms this picture.

In the absence of detailed chronological evidence, little can be said about developments in the region in the following decades. One thing, however, is very clear: ties with the Roman state were virtually non-existent in th period when measured by the only reliable source of i formation, the coin import. It is surely no coinciden that coins from the last quarter of the 3rd century a virtually lacking from the river area as well as from tl rest of the southern Netherlands, the present-day pro inces of Zeeland and North Brabant. In Zeeland curv break off abruptly in AD 273,[95] in North Brabant, as we as in the river area, a few years later.[96] Later coinage u til the end of Diocletian's reign is not entirely absen but it is very scarce indeed.

In principle, this can be taken as an indication for a tot disorder and a lack of administrative control continuir throughout the period of the tetrarchy, its few coins b ing attributed to import during the following perio But there is a good argument against such an interpret tion, namely, the spatial distribution of these coins. Cu rency from between AD 276–306 is available for Nijm gen (site 403), Cuijk (site 499), and Heumensoord (si 391), in a context that allows the conclusion that th were occupied. For sites such as Rossum and Qualbu (site 460) this is less certain, for Asperden (site 466) it unlikely.[97] But there are additional finds, notably alor and south of the Meuse below Cuijk, from such sites Grave, Lith, and Alphen.[98] Although there are, admi tedly, a few finds from the area north of the Waal,[99] tl evidence thus indicates that some degree of control w maintained (or restored after AD 293) in the area south the Waal. Perhaps a better interpretation is that this a plied to the area around a bridgehead constituted by Ni megen and environs, because the relevant finds are a concentrated around Nijmegen, up to Rossum in tl west, to Qualburg in the east, and to the south of th line.

This interpretation is supported by other evidenc namely, the survival of the secondary centres (*pagus* ca itals?) mentioned in chapter 11. Of the five centres on three survived into the Late-Roman Period: Cuijk, Wi chen, and Rossum. The centre north of the Waal, Els

93 See chapter 1.

94 Cf. chapters 4, 77, 92; 6, 158, 164, 167, 180; 8, 303; 9, note 32.

95 Boersma 1967.

96 Boersma 1963. See also below, figs. 141 and 142 and Haalebos 1976, 208; a publication on all river area coins has not yet appeared (cf. chapter 6, 134 and note 4).

97 For data see in general Bogaers/Rüger 1974, with further references. Qualburg is a difficult problem. The occupation by the *numerus Ursariensium* in about AD 260 could have been i terrupted after the end of the Gallic empire. On Rossum, s also Haalebos 1976, 203–4, on Asperden, Hinz/Hömbe 1968, 192.

98 See Boersma 1963. The information is thus not up to dat but details about more recent finds (e.g. Teeffelen: W.J.I Verwers 1981, 52) are not available.

99 For example, site 59 (Hien), and an occasional coin fro Wijk bij Duurstede and Vechten.

was apparently never reoccupied after AD 260/270, and neither was Halder in North Brabant, which is the furthest away from Nijmegen in the hinterland. Although very tentatively, it may therefore be concluded that the efforts of Constantius Chlorus were not implemented by a complete restoration of the instruments of force, archaeologically visible in the occupation of forts or other important sites. This does not at all mean that the literary evidence is invalid, because when his victory was as complete as is suggested the whole area in the west and north of the Waal may have been controlled by power (that is, without being occupied but through treaties imposed on and observed by the invaders). This control may have been effectuated from a nucleated area around Nijmegen, encompassing the area between Waal and Meuse (Land van Maas en Waal) and along the southern bank of the Meuse and in the east south of the line Nijmegen-Qualburg.[100]

The reasons for this situation are difficult to ascertain, but on the eve of the campaign against the British usurper a shortage of manpower, material, and, above all, time, is not difficult to envisage. A plausible conclusion therefore, is that the sites occupied under the tetrarchy were presumably continuously occupied and reflect the area controlled by the remainder of the original population. These were at the same time the only trustworthy allies in the region. Through their centre at Nijmegen, then at and around the Valkhof (site 403) and no longer in the former *civitas* capital (site 399), and the other still occupied sites, military and administrative control must have been restored in the rest of the delta. That this was increasingly ineffective further westwards need hardly be said, although there is every chance that there was not all that much to control there, due to the effects of the Duinkerke II marine transgression phase. The habitable surface must have been severely reduced.

Troops were recruited in the *civitas* that was more or less restored after AD 293. Perhaps this was not even an entirely new levy but in part a reorganization of some still existing Batavian units, for example, of the kind of the 3rd-century *numerus exploratorum Batavorum*.[101] As mentioned above, the total number of soldiers recruited, in itself already uncertain, could be used for a very dubious estimate of population size. There is every chance that the calculations given earlier (p. 436) are not realistic and the comparison with the reduction in the number of settlements from AD 275–425 relative to that from AD 50–275 is hardly if at all permitted. The favourable outcome by itself thus means nothing, especially not when it is supposed to be relevant for Diocletian's reign, which we know so little about anyway. Nevertheless, it is probably correct. The general comparison between middle- and late-Roman sites can be taken as a valid figure for the late-3rd century because it should approximately equal the number of sites which continued to exist, even if the inhabitants were newcomers. Some unusual and perhaps late-Roman native wares have occasionally been found at such sites,[102] but the opposite is also true (late-Roman imports in transrhenish sites such as nos. 1 and 7); and it is quite certain that there are no settlements in the river area with only hand-made late-Roman or Merovingian pottery that has not been recognized as such.[103]

The palynological data are, of course, a reliable independent third indication for the decreased total population of the area. The evidence (see fig. 137) is impossible to express in percentages with their false sense of exactness, but they do indicate precisely the same thing. We can safely assume that a reduction of the population to approximately one-fourth of its former level is a realistic estimate, and that does not exclude the possibility that there were intervals when the actual population was higher or lower.

Apart from the number of settlements, there is also the question of the size of settlements. As outlined in chapter 8, there is no real evidence in the river area for such large nucleated villages as are to be found even within 10 km north of the Rhine. But the clustering of sites, at a regional level around Nijmegen and locally around defensible settlements, does at least suggest that more people than before were living in the average settlement. In other words, clustering at the regional and subregional level is taken as an indication for clustering at the settlement level as well. It is possible that this con-

100 Note that this conforms to the overview of the situation given by Bogaers 1968, although part of the evidence and most of the conclusions are no longer acceptable (cf. below).

101 Inscription from Roomburg (*ER* II, 337 = *CIL* XIII, 8825). See Alföldy 1968, 79–80 with further references.

102 Cf. chapter 6, 180.

103 The addition to the list of late-Roman sites of those without wheel-turned late-Roman wares but with identifiable (middle- or late-Roman) transrhenish hand-made pottery would probably increase the survival rate of sites, but only by a few points.

tributed to the extremely low values for culture indicators in the pollen diagrams reached in the mid-5th century. Other factors may have been important, notably the departure of many Salii around that time, but there is no good reason why no-one else would have moved in after so many more or less futile attempts in the previous periods. It is therefore possible that the low values are also the result of increased nucleation of the population and its fields in the Late-Roman Period, causing a further growth and density of the river forest with the minimum values of the 5th century as a result.[104] The trend was reversed again by a very gradual dispersal of settlement that became noticeable in the 7th century.

The regional centre, the former town of Nijmegen (399), had ceased to exist in the late-3rd century, although from a regional analytical perspective one might also say that the *civitas* capital was reduced in size by the transfer from site 399 (Waterkwartier) to 403 (Valkhof). The new regional centre on the Valkhof hill and along the Waal at its foot has only partially survived post-Roman erosion by the river. Its population, especially somewhat later in the 4th century, probably did not exceed some 1000 souls, and if the calculations from the two cemeteries are relevant not more than 700.[105] At that time, although conclusive evidence is lacking, a second large settlement of 300–500 people (Wijchen?, Castra Herculis?, Quadriburgium?) is within the range of reasonable assumptions, as are others further down the line (Cuijk, Rossum). Although this line of reasoning quickly leads to pure speculation, there is every reason to believe that a late-4th-century rank-size curve (see fig. 132) would be not more than slightly concave, perhaps even slightly convex.

It can also be said that whatever the real curve will prove to be, it will be close to a lognormal distribution and thus conform to the rank-size rule.[106] This is normally connected to an integrated and economically mature settlement system, and to a certain degree it may be interpreted in this way: the size of the largest settlement is in proportion to the surrounding region (its productive capacity) and the centre does not drastically interfere in horizontal relations between lower-order settlements. On the other hand, it must be realized that this situation was only very partly the result of an autonomous deve ment. Ulpia Noviomagus may indeed have been dec ing already in the century before AD 275,[107] but the blow was dealt only at about that time with the pa elimination of the town's red tape: the religious, adr istrative, military and other central institutions, and people living off them. The new centre must have l such a super-structure, but the deadwood, the artifi ly maintained part of it, should have largely disappe in the 4th century. In Diocletian's reign, the situa may already have been comparable. There is a ch that initially more people stayed in Nijmegen, but th not very plausible. It is difficult to see where, betv AD 276 and 292, sufficient food would have come fr And there is no evidence for a sudden increase in ar land around Nijmegen in the late-3rd century, let a for a cemetery which should have left some traces if t were still, say, 2000 or 3000 people living in the tc Such burials are exceedingly scarce anyway, and w current views on the chronological position of grav the river area are maintained, virtually nobody died was buried in a regular fashion between AD 270 and Obviously, the chronologies of some cemeteries, in megen notably the end of site 398 (Hees) or the be ning of sites 405 (Mariënburg) and 410 (Hugo de Gr straat), or both, will need some revision. It shoul possible to do this by reversing the normal proced that is, by assigning all graves for which there ar compelling reasons to date them pre-AD 270 or pos 300 to precisely that period (= Qualburg I). In vie the relatively short duration of the period, that coul sult in an acceptable number of late-3rd-century gra An alternative or additional possibility is that espec in this period there may have been a relatively high i dence of very irregular disposal-of-the-dead practi The occasionally discovered 3rd-century mass gr are an example,[108] but these may in reality reflect small-scale disasters with enough survivors at leas bury the dead. A victorious horde of bloodthirsty barians, to state the case explicitly, may have beer less scrupulous, and the literary evidence is su enough to envisage in some years masses of cor floating down the Rhine, and thereby effectively ou the archaeological record.

104 See also the argumentation on p. 426 above.
105 Bloemers 1983, 193–5; above, p. 317.
106 Above, p. 418–9 and fig. 132.
107 Above, p. 418.
108 A.o. Destelbergen (De Laet a.o. 1969) and Gellep (Bechert 1982, 256).

Constantine

The limited evidence for Diocletian fortifications in the river area is compensated for by the quite apparent traces of the efforts under Constantine and his sons to consolidate the situation. The coins from the large ditch around the Valkhof (403) provide a possible date of around AD 330 for its construction,[109] and the find of a stamp of the *legio XXII*, without the title C(onstantiniana) V(ictrix), fits well into this.[110] The general coin evidence, with a drastic increase after AD 306 (see figs. 141–2), also supports the notion of a Constantinian rebuilding program in Nijmegen, as do the relatively numerous burials from the first half of the 4th century.

On the basis of the coin evidence, in addition to some other finds, it is very likely that the former frontier fort at Maurik (Mannaricium?) was also reoccupied at approximately the same time and perhaps the data from Rossum also point to Constantinian activity.[111] The earth-and-timber fort of Cuijk (499) is reported to have been built 'under Constantinus I'.[112] Other sites that should be noted in this context are the presumably still inhabited site 460 (Qualburg) and the evidently continuously garrisoned *burgus* at Heumensoord (391). In the hinterland, in addition to the information supplied by Von Petrikovits' overview, mention should be made of recent discoveries at Maastricht which have provided (dendrochronological) evidence for a Constantinian date of the fortification.[113]

From these data it appears that the Diocletian restoration of order at the frontier was finally implemented only under Constantine by the building of a true defence-in-depth system in Germania II. The earliest activity in this context may have been the construction, between AD 312 and 315, of the fortification Divitia, across the Rhine from Köln and linked with a bridge to the town.[114] Most if not all new defences north of there seem to have been constructed in later years. It looks as if these measures may have been taken as a result of the trouble in these parts after Constantine's accession to the throne. That is indeed a possibility, but no such direct link need be presupposed, and not only because the archaeologically visible measures would in fact be a rather belated response. As mentioned above, it was Constantine who reorganized the army by the formation of central field armies; this is generally supposed to have taken place in about AD 312 but certainly before AD 325. This implied the inclusion of former border troops (*limitanei*) into the *comitates*. Although at that time the frontiers were far from being depleted of garrisons,[115] this is a decisive change of strategy. Even if initially perhaps the result of an immediate need (to defeat Maxentius or for other campaigns[116]) it was never reversed. The further measures of refortifying strongholds on the borders, improving the infrastructure,[117] and protecting it by road-forts are the logical and necessary complement of the reorganization.

As the material from Maurik shows, the forward line of the defensive system did indeed extend up to the Rhine. In this case, the true nature of the Constantinian occupation remains obscure because the site – as so many others in the river area – was eroded by the river. It may well have been only a small *burgus* of the road-fortlet type, a watchtower, or a protected landing-place.[118] The primary function may have been to keep an eye on the ever troublesome Chamavi, but it could also be related to the protection of the waterway. In both cases, we should expect the future discovery of more and perhaps similar sites along the Rhine, and in fact indications for this have emerged. Apart from the enigmatic Brittenburg on the coast,[119] there is the possibility of Constantinian construction in Valkenburg. In a recent paper, Groenman-van Waateringe has presented detailed and incontrovertible evidence for her earlier voiced opinion

109 Cf. Haalebos 1976, 205; the coins from the later excavations are not yet available.

110 Wynia 1979, 65, fig. 73. On the stamp, see Von Petrikovits 1971, 183–4.

111 Haalebos 1976, 208–9. For Rossum the data are too limited to be confident about any dating or interpretation (cf. *op. cit.*, 204).

112 Bogaers 1967, 111.

113 Von Petrikovits 1971. For Maastricht, see Panhuysen 1984, 64: 'the provisional results of the analysis point to a construction date in the last years of Constantine's reign'. Wood from rammed-in oak foundation posts under the late-Roman wall.

114 Von Petrikovits *op. cit.*, 182.

115 Cf. Hoffmann 1969, 132.

116 For a concise overview, see Von Petrikovits 1978, 181–3.

117 In the Netherlands illustrated by the two Constantinian milestone fragments from Eygelshoven. See Brunsting 1946, 34–5 and *Afb.* 10.

118 The typology according to Von Petrikovits 1971, cf. respectively figs. 27, 28, and 24, and comparable to such installations as Hüchelhoven-Heidenburg, Asperden, or something like the ferry-terminal at Engers.

119 It is discussed below, p. 455.

that there is a post-Period 6 phase in the fort.[120] Apart from a few late-Roman sherds, no datable finds have turned up so far, but wood samples have been analysed from a drain and from the construction of the *horrea* (used for repairs) as well as from a post beneath the south wall of the *principia*. One re-used piece of wood yielded a dendrochronological date of AD 223 ± 20, two others of AD 316 ± 10 and AD 365 ± 40, and the foundation post '4th century'.

These data are not conclusive evidence for a Constantinian date of the new Valkenburg Period 7 fort, although they prove that one of the oaks used was felled no later than AD 326. It is quite conceivable that repairs and new constructions were only made under Valentinianus. The few sherds are inconclusive in this respect and coins are lacking,[121] but the most important aspect stands undisputed. The *limes* fort in Valkenburg was used again in the 4th century, with at least two *horrea* and a repaired *principia* within its perimeter.

In addition to Valkenburg, further down the line of former *limes* forts, mention should be made of coins from the period Constantinus I - Magnentius from Woerden, Vleuten-De Meern, Utrecht, Vechten, and in Wijk bij Duurstede (i.e., from Rijswijk-G. or Maurik ?), although in all cases only a few specimens.[122] The same is true for the coastal sites at Aardenburg and Domburg and, in somewhat larger numbers, from Westerschouwen.[123] Admittedly, these finds prove nothing and when more data are available, as in Utrecht,[124] a later-4th or early-5th century date is more likely, as is also the case for places such as Rhenen, Meinerswijk, or Huissen. But they should certainly not be rejected and in view of what we know today it is more than likely that Constantinian activity will be demonstrated at one or more of these sites or in hitherto unsuspected places.

On the other hand, it is perhaps useful to stress that this suggestion should not be interpreted as a proposal to consider a Constantinian restoration of the former *li* A *limes* in the context of a forward defence line is to incompatible both with the available evidence and basic strategy of a defence-in-depth system.[125] Wi the context of such a system there is no line but, ide a broad zone honeycombed with fortified sites and a bile force in the hinterland. In such a context and ir conditions of the delta, whole series of forts and for along the Meuse, Waal, and Rhine are theoretically sible, if Constantine and successors did not have to ry about time, costs or manpower – but they almost tainly did. The refurbishing of *some* old *limes* forts the first line of strongholds is an obvious labour-cost-effective measure. But that does not mean whole series of forts are to be expected.

Coinage

In addition to what was said so far, there is some dence for military sites in the eastern river area hith left undiscussed.[126] Most noteworthy are site (Ewijk) on the Waal and the nearby site 244 (B ingen). The former used to be a luxurious stone-l villa before AD 270, the latter must have been a sim settlement (no traces of stone buildings),[127] and have in recent years yielded an astonishing numbe late-Roman coins (figs. 141-2). Earlier coinage is lacking, but its relative importance is minimal (fig. and is merely further evidence for the long occupa history of both settlements. The coin diagrams of l settlements are very similar, indicating no activity u Diocletian, a sudden increase under Constantine, again under Valentinian.[128]

For a normal settlement, the quantity of coins itself i most incomprehensible, and as argued in chapter cannot be explained away by the use of metal detec in the pre-detector age the absolute total of coins re ered would have been much lower but their relative

120 Groenman-van Waateringe, 1977 and in press. I am very grateful to the author for providing the manuscript before its unduly delayed publication.

121 On this aspect, see below, p. 449.

122 Further literature in Bogaers 1967, 107, note 37.

123 Boersma 1967.

124 See below, p. 454.

125 The same is true for other unfortunate suggestions which have found their way into the literature, from the long-abandoned idea of a *limes Belgicus* (cf. De Boone 1954, 64 f.) to the more recent introduction of a Waal *limes*.

126 But see chapter 8, 309–12.

127 See chapter 8, 279 and 315.

128 I am grateful to W. Tuyn (AWN-Nijmegen) for provi the data. Most coins were determined by Dr R. Reynen an de Jong (Nijmegen), some by Dr J.P.A. van der Vin (Haag) and Dr N. Roymans (Amsterdam). The data in fig. can only be regarded as provisional and could need slight n ification or further additions.

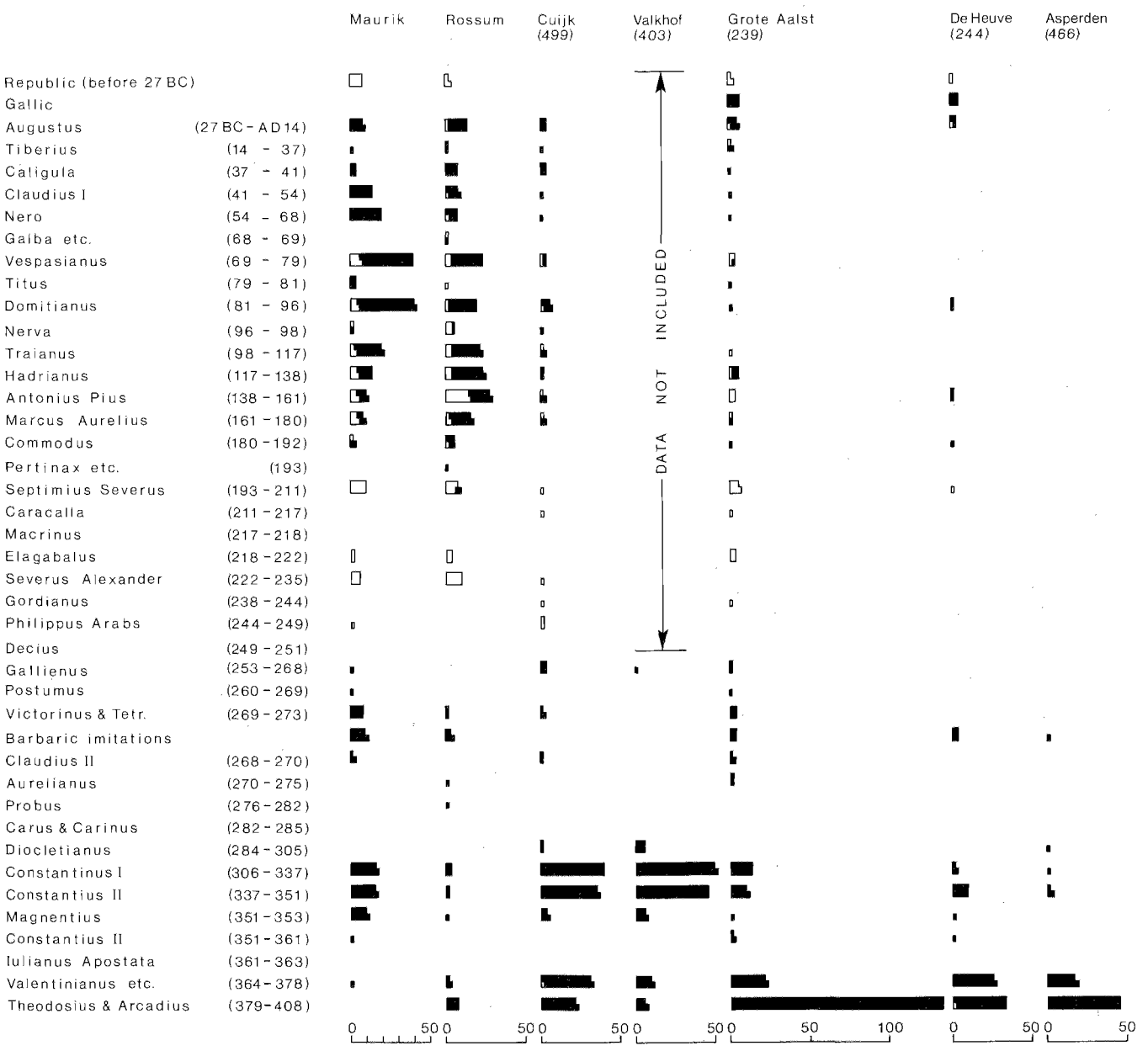


Fig. 141 Coin series from sites in the central and eastern river area. White: gold and silver coins, black: bronze coins. Maurik, Rossum, Cuijk, and Valkhof after Haalebos 1976, fig. 3. The Valkhof coins (late-Roman only) do not include the finds from recent excavations. Data for Asperden after Geißen, in Hinz/Hömberg 1968, 207–12.

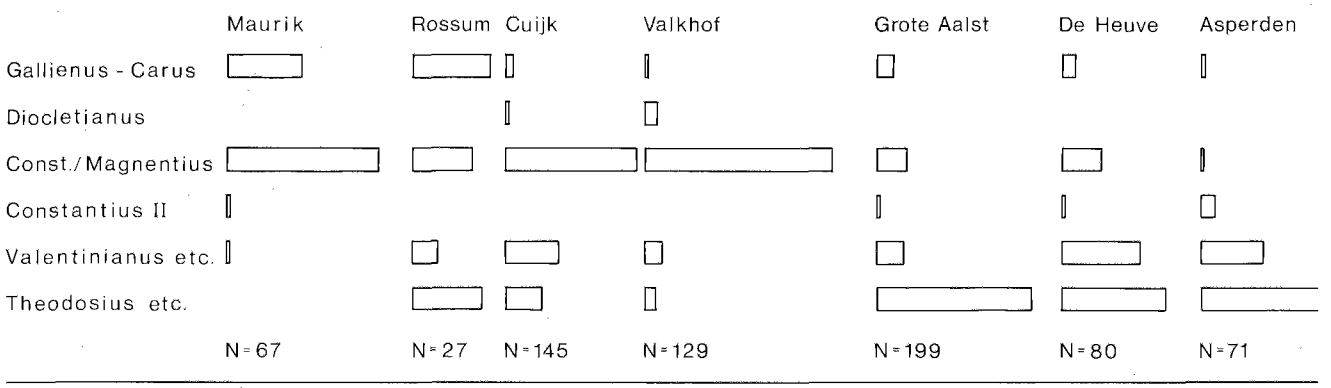


Fig. 142 Relative proportions of late-Roman coins from sites in the central and eastern river area. The provisional impression of the very numerous coins from recent ROB excavations at site 403 is that these will not drastically alter the relative proportions as presented here.

nificance in relation to other sites not less.[129] Another explanation is therefore required and the thoroughly searched surface of sites 239 and 244 permits a first conclusion which could be very relevant with respect to other findspots. That is the relative proportion of late-Roman to all coins, which is 75% and 80%. For the excavation at Cuijk it is also 75%, but for the river finds from Maurik and Rossum only 16% and 8%. When taken at face value, the latter figures could be used to depreciate the relative importance of these sites in late-Roman times. But when, in view of the find circumstances, they are considered a valid indication for late-Roman occupation (as they are by all authors on the subject), then the data from the excavation and the detector-searched sites indicate that the small change of the 4th century is underrepresented,[130] although not as strongly as would appear at first sight.

That is because a direct comparison implicitly assumes that both groups of sites were involved in the same pattern of coin circulation throughout their history, and that is an assumption which cannot be maintained. I fore the Late-Roman Period, sites 239 and 244 were ci settlements, Cuijk was a secondary centre and for a sh time a fort, the same is perhaps true for Rossum wh may also have been a fort much longer, and Maurik v certainly a military site. Coins are strongly associat with soldiers and it is an unquestionable fact that th are found in much larger quantities in or near forts th elsewhere. Therefore, it is likely that late-Roman co are less strongly underrepresented in Rossum and c tainly in Maurik than might appear.

This argumentation could also be turned around by cc cluding that in Ewijk, Beuningen, and Cuijk the late-F man coins are to some degree overrepresented. Becai their chance of being included in the collections stud is in general less and in the samples studied perh equal to that of earlier coins, the only plausible interp tation is that this overrepresentation must be explain by a change in the function of the site, because there enough other continuously occupied settlements wh such a phenomenon cannot be observed.[131] It is not lil ly that such very large quantities of late-Roman coin are to be expected at all late-Roman sites, even thougl would be logical if there were somewhat more late co around than early ones. That is because there was mu more small change.[132] Perhaps it is exaggerated that t

129 The total number of coins recorded is 336 for site 239 and 123 for site 244. With only a few exceptions all 'indeterminable' coins are late-Roman (75 and 28 specimens) and as far as attributable to IVA v. IVB the proportion is about 2:8. Undoubtedly most of these (small, worn down, fragmented) coins are the ones most likely not to have been found without a detector. The relative proportions (fig. 142) are therefore also comparable to those from the other sites.

130 This could have further implications for some of the si with a few late-Roman coins mentioned above, but on most quantities are so small that it is not useful to speculate much about this. Unless, of course, one is prepared to acc that many coins are a characteristic of *all* late-Roman sites.

131 See note 130.

132 For this argument, see Reece 1973 with respect to monetary situation in Britain compared to that in the west empire as a whole.

2598 coins of the Hapert hoard were only worth half a solidus,[133] but it is useful to remember that earlier only 200 Asses were worth half an aureus.[134]

That late coins do not occur in huge numbers everywhere where there was habitation can thus only be understood when it is accepted that for late-Roman times there is no more reason to assume a monetarized market economy than there is for earlier periods. It was, just as before, used to pay soldiers and state employees and it circulated primarily in a context associated with those people. That means that the coins from Ewijk and Beuningen can indeed be attributed to the same cause as we know with certainty for Cuijk: the presence of regularly paid soldiers. Because of the closeness of the two settlements and the availability of stone building material in Ewijk, and its proximity to the Waal, only that site has been interpreted as a military settlement. Perhaps a *burgus* was built there, perhaps it was only a refurbished and fortified villa, but the people living there are best seen as *ripenses* ploughing the surrounding fields and guarding the waterway as well as the road along it.

At this point, attention should be drawn to Valkenburg again. It was concluded above that the site was definitely reused in the 4th century and circumstances (e.g., the constructional details) dictate that this should logically be attributed to military activity. But it is very curious indeed that the extensive excavations of the frontier fort have not yielded one single late-Roman coin. That should imply that there was no military activity, let alone a state magazine or caravanserai.[135] Unless, of course, something is very wrong about Valkenburg. This may be so. It is remarkable that the excavations have yielded so very few coins at all: the entire, deeply stratified, site yielded no more than a mere 79 determined coins.[136] As Groenman-van Waateringe has demonstrated,[137] the granaries were also the last recognizable features under the black disturbed topsoil in which the surface of the later forts must be sought. The process of 'removing the topsoil' may thus have been responsible for the effective removal of late-Roman artefacts. The accidental discovery of two late-Roman terra sigillata sherds from this upper stratum may illustrate the situation, but even then it remains curious why the first excavation planes above the Roman level, with much early-(??) medieval material, did not yield somewhat more late-Roman finds.[138]

When the available data are combined there is, however, a possible interpretation. For the moment, we may accept that most 4th-century artefacts have indeed not been recovered, but only on the condition that the total amount of such finds was fairly limited. As for features, we do have definite evidence for 4th-century constructions. But we do not have evidence for a heavily fortified 4th-century stronghold: at best, the defences of the Period 6 fort were still used and no new ditch appears to have been dug.[139] A combination of the evidence suggests a possible solution, and one which was offered earlier by Glasbergen in his 1967 summary: 'Two rim-fragments of a fourth-century bowl Chenet 320 suggest a short visit of Roman forces at Valkenburg in the 4th century (during the expeditions of Julianus in the late fifties?)'.[140] The visit must have been less ephemeral than Glasbergen believed, but it was not of long duration either. For the moment, we can perhaps best see it as a temporary reoccupation to establish a grain depot in the context of one or another campaign, of the kind that Julianus erected along the Meuse by repairing three forts.[141]

This also removes one other difficulty, namely, that the only kind of more permanent Roman fort which could legitimately be expected in such an exposed position is the usual self-contained stronghold with massive walls and a wide ditch. Although a defence-in-depth system, let alone a *limes* line, in the area behind the coast is almost unthinkable because we know that there was not much worth protecting, there is no reason to reject such

133 See above, 329 and note 403.

134 Or 400 semisses or 800 quadrantes, but that is irrelevant because such coins are virtually absent and must have circulated in only very limited quantities in Lower Germany, compared to Asses. By contrast, most of the late-Roman coins in representative collections are aes-IV with some aes-III.

135 Groenman-van Waateringe 1977 and in press.

136 Glasbergen 1967, 63. By comparison, the figure for Zwammerdam is 114, for Vechten 836, and for Maurik 292 (see Haalebos 1976; 1977).

137 See 1977, esp. 230–1 and the field-drawing (fig. 3).

138 See e.g. the description of the 1962 excavation by Glasbergen 1967, 16–20. Earlier campaigns under Van Giffen may have been less scrupulous, but in later excavations (1980) special attention was paid (under unfavourable circumstances, cf. Groenman-van Waateringe, in press) to possible 4th-century finds.

139 Further reinterpretations may perhaps prove this wrong, but at present it is unacceptable that Valkenburg was converted into a true 4th-century fort: there are far too few artefacts for such an assumption.

140 *Op. cit.*, 152.

141 Amm. Marc. XVII 9, 1–2.

strongholds in principle. The literary evidence quite explicity shows that the Rhine was used to transport grain at least in IVB, and there is not one good reason why we should read Waal there. And in contrast to our very early sources the later ones were well aware of the difference between Waal and Rhine.[142] Protected landing stages are entirely conceivable in this context, as well as a transshipment base at the mouth of the river (Brittenburg?). But Valkenburg 7 was not one of them or only very temporarily, and its possible Constantinian date could, if true, be one further indication. The classical sources suggest that the really large-scale efforts on the Rhine did not take place until Julianus and later.

12.2.2 Batavia Salica

Shortly after AD 350, the Frankish invasions reached massive proportions in the wake of Magnentius' usurpation. It is only logical that this should have had a disastrous effect on society in the river area. The Betuwe may have been subjected to some kind of invasion, followed by a treaty with Constans, at an even earlier date,[143] but this time the defence-in-depth system could not function, apparently for lack of troops. The disaster was now complete, as we are told, and the numerous hoards,[144] in our area for example in Kessel on the Niers (site 467),[145] may illustrate this. Moreover, there are good reasons to assume that among the towns conquered by the Franks was Nijmegen. The coin evidence from the large ditch shows that it must have been filled in shortly after AD 350,[146] as well as the two outer ditches.[147] When Nijmegen fell, it followed that the surrounding fortifications of the Constantinian system would share the same fate, and from what we know this is precisely what happened.

For the best known of these forts, Cuijk, it has been concluded on good grounds that the Constantinian installations were replaced by a new stone fort, perhaps under Valentinianus I and Valens,[148] or earlier under Julianus and Constantius II.[149] A connection with the events in the early 350s is therefore quite likely, and the same applies to the installation at Maurik, where the coin se breaks off sharply with the issues of Magnentius and l centius.[150] A similar conclusion can be drawn for s 239 and 244, and for Rossum (cf. figs. 141–2). One ot coin series, that from Westerschouwen on the coas the mouth of the Scheldt,[151] can also be interprete this way; at other sites the number of coins is certai too small for further inferences.

There is thus every reason to accept the conclusion t with all its strongholds and refuges gone, society in river area as it had continued to function within a sor times more, sometimes less coherent form ceased to ist. What happened to the population we can only spe late about, and necessarily in a very negative way. E though we lack excavations of rural settlements wh might illustrate what happened, there are a few indi clues which can be used as evidence for a wholesale placement of population groups, in addition to the plicit historical evidence. One element thereof has yet been mentioned, namely, the absence of the *duc* Germania II from the *Notitia Dignitatum*.[152] This r be accidental, but that is not very likely. After all, b northern *civitates* of Germania II are absent in the *N tia Galliarum* (VIII). It is generally accepted that t disappeared after the mid-4th century,[153] their for territories being inhabited by Frankish federates, v were of course responsible for the defence.[154] I former conditions been restored, this situation wo presumably have been rather different.

Archaeological indications for the change can to sc degree be deduced from the burial evidence, meagre a is.[155] The cemeteries which show continued use into 4th century (sites 292, 413, 418, and 419) do so only to the middle of the century. Elsewhere (sites 12, perhaps 321) cemeteries start only after the mid-4th c tury. Nijmegen is the exception, because there the r cemeteries begin in the late-3rd century. That is unc standable in view of the move from the former tow the Valkhof site, but both sites 405 and 410 remaine

142 Cf., e.g., note 58. Which does not mean that the Waal or other rivers were not used.
143 See above, p. 436.
144 Von Petrikovits 1978, 187.
145 See above, p. 319. A second but dubious hoard perhaps around Wageningen-Renkum (cf. ch. 8, note 325).
146 Cf. Haalebos 1976, 205. The new excavations at site 403 may provide further evidence in this respect.
147 Above, p. 306–7.
148 Bogaers 1966, 68–70; 1967, 111; see also figs. 141–2.
149 Haalebos 1976, 205–6; see also Bogaers 1966, 70.
150 Haalebos 1976, 208.
151 Boersma 1967, 69 and 95–6, but also 72.
152 *Occ.* 5.
153 E.g., Von Petrikovits 1978, 217; see chapter 8, 312.
154 Cf., e.g., Böhme 1974, 201.
155 See above, paragraph 8.5.3.

use after c. AD 350, although the provisional analysis of the Hugo de Grootstraat cemetery (site 410) shows that the later graves constitute a clearly discernable group.[156] The evidence is not nearly good enough, but we may at least say that discontinuity in burial sites in the mid-4th century is a possibility, in addition to the quite obvious changes in burial rites.

The last observation need not be extensively discussed here, as it has been fully documented in the comprehensive and highly illuminating study by Böhme.[157] The characteristic Germanic objects in male and female burials dominate only after the early-4th century and have been associated with a Germanic population group intimately associated with a military function. The graves offer an interesting amalgam of burial practices (acculturation?), in the adoption of inhumation on the one hand and the introduction of weapons and other personal equipment on the other. The detailed analysis of typochronological developments has also shown that there must have been very close contact with the transrhenish population up to the North Sea coast until the early-5th century, when uniform developments ceased.[158]

Yet another indication for the change in the river area may be constituted by the pottery evidence. Perhaps this is caused by the somewhat obscure position of late-3rd- and early-4th-century pottery, but it is nevertheless remarkable that where we are sure of dealing with late-4th and 5th-century wares these are suddenly found again all over the inhabited Betuwe sites.[159] They are also present at contemporary sites south of the Waal, and it could be that they indicate a reoccupation of the only very sparsely settled region between Waal and Rhine by the new immigrants. Such pottery is also available from settlements north of the Rhine and has been found as far as the North Sea coast, but it decreases rapidly in numbers north of the river.

The above development is, of course, only understandable under the conditions of relative safety which were established after the campaigns of the *caesar* Julianus and the following restoration and further development of the defence-in-depth system under Valentinianus. The archaeological data, limited as they are, indeed confirm such an enterprise and also some of the claims considered exaggerated by several more recent studies. The above mentioned late-Roman pottery and its distribution already show that Roman force was restored up to the Rhine and slightly beyond. The view that Germania II was reduced in size in the north and reached only to the Waal can no longer be upheld.[160] On the contrary, following the earlier but perhaps less fully implemented Constantinian program, it might be argued that now Germania II, for the first time after the fall of the *limes*, was completely and effectively extended to its former border again, and precisely at a time when the river area was entirely occupied by Franks.

The defensive system: a first approach (fig. 143)

The new fortification program may have involved two stages, with Julianus' measures being followed by the Valentinian enterprise a decade later, but such a fine distinction can only be maintained with sophisticated data. These we do not have, apart from the never wholly certain identifications of sites with historically attested places, which sometimes give a precise date. When reviewing the possibilities in this respect the three forts lying in a straight line along the Meuse, which were restored by Julianus in AD 358 to serve as storage depots,[161] are the first to come to mind. One of these may have been Cuijk, repairs perhaps being limited to restoring the timber work of the earth-and-timber wall, as observed during the excavation.[162] As far as the other two fortifications are concerned, their location depends on the direction of Ammianus' straight line. If north-south, the next site could have been Lottum, a probable 2nd-3rd-century road station where late-Roman pottery and coins have been found,[163] and the last one Blerick.[164]

There are other sites further south, but these three have the advantage of being not too far away. On the other hand, Ammianus reports that the corn of the Chamavi was not yet ripe and Julianus had to deal with a mutiny of hungry soldiers, so it could be that the refortified depots were indeed too far south. In that case two further

156 See Bloemers 1983, 195–8.
157 Böhme 1974.
158 Böhme, *op. cit.*, 187 f.
159 See, e.g., chapter 6, 170.
160 See Bogaers 1968 and, very cautiously, Von Petrikovits 1978, 222. See also Van Es 1981, 127–8.
161 Cf. note 141.
162 Bogaers 1966, 69. This would leave the, perhaps later, stone wall on the outside as a further improvement under Valentinianus. Both measures may also be contemporaneous.
163 Cf. Bogaers, in Bogaers/Rüger 1974, 88.
164 The Blariacum of the *Tabula Peutingeriana* (see figs. 20–1). A findspot is unknown, but late-Roman finds are not lacking (see *ER* III, 66).

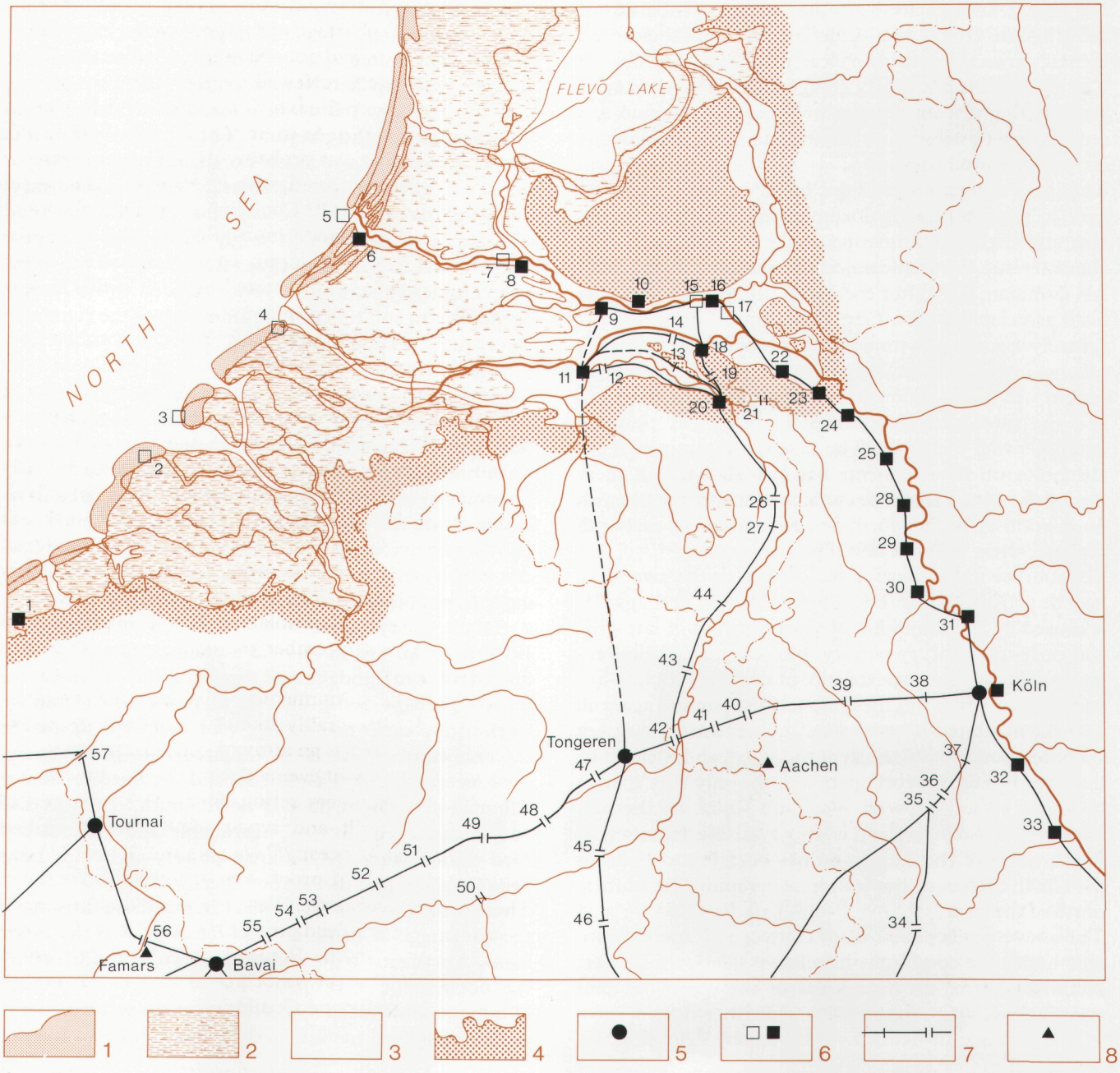

NORTH SEA
FLEVO LAKE
Tongeren
Aachen
Köln
Tournai
Famars
Bavai
1
2
3
4
5
6
7
8

Fig. 143 Model of the 4th-century defence-in-depth system of Germania Secunda and part of Belgica Secunda. In view of the numerous uncertainties and unknowns, no attempt has been made to distinguish between the different (Diocletian, Constantinian, and Valentinian) stages of development of the system. As a model, the system as drawn here should be valid for the last two of these stages; as a representation of facts and some assumptions, it is largely valid only for the Valentinian system. To avoid a complicated legend, different but functionally comparable sites have been indicated by the same symbol.
Legend: 1 coastal dunes; 2 marine clay deposits and peat; 3 fluvial deposits; 4 Pleistocene deposits; 5 fortified towns; 6 less or more certain military fortifications; 7 less or more certain road forts (fortified settlements and *burgi* along land- or water-routes); 8 other late-Roman urban settlements. Sites: 1 Oudenburg, 2 Domburg, 3 Westerschouwen, 4 Oostvoorne, 5 Brittenburg, 6 Valkenburg, 7 Vleuten-De Meern, 8 Utrecht, 9 Maurik, 10 Rhenen, 11 Rossum, 12 Kessel, 13 Wijchen, 14 Ewijk, 15 Driel, 16 Meinerswijk, 17 Huissen, 18 Nijmegen, 19 Heumensoord, 20 Cuijk, 21 Asperden, 22 Qualburg, 23 Altkalkar, 24 Xanten, 25 Rheinberg, 26 Lottum, 27 Blerick, 28 Moers-Asberg, 29 Krefeld-Gellep, 30 Neuss, 31 Haus Bürgel, 32 Bonn, 33 Remagen, 34 Junkerath, 35 Zülpich, 36 Rövenich, 37 Brühl, 38 Hüchelhoven, 39 Jülich, 40 Heerlen, 41 Hulsberg, 42 Maastricht, 43 Stokkem, 44 Heel, 45 Amay, 46 Vervoz, 47 Oreye, 48 Braives, 49 Taviers, 50 Namur, 51 Penteville, 52 Liberchies, 53 Morlanwelz, 54 Waudrez, 55 Givry, 56 Famars, 57 Kortrijk.

sites should be mentioned, namely, Catualium and Feresne, both indicated on the *Tabula Peutingeriana*. Catualium is probably Heel and Feresne could be the eroded late-Roman installation at Stokkem (Belgium).[165] Finally, there is also the Constantinian fortification of Maastricht but that is not likely to have been one of the sites mentioned by Ammianus.
When, however, the line of fortifications ran east-west, Cuijk was almost certainly one of them. There is much to say for such an east-west location, because for a campaign against the Chamavi it makes tactically much more sense to have three supply depots as a broad base-line for the operations than to have them lined up deep into the hinterland. The shortage of food may have been due to a variety of causes and not necessarily to distance. When the possibilities are reviewed for the other two depots, there is at least one site which could qualify: in Kessel, not far from Rossum. As in Stokkem, washed down wall fragments of what was probably a late-Roman fortification have been discovered, with appropriate pottery and coins (a.o. from IVB).[166] Could it be that this is the long-sought site of Ad Duodecimum from the *Tabula Peutingeriana*?[167] The third supply base in this case is as yet unknown, unless Rossum could qualify. This site was reoccupied under Valentinianus.
For the forts on the Rhine restored by Julianus we have the quite likely identifications of sites 460 as Quadriburgium and site 126 as Castra Herculis. For the remaining strongholds of the late-4th-century system, no very precise datings are possible. It included, in the eastern river area, the two *burgi* at sites 391 (Heumensoord) and 466 (Asperden), site 239 in Ewijk, and perhaps site 315 (Wijchen). In the hinterland, no other potential strongholds can be identified, although the continuing occupation, let alone the Germanic soldiers' graves, in Nijmegen is more than enough evidence that at some point the site was also restored. Valentinianus himself may even have been there on 20 September 368, as we may infer from a law signature.[168]
Back to the Rhine. Ammianus Marcellinus, although inclined to write favourably about Julianus, was a responsible historian, and as an army officer who had served in Gaul and wrote about events during his own time he deserves to be taken very seriously. When he writes that Valentinianus built and repaired forts all along the Rhine as far as the ocean,[169] we are not at liberty to read 'Waal' without solid proof. Such proof is lacking, despite the fact that on the Waal some strongholds can be expected and are indeed present. In addition to the habitation evidence in the Betuwe and Castra Herculis at Meinerswijk, there are other sites to be considered. Between Meinerswijk and Qualburg there may be a fort at

165 On Heel, see *ER* III, for Stokkem, Mertens, in Bogaers/Rüger 1974, 153.
166 See *NKNOB* 1977, 189 and Haalebos, in press.
167 The *tabula* locates the site at 18 *leugae* from Nijmegen, which could fit. Even if the name does mean that the place was located 'at the twelfth league-stone' as current assumptions maintain, that need not mean that it was 12 leagues from Nijmegen. Unfortunately, this causes problems with the identification of Rossum = Grinnes at V (some say VI) *leugae* from Ad Duodecimum, but we can easily declare this corrupt and read II, which could be nearly correct. But then the site is still on the wrong side of the present-day Meuse. See also chapter 3, 70, notes 218 as well as 217.
168 Cf. De Boone 1954, 103, Von Petrikovits 1978, 206.
169 Amm. Marc. XXVIII 2, 1.

Huissen (site 135), and downstream from Meinerswijk is Driel (site 123).[170] Next there is Rhenen (site 12), where all we have is a cemetery with Germanic soldiers, but Böhme 's interpretation that they manned some kind of military post on the high Pleistocene ridge there is plausible.[171]

Further downstream, Maurik may have been reoccupied again by Julianus or under Valentinianus. There are too few coins to be certain, but then the very small latest Roman aes-IV would be the first kind of coin to be underrepresented in view of the find circumstances there. The finds also include a late-4th-century hairpin and parts of crossbow fibulae.[172] No statements can be made as yet about Rijswijk (G).[173] The late-Roman finds 'from Wijk bij Duurstede' may originate from this site, but that is wholly uncertain. Likewise, Vechten in late-Roman times must also be considered doubtful. The minimal amount of coins and pottery, if anything, would suggest early rather than late-4th-century activities.[174]

Utrecht, however, has yielded more useful evidence. Van Giffen ascribed phases IV a–c to constructions under Julianus, but apparently this need not be true.[175] Nevertheless, there are some coins up to Valentinianus and there is a not inconsiderable collection of late-4th and early-5th-century pottery, in part from a significant stratigraphical position.[176] In addition, recent discoveries include three graves from a hitherto unknown cemetery with the earliest datable excavated burial dating to the second half of the 5th or the early-6th centuries.[177] At the same time, these burials provide a *terminus ante quem* for an as yet undatable, artificially raised stratum overlying an earlier, late-2nd-century raised layer. For the moment, even if Van Giffen's arguments are questionable, there is thus every reason not to reject his conclusions.

At the next former *limes* fort, in Vleuten-De Meern, relevant finds are apparently meagre, but the excavations

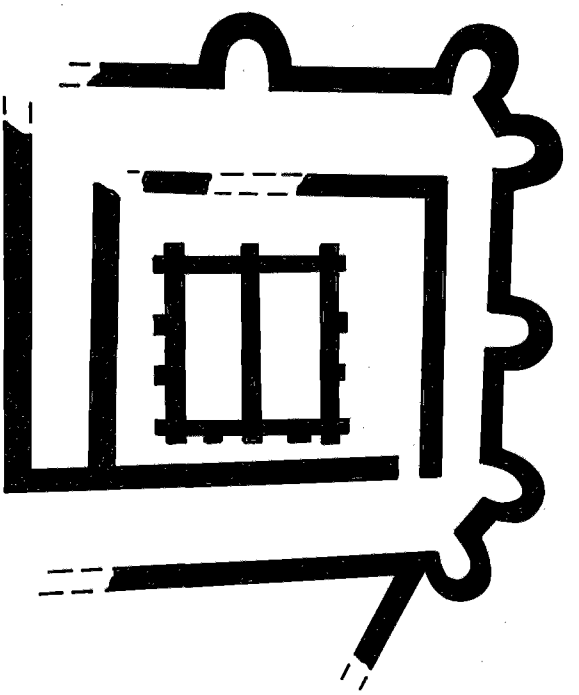


Fig. 144 Plan of the Brittenburg as depicted by Abraham telius in AD 1568.

have yielded two massive foundations which poste the late-3rd-century destruction of the fort.[178] The ex vators suggest these should date to the 4th century observation which becomes even more relevant in context of the site, where post-AD 100 layers are seve disturbed and at least 1–1.5 m (all upper strata) w dug off in the past. The limited excavations provided further details and all we have from Vleuten is appar ly one coin of Magnentius. This prohibits further c clusions beyond those of the excavators. Of the th

170 On both sites, see chapter 8, 309. The absence of much coinage in both cases follows from the find circumstances.

171 See chapter 8, 317–8.

172 The finds are only briefly mentioned (Haalebos 1976, 209) and include pottery. At least the hairpin must be assumed to date from the late-4th or early-5th centuries (Böhme 1974, 35 f.).

173 Van Es 1984, 280–1.

174 The one post-Magnentius coin is one of Honorius, but it is a solidus.

175 See Bogaers 1967, 107, note 37, which does not give any conclusive evidence against the earlier interpretation. Professor H. Brunsting, however, is also in doubt (pers. cor 1980).

176 I am grateful to the town archaeologist, T.J. Hoek and collaborators, with whom the finds were examined in vember 1983. They contain several Alzei forms including the (type 3) cooking-pots with thickened, cordoned rim cussed in chapter 6, 168–70. Moreover, part of these finds found under the collapsed wall of the fort. There is also rovingian material among the sherd collection.

177 De Groot/Hoekstra a.o., *NKNOB* 1982, 173–6.

178 See Jongkees/Isings 1963, 8–11, 17, 38, 98, and Plat

possible dates for the building activities, IIId, IVA, or IVB, the last two are the more likely.

In Woerden, where all upper levels were also removed, presumably in about AD 1700, and where not only Merovingian but even late-Medieval pottery is very scarce,[179] stray finds include coins from Valens and Theodosius in addition to the Constantinian specimens.[180] For Bodegraven and Zwammerdam evidence for late-Roman activities is lacking.[181] For Alphen all we have despite many finds in recent years is an old reference about 'coins up to Honorius', not necessarily invalid but not very informative either, and for Leiden-Roomburg all we have is inconclusive literary evidence.[182] Apart from the already discussed evidence for Valkenburg, no 4th-century finds are known from the remaining sites towards the coast. There is, however, the enigmatic Brittenburg (fig. 144), as a possible late-Roman fortification the subject of much discussion recently revived again.[183]

The walls with half-round projecting towers are now considered the remains of a 3rd-century town wall of Lugdunum, of a 4th-century fort, and even of an early-Medieval castle. None of these proposals can be preferred on the grounds that it provides a better context for the two curious double-towers at the corners. But all finds, especially those of building material and the evidence on the foundation method,[184] indicate that the ruins must have been those of a Roman installation. The limited reliable finds from the site cover the period from about AD 100–250, those from the 1982 excavations have been dated to AD 160–240. The evidence allows the conclusion that there was a *limes* fort here, presumably occupied by the *cohors I Raetorum civium Romanorum p.f.*, and also a naval base of the Classis Germanica (20% of all tile-stamps). The numerous military tile-stamps (35 from 1982 plus 23 older ones) cannot be used as evidence against a civilian town there, but the only advantage such an interpretation has over a designation as 4th-century fortification is that contemporary finds are available. The significance of that fact is limited, because the 1982 excavations were at a considerable distance from the actual Brittenburg, at the outer perimeter of the *vicus*, and the lack of late-Roman finds still surviving after their discovery in the 16th or 17th centuries is of course entirely undecisive.

For the moment there are thus no compelling reasons to reject the Brittenburg as the heavily fortified late-Roman stronghold of c. 75 × 75 m it seems to be, its central *horrea* indicating a function as a (grain) storage depot. This is compatible with the Valentinian defensive system as discussed below; in a way it is even a necessary element thereof, as a transshipment base as well as to block access to the Rhine.[185]

In addition to the Brittenburg, there might be other late coastal sites relevant to the present theme, especially at strategically important sites comparable to the mouth of the Rhine at Katwijk. Bogaers has recently discussed the evidence for the Helinium, Tacitus' *os immensum* and the mouth of Waal, Meuse, and probably a branch of the Scheldt.[186] The dispersed evidence is apparently sufficient to assume one or more military sites, a naval base and (or in combination with?) a fort where cavalry was stationed, perhaps to be localized at Oostvoorne where in the 18th century foundations were seen. Although the evidence for late-Roman activity is thin, the suggestion of a late stronghold is indeed acceptable and for very much the same reasons as the Brittenburg. For the mouth of the Scheldt, the coins from Westerschouwen on the northern bank and from Domburg on the southern bank should be mentioned.[187] The vague indications for 4th-century activity in Aardenburg and Brugge are probably irrelevant,[188] the more so because the main road into the hinterland from there was effectively protected by the fortification at Oudenburg.[189]

179 Bogaers/Haalebos 1983, 309.

180 See *ER* III, 133.

181 Haalebos 1977, 203, 216, mentions one possible 4th-century coin from the *principia* at Zwammerdam.

182 Alphen: cf. Bogaers 1967, 107, note 37; if Roomburg = Matilo, then the site is mentioned by the Anonymus Ravennas (IV, 24), cf. fig. 140. But this cannot be used as evidence in the present context.

183 An overview of older literature in Bogaers/Rüger 1974, 36–9. For recent data, see Bloemers/De Weerd 1984. A paper by De Weerd will be published in the Proceedings of the 13th Congress of Roman Frontier Studies, Aalen.

184 See Dijkstra/Ketelaar 1965 for an overview of relevant drawings.

185 In this respect, the so-called 'tower of Kalla', a round tower of which nothing is known except that it was further into the sea than the Brittenburg, is perhaps relevant. Together with the two walls running from the fort into the sea one might entertain the notion of harbour works.

186 Bogaers 1974.

187 Boersma 1967.

188 See Willems 1983a, 123, with further references.

189 See Mertens/Van Impe 1971 for a discussion of the fort as well as the cemeteries.

The available information mentioned so far has been combined in fig. 143, together with other published sites.[190] With respect to several of the indicated sites, their indication on the map rests on uncertain or real but very limited evidence. That this is due to complicating factors such as the destruction of late-Roman levels in some cases or the erosion by rivers and by the sea in others, is true. Also true is that more research is needed at several tangible sites, and that a combination of conclusive and inconclusive evidence cemented by wishful thinking should not lead to presenting fiction as fact. Therefore, fig. 143 as a whole can be regarded only as a model of the situation, but one which is based on hard evidence and on the analysis of that evidence, to which the uncertain elements have been added because they agree with that analysis. They are also based on the assumption that accidental or surface finds, or those from limited excavations, are normally indicative of much more contemporaneous artefacts after more research. For the eastern river area, the find-histories of numerous sites confirm that assumption.[191] When the context is different, such as with large excavations, the interpretation must be different also. Zwammerdam, with one (uncertain) 4th-century coin, cannot be considered in the present discussion. Valkenburg, however, shows that even a mere few sherds may yet be very significant indeed, although the scarcity of finds prohibits inferences about a prolonged reoccupation of the fort.

As far as the river area is concerned, the imported wheel-turned pottery shows that the region was relatively densely settled in the east with people up to the Rhine having direct contact with the hinterland. In the central part, research has not yet been completed, but a first published distribution map[192] shows that settlements with similar evidence for late-Roman habitation are present although in lower numbers. In the coastal area, such sites are virtually unknown.[193] Although the effects of the marine transgression phase must have been important, some areas were obviously still habitable and we may conclude only that we are dealing with a probably very sparse habitation, the people involved not being in any regular communication with the hinterland.[194]

When the fortifications are added to this picture, there is good evidence for protection of the frontier zone in tl east and somewhat less in the central river area. For tl moment, there is every reason to assume that the ar east of the line Utrecht (or Vleuten)-Rossum was pr tected by a defence-in-depth system, and it is hardly coincidence that this is the region where the late-Rom pottery is found. Entrance to the area over water w controlled by front-line strongholds: from the Fle lake, if the IJssel could be used, by Meinerswijk-Cast Herculis (and Huissen?) and for the Vecht by Utrech Traiectum; from the coast over the Lower Rhine t Utrecht (or Vleuten?, or Woerden?), over the Lek, th presumably a functioning river,[195] perhaps by Rijswij G./Wijk bij Duurstede or Maurik; and from the He nium over the Waal by Rossum and over the Meuse t Kessel.

The latter two sites may also have served to block a ro into the hinterland to Tongeren, which quite definite was a function of the Cuijk fort. The necessary interm diate posts also seem to be present, with Rhenen on tl Rhine and Nijmegen on the Waal as the best example to which can be added Ewijk on the Waal, with less ce tainty Wijchen on the Meuse and Driel on the Rhin and quite uncertainly Vechten. For land routes, He mensoord and Asperden are of course very important i termediate stations. In spite of the limitations of the ev dence it is thus quite clear that the entire river area w under Roman control in the later 4th century, and it also obvious why this should be so because we know th the area was a vital link in the transport of British grai If this had been transported only over the Waal, tl control of Batavia (i.e., the area between Rhine ar Waal) was necessary for securing the waterway. But o sources do not say so; they tell us instead that even tl foreland beyond the Rhine was, at least from time time, under Roman control.

Transport over the Lower Rhine may, occasionall have been rather perilous. But there is no reason to a sume that the Germanic soldiers manning the stron holds were just sitting there waiting for the next attac In general they must have known reasonably well wh was going on in the north. Also, it may have been adva tageous to use the Lower Rhine. Even aside from tl

190 Notably Von Petrikovits 1971, Bogaers/Rüger 1974, and Johnson 1983.

191 See chapters 3, 71–2 and 4, 90–1.

192 Van Tent 1984, 259, fig. 78.

193 Recent detailed inventarisations of sizeable areas, such as Midden-Delfland and Den Haag (see Bult 1983; Van Heeringen 1983) have not provided any indications except for tl long-known coins from Voorburg-Forum Hadriani.

194 See Bloemers 1978a, 113, Bult 1983, 28–31, but al Hallewas/van Regteren Altena 1979, 99–100.

195 Berendsen 1982, 185 and figs. 8.14–5.

sychological impact of demonstrating power, refur-ishing former forts was easier than building new ones. n addition, the Lower Rhine was a relatively small and afe river. The Waal was larger and, perhaps very im-ortant, it had its mouth in the Helinium. The effect of he Duinkerke II transgression and periodical storm sur-es must have made the *immensum os* even more im-nense, with numerous creeks and gullies, constantly hifting shoals and sandbars, and thus not a very attrac-ive area to pass through. There is, however, no reason o assume this was not done and, in order to be able to ontrol passage through the area, a fortification there nust be considered a strategical necessity.[196]

To return to the Rhine: in view of the recent discoveries, Valkenburg can now be used as definite evidence for the act that the last stretch of the Lower Rhine was indeed onsidered to be of interest. Even if itself only relatively hortly occupied in the context of one or perhaps more ampaigns as a depot, it demonstrates the army's con-ern with the river. Valkenburg thereby places the other inds, even though in many cases we cannot yet be sure bout their true meaning, in a logical context. And that ncludes the Brittenburg, which can be profitably seen as stronghold blocking the entrance through the mouth of he Rhine in case of seaborne raids, and as a transship-nent base.

ocial and economic developments

stimating a population size for the late-4th century is a opeless task. We must be content with the observation hat as far as archaeological evidence for occupation is oncerned, it was probably larger than at the beginning f the century. If we consider that the Salii probably nanned most if not all of the strongholds and in addition rovided an *auxilium* for the field army, that could sup-ort such an interpretation.

f there remained some organized administration, its entre should still have been located in Nijmegen and he fact that Valentinianus himself may have been there ould be relevant. We know at least that habitation con-inued at the Valkhof and below it on the bank of the Waal, and from the burials it is evident that Germanic oldiers and their wives lived in Nijmegen. Conceivably they were the retinue (later called *antrustiones*) of a (the major?) king of the Salii residing at site 403. The only other site which could have been of administrative importance is Wijchen (315), but all we have from there are surface and dredged-up finds. The defensive system and the region's importance for the Rhineland allow the conclusion that at least the broad framework of a Roman administration may have existed, but there was also an effective internal socio-political organization.

This can be deduced from the earliest codification of the Lex Salica, the *pactus legis Salicae*, formulated between AD 507 and 557 presumably under Childeric's son Chlodoweg (that is, before 511).[197] We can only assume that the legal and organizational system contained in the Lex Salica or in the works of Gregorius of Tours was a further development from that which the Salii of the late-4th and early-5th century must have had. But even so, many aspects of the later codification go back a long way, and Frankish society a century before will surely have been markedly stratified.

To a certain degree, river area finds confirm this. The Germanic soldiers' graves and those of their wives – as analysed by Böhme – fit into the higher echelons of the historically attested social strata. Böhme[198] sees their appearance as a new and original development of the recent immigrants, conscious of their important position in Gaul. In the graves there is a very rapid development towards rather uniform grave goods and stereotype combinations which may be profitably seen as marking social status. The distribution of characteristic grave goods such as abundantly decorated belt-fittings all along the Rhine-Danube frontier are an indication for the development of a new, and this time Germanic, 'military caste' along the frontier, archaeologically even more clearly visible than that of the late-2nd and early-3rd centuries.

Again, fibulae were also a distinctive element, and not only in female dress. The role of crossbow fibulae as insignia has already been mentioned, and in our area it is paralleled by the related *Stützarmfibel*, with a marked concentration in the river area and also in the northern coastal area between Elbe and Weser.[199] An extraordinary find in this context is the gold specimen from Wij-

96 Cf. Bogaers 1974, 76. Oostvoorne is a possibility but in iew of the size of the mouth a location better situated with an ye to control purposes, and thus further inland, could (also?) e considered. Given the geological situation (see Bloemers 978a, Appendix 8, and Hallewas/van Regteren Altena 1979) it night therefore be predicted that, if anywhere, a late-Roman stronghold is most likely to be found in the area of present-day Spijkenisse or Vlaardingen.

197 See Zöllner 1970, 112 f.

198 Böhme 1974, 165, 190.

199 Böhme *op. cit.*, *Karte* 10. Haalebos, in press.

chen (fig. 89),[200] and in ascribing it to a high-ranking Germanic official we should not be far off the mark.

In paragraph 8.5.4, the gold torcs such as those of the 'Velp-type' (see fig. 145) have also been interpreted as probable status symbols of very high-ranking individuals.[201] Their concentration in the eastern river area and specimens to the north (not surprisingly that part where the Salii, and perhaps the Tubantes, came from) and east of the Rhine is yet another indication of the close ties with transrhenish groups. Perhaps their occurrence in hoards can be connected to the Salii's great move southwards. The gold hoards quite obviously belonged to chieftains, a very large one such as the first hoard from Velp may qualify as the *thesaurus* of one of the *reges critinos*.[202] It is possible that, contemporary to or in the wake of (if not indeed in coordination with) the Salii under Chlogio, other groups set into motion, at first of course only the warriors and their leaders who are not likely to have taken their own *thesaurus* with them.[203] The hoards can thus be seen as instances when the chieftain and others who may have known its location did not survive the campaign. The precise date of burial is difficult to ascertain, and in many cases 'about AD 400' may be proposed, but 'VA' would do just as well, because that is *the* phase of the great gold hoards.[204]

There is a difference between hoards with torcs and those consisting only of solidi.[205] But in terms of wealth it is remarkable that of the very large hoards, containing 0.5 kg of gold or more, all but one (Dortmund) have been found on the Rhine, a second category of 0.1–0.5 kg is virtually limited to transrhenish hoards, and only the hoards of the third category of less than 0.1 kg oc on both sides of the Rhine. This can be the combined fect of Frankish chieftains' treasures being bur beyond the Rhine and the fairly effective retrieval gold by the Roman state behind it. But it may not b coincidence that four very large hoards were found the Rhine, two near Xanten and two in the eastern ri area.

It may be a symptom of the growing prosperity and e nomic activity in the late-4th century, centred on Rhine area and affecting the regions within and beyc the frontier. For example, the river area could be source of some of the late-Roman or earliest Medie metalware, such as the *Stützarmfibulae* and long ha pins.[207] Other indications are the well-known te nigra-like cups Chenet 342 (fig. 37), which have a dist bution between Elbe and Seine comparable to the met ware studied by Böhme, with the densest concentrati on the North Sea coast, the Lippe/Ruhr area, and eastern river area.[208] In principle, these cups may considered a 'Frankish' development and the product native form and Roman technique; and there is so evidence for production in the Lower Rhine area.[209]

In this respect attention should also be drawn to the te tative identification of a group of pottery labelled *lo Rhenish* (or *Frankish*) *grey sigillata derivatives* that v discussed at length in chapter 6.[210] Whether the labe correct remains to be seen, but in any case the group such is again an original development which from ma points of view (fabric, decoration, chronology) star between 'late-Roman' and 'Merovingian'.

200 See also Haalebos, in press.

201 There are also typologically identical bronze specimens. See Waterbolk/Glasbergen 1955 for discussion and distribution map.

202 See above, 440 and note 83. In view of the amounts of money paid or received in various contexts as mentioned in the *Historia Francorum*, the true significance of even the largest hoards is very difficult to evaluate.

203 The same was later the usual practice for the Frankish kings; the *thesaurus* was kept in the king's residence and stayed there when he went away on campaigns or on other business: see Weidemann 1982, 19–20. It only moved when the residence changed.

204 For an overview, see Bloemers 1983, 198–9, to which should be added the (unquantifiable, but probably worth over 500 solidi) first hoard of Velp (cf. chapter 8, note 390) and the hoard of Xanten-Menzelen of at least 207 solidi (cf. chapter 8, note 338).

205 See further paragraph 8.5.4.

206 The two Velp hoards, sites 28 and 30. The significanc the location of these two hoards is uncertain. Perhaps they be connected to Meinerswijk-Castra Herculis, but it is entir conceivable that archaeology has so far failed to locate a v important late-Roman site (stronghold?) on the hills of pr ent-day Velp (or Arnhem). However, a hoard is not a cemet and we cannot simply postulate such a site as in the case Rhenen (where there is also a hoard in addition to the cer tery!).

207 See Böhme, 1974, maps 9 and 10; Haalebos, in press. I specimens, see above, figs. 89 and 56, respectively.

208 See the map in Bloemers 1983, fig. 8.27 and chapter 164–5 with eleven new findspots for the river area. The sa conclusions already in Van Es 1967, 550–2.

209 Chapter 6, 165, note 106: site 7, Bennekom.

210 See p. 172–7.



Fig. 145 Gold torcs from Rhenen (site 9). Two faceted torcs of the Velp type and part of a third torc (with hinges), decorated with five glass paste stones. (Photo Centraal Museum, Utrecht).

All these aspects, from pottery and metalware to the new burial rites and the relative prosperity can be understood as a process of mutual acculturation between Franks and Gallo-Romans leading to new and original developments, in particular in the regions at the interface of both groups along the imperial frontier. The increasingly important role of Franks as high officials of the Roman state can be seen as a result of this development. It is difficult to identify with certainty a core area which could be considered the nursery of Frankish society. There may have been several nuclei along the Rhine in Lower Germany. The Salii's quite obvious pre-eminence soon afterwards, shown by the fact that their highest elite, the Merovingians, became the Frankish royal house and their law, the Lex Salica, became the Frankish law, could be a result of historical accident. It could be attributed to the administrative and military capacity of such men as Chlogio, Childeric, and, of course, Chlodoweg. But it is not fruitful to speculate on this.

The end of the developments from our point of view came when Chlogio moved south, when the centre of gravity shifted further into northern Gaul. This can be seen as Frankish society hatching out of its nursery, and further decisive developments, whether put into a catalogue of historical facts or treated as a further process of socio-political and economic development, took place elsewhere. The brief but crucial period which has been termed Batavia Salica, perhaps not entirely correctly, had ended by the mid-5th century. And so had, even before then, all real power of Rome.

12.2.3 The River Area in the Merovingian Period

The collapse of the Roman state, a growing power vacuum in Gaul, and Frankish force are of course elements which determined 5th-century developments. It must also be realized that by controlling the area in southern Belgium and northern France the Franks controlled an area of great agricultural potential. When current assumptions are valid, the Roman occupation may have had a disastrous effect on the fertility of the Pleistocene sandy soils, in contrast to the calcareous soils of Britain and the continental limestone/löss area, which are less easily exhausted.[211]

The Holocene river area deposits can hardly have been exhausted, but there another factor is very relevant, namely, the position of the area from a supra-regional point of view. The end of Britain as a Roman province the early-5th century[212] meant that at that time at the le est the strategical importance of the Dutch river area f transports was also terminated. And that must ha drastically reduced the fuel that fed the local economy. In this respect, it is perhaps worth mentioning that coi are not an important indication. The earliest 4th-cent ry coins are very scarce indeed in the river area, b bronze coins were issued by the state in order to retrie gold, not to facilitate economic exchange.[213] The ve unequal distribution of bronze coinage during the enti Roman Period is a consequence of this. The cessation Roman control implied the cessation of official paymen in gold and thus of the necessity to provide bronze. Tl fact that so few later bronze coins are found and that r replacements were issued in the form of imitations, co firms the limited function here as elsewhere. The san phenomenon may, in fact, explain the relatively hu number of coins from the House of Theodosius four on some sites (fig. 142). That so many are found must l because they were the dominant issues in the pool of ci culation at the time when small bronze change becan obsolete.

Although unrelated to the diminished importance of tl river area as a trade corridor, the breakdown of the lat Roman monetary cycle meant the termination of y another, although more general, economic stimulu Relevant for the river area are also the changes in the flu vial system which must have started in the 4th centu and were increasingly important in the 5th centur when many formerly navigable branches began to sil up.[214] With an independent Frankish kingdom in th Rhineland around Köln, Alamanni and other grou further upstream, and the departure of many people an most of the elite with their retinues to the south, th Dutch river area in the late-5th and 6th centuries mu have become a backwater in all but the literal sense.

The maximum extension of river forests in this perio (fig. 137) confirms this picture, and so does archaeolog not so much by positive indications but rather by th lack of them. The dating of pottery, let alone that of th sherds found at the surface of settlement sites, is n very precise. As outlined in paragraph 8.6, there is cont nuity of occupation for the region as a whole and fo some sites in particular, but such continuity is certai only in a few instances. As far as the evidence goes, th

211 See Groenman-van Waateringe 1981, 1983.

212 See, e.g., Frere 1978, chapter 17.

213 Cf. paragraph 8.5.4 and note 337.

214 See paragraph 7.2.1 and figs. 58–9.

indications are that the 106 'Merovingian' settlements are the result of population growth into the 7th and early-8th centuries, but from what minimum level it is impossible to estimate.

The archaeologically visible economic activity at least is datable to a relatively late phase. Gold coins may have been minted in Nijmegen in the late-6th century and the production of wheel-turned pottery at site 420 (Ubbergen) can be dated to the late-7th and early-8th centuries. That some people were still able to hide (or occasionally perhaps lose) gold coins throughout the 5th and 6th centuries[215] is an illustration of the continued relations with the south, but even there the minimum is reached in the late-5th – early-6th centuries.

This is most likely to be the phase with the lowest population density and a largely autarkic economy. Not surprisingly, human interference in the landscape was down to the Bronze Age level (fig. 137), and economic life may not have differed widely in the two periods, although we are not at liberty to extend this to social life as well. That the Scandinavian raiders of the early-6th century were not interested in the river area but at best only in the region from Nijmegen to the east[216] thus agrees with expectations.

What could be more surprising is the apparent continuity of settlements, which is striking compared to evidence from other regions further upstream.[217] A different location of settlements from that in the Roman Period, which is the rule elsewhere, is the exception in the river area. Nor is there an overall relocation of settlements after AD 350, when the major change of population must have taken place. The explanation for this abnormal situation is, however, given by the above conclusion that in, say, AD 500, a minimum population level was reached and only relatively few sites were inhabited. This implies that the possible continuity on so many sites visible in fig. 23 (chapter 4) has quite correctly not been interpreted as such and that the interruptions in the bars indicating the chronology should in most cases indeed be taken as real discontinuity.

That so many late-Iron Age and Roman Period settlements were apparently reoccupied after one to four centuries is in fact logical. Whereas in the Rhineland and the Pleistocene sand areas a clear preference of new Frankish settlers for valley-floors and streams can be observed, this was already and necessarily true for the river area in Roman times and earlier. Site location here is prescribed by geological and hydrological conditions and in every period settlers chose the best-located places which were thus necessarily the same in almost all instances. The only significant change in the habitation of the river area with regard to site location took place after World War II and most strongly in the past two decades, when it extended into the flood-basins. But such a flagrant negation of natural conditions was never possible before.

215 See paragraph 8.6.3, 328–9.

216 See above, 440–1 and notes 87–9.

217 A recent overview by Müller-Wille/Oldenstein 1981, esp. 280 f.

SAMENVATTING (*DUTCH SUMMARY*)

Het verschijnen van dit werk betekent de afronding van een studie waarbij voor de derde keer binnen 150 jaar de inheemse en Romeinse bewoning in het rivierengebied aan een samenhangend onderzoek onderworpen is. De eerste keer was al in 1838–9, toen dominee O.G. Heldring zijn *Wandelingen ter opsporing van Bataafsche en Romeinsche oudheden, legenden, enz.* publiceerde: in feite een van de eerste min of meer systematische archeologische veldverkenningen in Nederland. Ruim honderd jaar later volgde een serie artikelen waarin P.J.R. Modderman, naar aanleiding van de al tijdens de oorlogsjaren van '40–'45 begonnen bodemkartering, een overzicht gaf van de vroegere bewoning in relatie tot het landschap.
Deze studies, en al wat er in deze en in de vorige eeuw in met name het oosten van het Nederlandse rivierengebied aan opgravingen werd verricht, hebben aangetoond dat de streek in de Romeinse tijd niet alleen dicht bewoond was maar ook dat de sporen van deze bewoning in het algemeen goed bewaard zijn gebleven. Ze bleken bovendien relatief eenvoudig op te sporen. Deze wetenschap vormde in feite de reden voor het nu afgeronde onderzoek, al was de aanleiding een andere. Die werd gevormd door de sinds 1951 vrijwel ononderbroken voortgaande ROB opgravingen in Nijmegen. In de zeventiger jaren werden deze breder van opzet en verschoof het accent van een vooral op Romeinse militaire forten en kampementen gerichte belangstelling naar een onderzoeksstrategie waarin juist de nadruk kwam te liggen op de omringende, civiele bewoning en de veranderingen daarin in de loop van de tijd.
Dit deed de noodzaak gevoelen om de gegevens uit Nijmegen, in de Romeinse tijd het militaire en bestuurlijke centrum van de *civitas*, het departement of stamgebied van de Bataven, te plaatsen tegen de achtergrond van dat gebied. Uiteraard was dit mede het resultaat van nieuwe ontwikkelingen in het vak archeologie sinds het eind van de zestiger jaren die, onder meer via regionaal onderzoek, leidden tot pogingen om een completer beeld van het verleden te krijgen en met archeologische middelen uitspraken te doen over, bijvoorbeeld, sociale en economische patronen en structuren.
In dit verband is het vermeldenswaard dat de huidige studie en de beide eerdere heel duidelijk de ontwikkeling van de archeologie als wetenschap illustreren. Heldring is een typische, zij het zeker vooruitstrevende representant van de 19e-eeuwse oudheidkunde. Het werk van Modderman is een wetenschappelijk onderzoek dat past in de typisch Nederlandse traditie waarvoor tus beide wereldoorlogen door Van Giffen de basis w legd. Het is een gedegen studie van vindplaatsen verspreiding, het aanwezige vondstmateriaal en ook de relatie tussen bewoning en landschap, toer een belangrijk nieuw onderzoeksaspect en uiteraa steeds van essentieel belang.
De nu gereedgekomen studie poogt daaraan het van de relatie met het 'sociale' landschap toe te v en dat is tevens de reden waarom het oostelijk ri gebied in de Romeinse tijd een in de Nederland houdingen zeer geschikt onderzoeksterrein vorn de eenvoudigste uitspraken te kunnen doen over schappelijke structuren en processen zoals de Ron ring van de inheemse bevolking, zijn al zeer veel verschillende gegevens nodig. Het vele al gedane zoek in de streek rond Nijmegen maakt deze dus relatief aantrekkelijk onderzoeksgebied vanuit ee theoretisch perspectief. Relatief, want uiteraard l ook hier veel te wensen over en zal er in de toekon heel wat gegraven en onderzocht moeten worden. Een heel ander voordeel bij een archeologisch ond dat poogt de materiële gegevens ook te gebruike conclusies omtrent immateriële zaken is de aan heid van althans enige historische informatie. zijn immers, met de nodige voorzichtigheid, he aanwijzingen te vinden die in combinatie met in uit de sociologie en antropologie veel extra aankn punten leveren.
Bij al dit soort overwegingen blijft natuurlijk o dat de basis gevormd moet worden door zo veel m relevante archeologische en andere gegevens; 'feit men wil. Deze gegevens en hun directe interpre de zin van chronologische positie, functie, bodem ge betekenis en dergelijke zijn het onderwerp van (hoofdstuk 1–6). Hoofdstuk 2 is gewijd aan een historische gegevens in verband met de periodizer onderverdeling van het totale bestudeerde tijdstr een aantal analytisch relevante fasen. Naast de (12 v.Chr. – ± 50 na Chr.), Midden- (± 50 – Chr.) en Laat-Romeinse tijd (± 270 – 425 na Ch dat ook de Late IJzertijd vanaf ± 250 v.Chr., en rovingische periode tot ± 750 na Chr. Immers, w het doel is meer te begrijpen van de toestanden wikkelingen gedurende de Romeinse tijd dan noodzakelijk ook enig begrip te hebben van de voor de komst van de eerste legioenen en te wet het effect was van het weer los raken uit het verba de Romeinse staat.

Hoofdstuk 3 geeft vervolgens een uitvoerige studie van het landschap en de reconstructie daarvan in de Romeinse tijd. De chronologie van de nederzettingen en andere vindplaatsen, en een aantal gegevens over elke plek, zijn ondergebracht in de hoofdstukken 4 en 5. Hoofdstuk 6, tenslotte, is gewijd aan een soms meer soms minder gedetailleerde bespreking van een aantal vondstcategorieën die op de in totaal 549 behandelde sites (plaatsen van menselijke activiteit) zijn aangetroffen.

Hoofdstuk 7 vormt de verbinding van deel 2 met de eerdere hoofdstukken, en geeft tevens een nadere uitwerking van de reconstructie van het landschap in de Romeinse tijd, de daarbij behorende flora en fauna en de exploitatiemogelijkheden voor de mens. Hoofdstukken 4–6 worden nader uitgewerkt in 8, dat is gewijd aan de typologie van de sites. Daarin wordt, op basis van opgravingen en vondsten in en rond het rivierengebied en vergelijking met gegevens van elders, weergegeven welke soorten van nederzettingen, grafvelden, en andere plaatsen van menselijke activiteit onderscheiden kunnen worden. Bovendien wordt nagegaan wat de onderlinge verschillen kunnen betekenen, met name tussen de nederzettingen, en ten dele ook welke ontwikkeling daarin valt waar te nemen van de Late IJzertijd tot de Vroege Middeleeuwen. Deze materie wordt, tenslotte, verder uitgewerkt in de concluderende hoofdstukken 10–12.

Het tussenliggende hoofdstuk 9 is de publicatie van een kleine opgraving in Arnhem-Meinerswijk die in 1979 werd uitgevoerd om twee hoofdredenen: het onderzoeken van de aard van deze site, die een Romeins castellum bleek te zijn, en daarmee het testen van de procedure om op grond van toevalsvondsten het karakter van de bewoning te bepalen. In dit geval leverde dat een succes op en zoals besproken in paragraaf 8.4.3 bestaat er een gerede kans dat dit in de toekomst ook voor andere nederzettingsvormen kan worden toegepast.

De laatste drie hoofdstukken vormen, zoals gezegd, het voorlopige sluitstuk van het onderzoek. Voorlopig, omdat er veel in staat wat in de komende jaren zal moeten worden onderzocht en getoetst aan nieuwe gegevens. Dat kan gelukkig ook, omdat het project 'Oostelijk Rivierengebied en Maasdal' van de ROB juist ten doel heeft door opgravingen en veldkarteringen niet alleen Nijmegen maar ook haar achterland in de Romeinse tijd beter te leren kennen en begrijpen. De hier gepresenteerde conclusies en hypothesen kunnen daartoe een zinvolle leidraad zijn, om gericht te kunnen werken, en ze worden daarbij zelf verder onderbouwd en natuurlijk ook veranderd.

Hoewel ze vóór hoofdstuk 10 maar af en toe ter sprake komen is het onderzoek in feite gericht op Romeinen en Bataven en, in meer algemene termen, op allochtonen en autochtonen en hun interactie in een ruimtelijk en chronologisch beperkt gebied. In hoofdstuk 10 wordt daarvoor de basis gelegd en worden de ontwikkelingen onderzocht in de Late IJzertijd tot het moment, in 12 v.Chr., waarop de eerste grote Romeinse troepenmacht daadwerkelijk in het rivierengebied arriveert. De bevolking komt daarbij naar voren als een vrij egalitair georganiseerde tribale samenleving met een sociaal-economische structuur die, blijkens een combinatie van archeologische, historische, antropologische en ecologische gegevens, was gecentreerd rond een waarschijnlijk nogal extensieve vorm van veeteelt, overigens zonder dat er aanwijzingen zijn voor een nomadische levenswijze.

Caesar heeft op deze groepen nooit goed vat kunnen krijgen, als dat al de bedoeling was, maar onder Octavianus, de latere keizer Augustus, is men begonnen ook dit perifere gebied enigszins te controleren. Dit gebeurde door het sturen van met Rome geallieerde stammen. Voor het rivierengebied was dat een, waarschijnlijk uit Noord-Hessen afkomstige, groep Chatten die zich Bataven noemden of zo gingen heten toen ze in het gebied waren. Dit weten we overigens alleen uit de historische overlevering, want de archeologische aanwijzingen zijn nogal vaag. Ze betreffen echter wel een strategisch belangrijk areaal midden in het rivierengebied waar Maas en Waal dicht bijeen komen en de Brabantse riviertjes uitmonden in de Maas.

Na een theoretische beschouwing over imperialisme en kolonialisme en de specifiek Romeinse vorm daarvan komt vervolgens, in hoofdstuk 11, de eerste fase van werkelijke interactie tussen Bataven en Romeinen aan de orde. Vanaf 12 v.Chr. werden Bataven, vanwege hun krijgshaftige karakter, steeds ingeschakeld als irregulaire hulptroepen (*tumultuarii*) bij de verschillende campagnes om het vrije Germanië tot aan de Elbe te veroveren. Ook leverden ze, misschien al vanaf 38 v.Chr., tot aan de dood van Nero in 68 na Chr. het overgrote deel van de persoonlijke lijfwacht van Romeinse keizers. De onregelmatige krijgsdienst paste overigens goed bij hun sociaal-economische structuur die er, met de extensieve veehouderij, traditioneel op was ingesteld dat grote groepen mannen tijdens het hoogtepunt van het akkerbouwseizoen afwezig konden zijn.

Deze situatie, hoewel ze wederzijds niet echt onbevredigend zal zijn geweest, leidde vermoedelijk niet tot werkelijk diepgaande contacten en veranderingen, althans

niet indien de penetratie van de Romeinse materiële cultuur op inheemse sites daarvoor als maatstaf genomen wordt. De onderlinge verhouding veranderde echter toen, onder keizer Claudius, de veroveringen meer op Britannia werden gericht. Bataafse *tumultuarii* werden toen tot regulaire hulptroepen (*auxilia*) omgevormd en duizenden mannen, op een totale bevolking van tegen de 40 000, waren plotsklaps niet tijdelijk maar permanent van huis. Daar kwam nog bij dat de Rijn als grens werd ingericht, met een keten van forten, en dat een Romeinse bestuursstructuur steeds verder moet zijn doorgevoerd, met een vrij kunstmatige hoofdstad in Nijmegen. De daarop volgende ontwrichting van het traditionele inheemse samenlevingspatroon bleek een vruchtbare voedingsbodem voor een inheemse revolte, de Bataafse opstand van 69–70.

De onderdrukking daarvan leidde tot een verdere uitbouw van de Romeinse militaire en administratieve infrastructuur in het gebied onder de Flavische keizers: een groot investeringsprogramma dat in het laatste kwart van de 1e eeuw zo'n 1% van het totale inkomen van het hele Romeinse rijk opgeslokt kan hebben. Die tijd, nu met alle eigen troepen ver weg en onder Italiaanse officieren, een legioen (het 10e) in Nijmegen en vreemde hulptroepen, met name Thraciërs, in de grensforten, kan voor de inheemse bevolking niet bepaald plezierig zijn geweest. Archeologisch zijn er dan ook geen aanwijzingen dat ze profiteerde van alle investeringen.

Dat veranderde pas in de 2e eeuw, toen de 'Romeinse' input weliswaar verminderde maar er enerzijds een ook archeologisch aantoonbare opwaartse spiraal moet zijn ontstaan en anderzijds de nodige sociale veranderingen. Er kwam een eind aan een speciale en naar alle waarschijnlijkheid voor extensieve veeteelt typerende nederzettingsvorm. Ook in het rivierengebied begon in die zelfde tijd een nieuw soort nederzetting te floreren: de villa, ongetwijfeld in samenhang met de na 70 nieuw gebouwde hoofdstad, Ulpia Noviomagus Batavorum. Er zijn echter goede aanwijzingen dat de hele militair/administratieve structuur alleen dankzij externe impulsen kon functioneren en Nijmegen is noch als stad noch als regionaal centrum ooit echt volwaardig geworden. Inscripties tonen bijvoorbeeld aan, en dat is een grote uitzondering, dat de Bataven hun *civitas* vaak door kooplieden lieten besturen.

De relatieve welstand moet dan ook, behalve aan de verkeersgeografisch gunstige ligging van het gebied in de delta van belangrijke rivieren, vooral te danken zijn geweest aan wat, op archeologische gronden, een zich steeds verder ontwikkelende Bataafse soldatenkaste ge noemd kan worden, zeker in de eerste helft van de 3 eeuw. Toen daarna door interne troebelen en externe Germaanse, druk het Romeinse grensbeveiligingssys teem ineenstortte moet deze groep, tenminste ten zuide van de Waal, het stamgebied in stand hebben weten t houden. Korte tijd was het toen onderdeel van het Gall sche keizerrijk onder de usurpator Postumus, overiger waarschijnlijk zelf een Bataaf.

De op deze ontwikkeling volgende restauratie van daac werkelijke Romeinse invloed wordt behandeld in hoofc stuk 12. De sociale en economische structuur en mis schien ook zelfs de ethnische samenstelling van de Ba taafse bevolking verschilde toen (en eigenlijk al vanaf d latere 2e eeuw) wezenlijk van de groep die de veldhee Drusus in 12 v.Chr. moet hebben aangetroffen. Maar di bevolking ging wel een vergelijkbare functie vervulle door samen met de Germaanse Heruli, die in het rivie rengebied waren binnengedrongen, als een soort buffe te functioneren in het laat-Romeinse diepteverdedi gingssysteem. Beide groepen leverden ook weer hulp troepen en de bevolking, naar schatting nog zo'n 25% van het inwonertal (± 50 000) in de 2e eeuw, had ee nieuw, veel kleiner en zwaar versterkt centrum op e rond het Valkhof in Nijmegen.

In tegenstelling tot vroegere opvattingen kunnen we e van uitgaan dat de Romeinse macht zich onder Constan tijn en opvolgers wel degelijk uitstrekte tot aan de Rij en dat was ook zo na 350, toen grote groepen Germaner in de bronnen collectief aangeduid als Franken, Galli binnenvielen. Voor de Bataafse *civitas* betekende da echter wel het einde: in de bronnen horen we er niet meer van en ook archeologisch zijn, bijvoorbeeld in Nij megen, grote veranderingen aantoonbaar precies in dez tijd. De bevolking moet grotendeels vervangen zijn doc Franken. Voor zover we weten kunnen dat alleen maa de Salii geweest zijn die ergens uit de buurt van het hui dige Salland gekomen zullen zijn. Indien we de vanaf d 3e eeuw voorkomende term Batavia gelijkstellen met d *civitas Batavorum* die zich, zoals in hoofdstuk 11 beargu menteerd, naar het zuiden uitstrekte tot aan de Kemper dan kan zoals in hoofdstuk 12 wordt gedaan na 350 du gesproken worden van Batavia Salica.

Deze Saliërs gingen dezelfde functie vervullen als de Ba taven vóór hen, en de hernieuwde uitbouw van het diep teverdedigingssysteem onder Julianus en Valentinianu is ook in het rivierengebied archeologisch aantoonbaar Eveneens aangetoond is al eerder dat de bevolking in he rivierengebied opnieuw een soldatenkaste telde, ditmaa

met nauwe relaties met stammen ver in het noorden tot in het gebied tussen Weser en Eems. Vanwege de noodzakelijke graantransporten van Britannia naar het Rijnland was controle van het rivierengebied ook van groot belang. Er zijn dan ook allerlei aanwijzingen dat zich, misschien gestimuleerd door deze transport-route, een bloeiende samenleving ontwikkelde met een vermenging van Germaanse en Romeinse elementen.
De definitieve ineenstorting van het west-Romeinse rijk, die kort na het begin van de 5e eeuw aanving, veranderde daarin niets. De Salii breidden hun invloed juist verder zuidwaards uit en kwamen later zelfs bovendrijven als de belangrijkste groep in het Merovingische koninkrijk. Misschien ten dele daardoor, en zeker door het wegvallen van haar verkeersgeografisch gunstige positie in de nieuwe situatie, werd het rivierengebied in de 5e eeuw een onbelangrijk achterland. Er zijn verschillende soorten gegevens waaruit blijkt dat de streek nog maar dun bevolkt was, en de natuurlijke vegetatie kon zich herstellen tot een niveau dat sinds de Bronstijd niet meer bereikt was. Pas vanaf de late 6e eeuw ging de ontwikkeling weer opwaarts en werd het rivierengebied opnieuw belangrijk en onderging een integratieproces in het Merovingische rijk. Daarmee begon in feite een nieuwe cyclus van ontwikkelingen die, bijvoorbeeld, leidde tot de opkomst van het Karolingische Dorestad als internationaal handelscentrum; deze episode valt evenwel buiten het perspectief van het hier samengevatte onderzoek.

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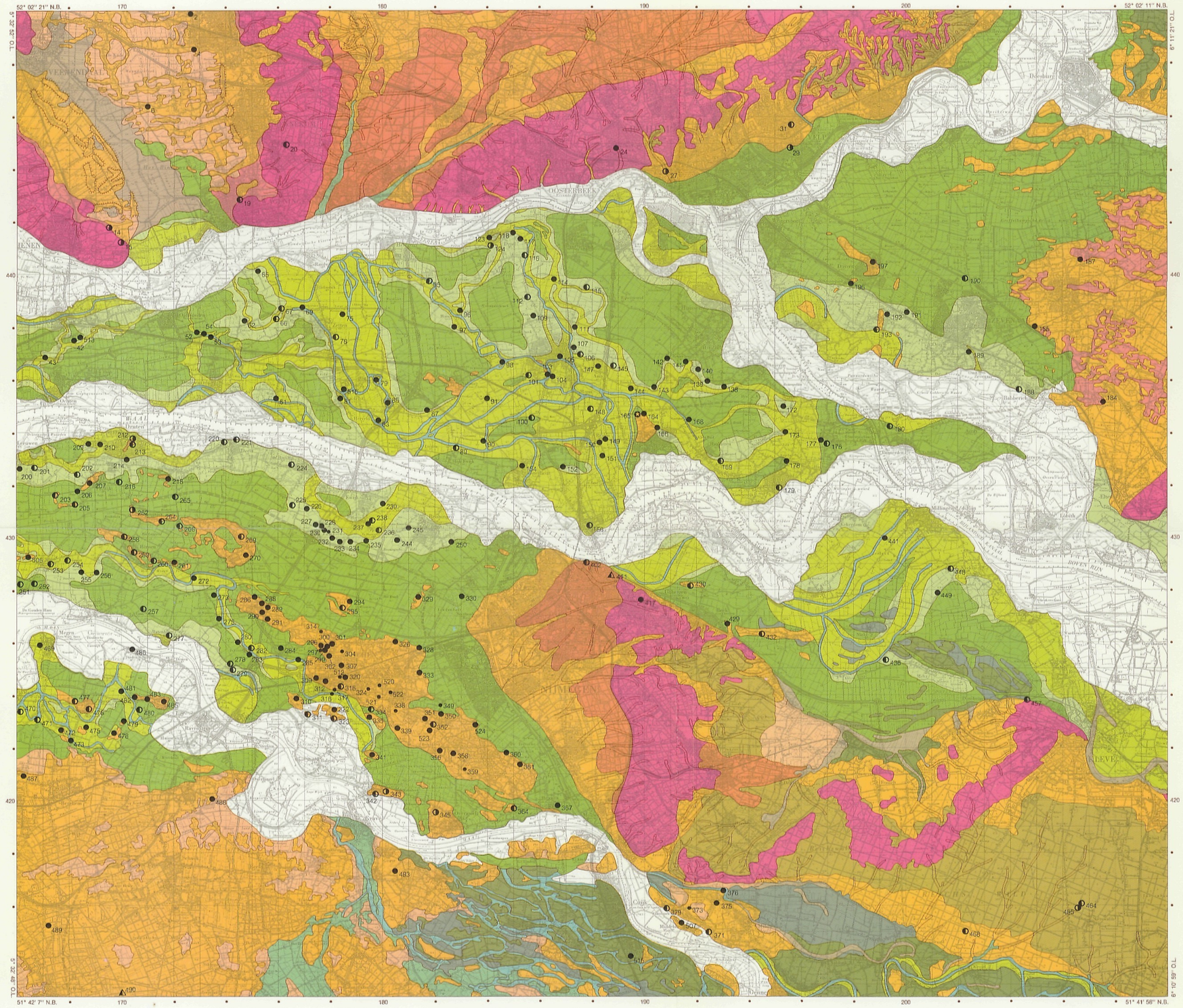
Archeologische kaart van Nederland 1:100.000
Archaeological Map of the Netherlands 1:100,000

Blad: Oostelijk rivierengebied
Sheet: Eastern River Area

Late IJzertijd
Late Iron Age

Archeologische vindplaatsen
Archaeological Sites

Appendix 1. Geological situation and archaeological sites of the Late Iron Age in the eastern river area.



LEGENDA GEOLOGIE
LEGEND GEOLOGY

Holocene afzettingen
Holocene Deposits

- Na-Romeinse stroomafzettingen en huidige rivierlopen
  Post-Roman channel deposits and present river courses
- Rivierloop of restgeul
  Channel or fossil channel
- Voor-Romeinse en Romeinse stroomafzettingen
  Pre-Roman and Roman channel deposits
- Voor-Romeinse en Romeinse oeverafzettingen
  Pre-Roman and Roman bank deposits
- Komafzettingen
  Flood-basin deposits
- Veen
  Peat
- Beekafzettingen
  Brook deposits

Pleistocene afzettingen
Pleistocene Deposits

- Rivierafzettingen, relatief laag gelegen oppervlak
  Fluvial deposits, relatively low lying surface
- Rivierafzettingen, relatief hoog gelegen oppervlak
  Fluvial deposits, relatively high lying surface
- Dekzand, relatief laag gelegen oppervlak; mogelijk bedekt met veen
  Coversands, relatively low lying surface; possibly covered by peat
- Dekzand, relatief hoog gelegen oppervlak; rivierduinen zijn inbegrepen
  Coversands, relatively high lying surface; river dune sands included
- Dekzandkopje of -rug
  Coversand hill or ridge
- Löss
  Loess
- Fluvioperiglaciale afzettingen
  Fluvio-periglacial deposits
- Fluvioglaciale afzettingen
  Fluvioglacial deposits
- Stuwwal
  Ice-pushed ridge

Diversen
Miscellaneous

- Droog dal
  Dry valley

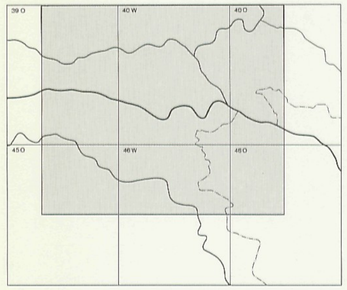
LEGENDA ARCHEOLOGIE
LEGEND ARCHAEOLOGY

Nederzettingen
Settlements

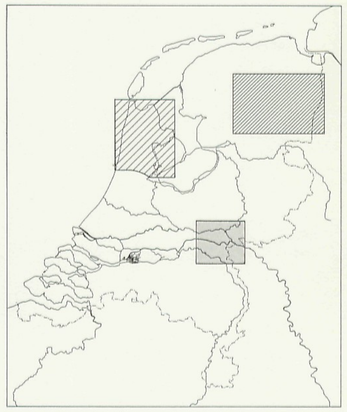
- Nederzetting
  Settlement
- Waarschijnlijke nederzetting
  Probable settlement
- Mogelijke nederzetting
  Possible settlement

Overige vindplaatsen
Other sites

- Vindplaats van bijzondere aard
  Special purpose site
- Waarschijnlijk klein grafveld of graf
  Probable small cemetery or single grave
- Geïsoleerde vondst(en)
  Isolated find(s)



Bladen van de Topografische Kaart van Nederland 1:50.000
Sheets of the Topographic Map of the Netherlands 1:50,000



Bladen van de Archeologische kaart van Nederland; IJzertijd
Sheets of the Archaeological Map of the Netherlands; Iron Age

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Topography: scaled down version of the topographic map 1:50,000, edited 1977-78.

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Druk/Printed by: Europeesch Cartografisch Instituut (ECI), Rijswijk (Z.H.).

Archeologische kaart van Nederland 1:100.000
Archaeological Map of the Netherlands 1:100,000

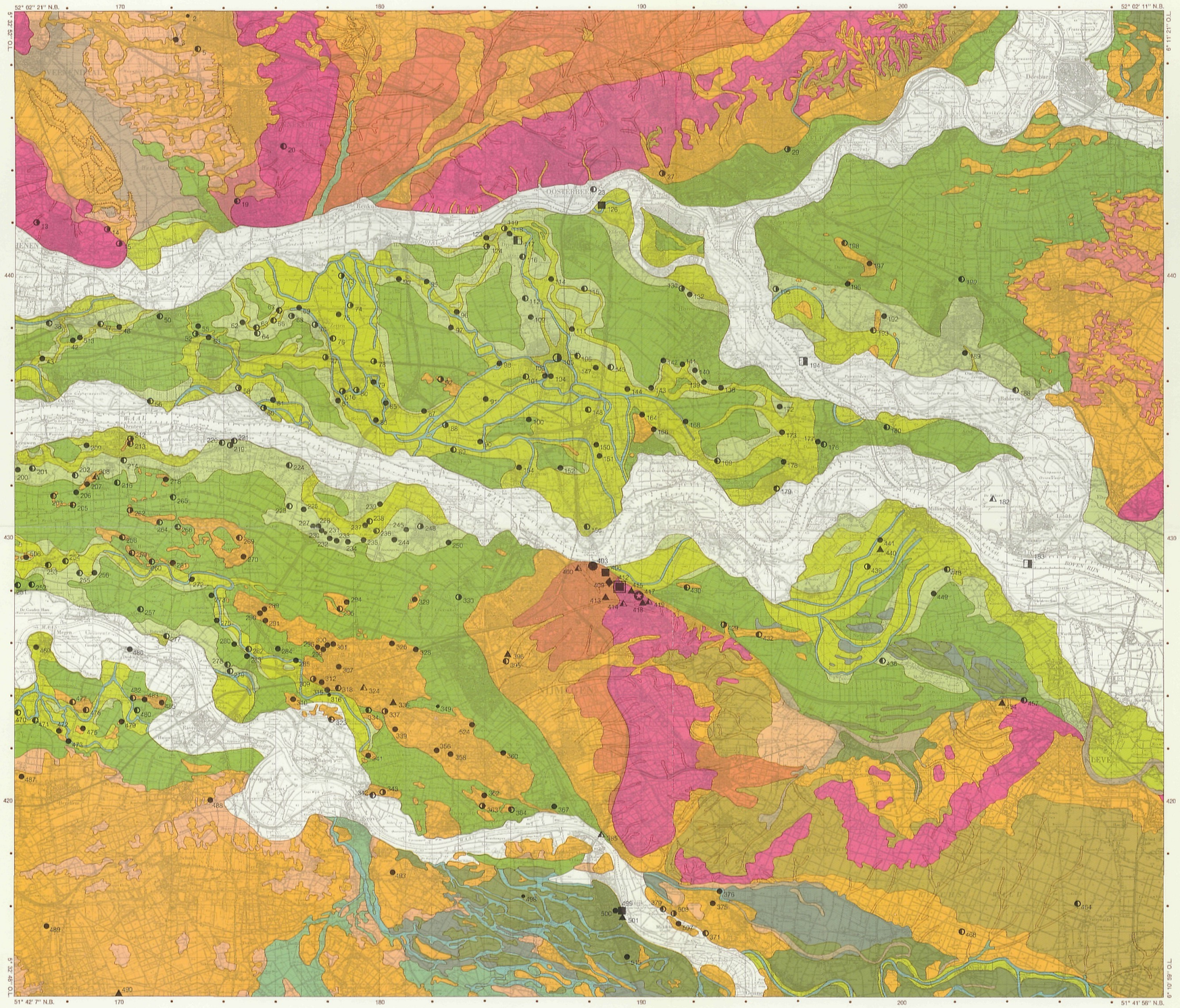
Blad: Oostelijk rivierengebied
Sheet: Eastern River Area

Vroeg-Romeinse Tijd
Early-Roman Period

Archeologische vindplaatsen
Archaeological Sites

2

Appendix 2. Geological situation and archaeological sites of the Early-Roman Period in the eastern river area.



LEGENDA GEOLOGIE
LEGEND GEOLOGY

Holocene afzettingen
Holocene Deposits

- Na-Romeinse stroomafzettingen en huidige rivierloopen
  Post-Roman channel deposits and present river courses
- Rivierloop of restgeul
  Channel or fossil channel
- Voor-Romeinse en Romeinse stroomafzettingen
  Pre-Roman and Roman channel deposits
- Voor-Romeinse en Romeinse oeverafzettingen
  Pre-Roman and Roman bank deposits
- Komafzettingen
  Flood-basin deposits
- Veen
  Peat
- Beekafzettingen
  Brook deposits

Pleistocene afzettingen
Pleistocene Deposits

- Rivierafzettingen, relatief laag gelegen oppervlak
  Fluvial deposits, relatively low lying surface
- Rivierafzettingen, relatief hoog gelegen oppervlak
  Fluvial deposits, relatively high lying surface
- Dekzand, relatief laag gelegen oppervlak; mogelijk bedekt met veen
  Coversands, relatively low lying surface; possibly covered by peat
- Dekzand, relatief hoog gelegen oppervlak; rivierduinen zijn inbegrepen
  Coversands, relatively high lying surface; river dune sands included
- Dekzandkopje of -rug
  Coversand hill or ridge
- Löss
  Loess
- Fluvioperiglaciale afzettingen
  Fluvio-periglacial deposits
- Fluvioglaciale afzettingen
  Fluvioglacial deposits
- Stuwwal
  Ice-pushed ridge

Diversen
Miscellaneous

- Droog dal
  Dry valley

LEGENDA ARCHEOLOGIE
LEGEND ARCHAEOLOGY

Militaire nederzettingen
Military Settlements

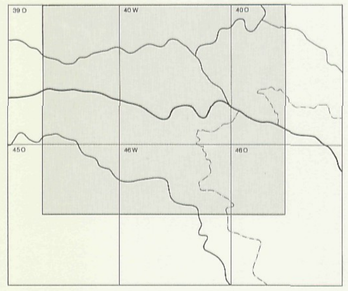
- Legioensvesting
  Legionary fortress
- Fort
  Fort
- Waarschijnlijk fort
  Probable fort
- Mogelijk fort
  Possible fort

Burgelijke nederzettingen
Civil settlements

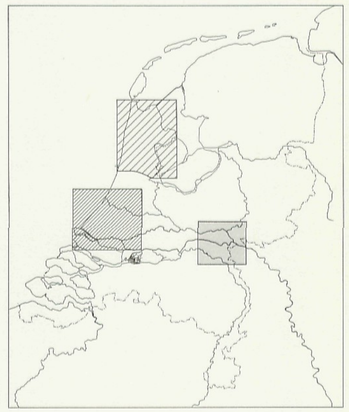
- Grote nederzetting
  Large settlement
- Mogelijk grote nederzetting
  Possible large settlement
- Nederzetting
  Settlement
- Waarschijnlijke nederzetting
  Probable settlement
- Mogelijke nederzetting
  Possible settlement
- Nederzetting van bijzondere aard
  Special purpose settlement

Overige vindplaatsen
Other Sites

- Grafveld
  Cemetery
- Klein grafveld of graf
  Small cemetery or single grave
- Waarschijnlijk klein grafveld of graf
  Probable small cemetery or single grave
- Geïsoleerde vondst(en)
  Isolated find(s)



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Bladen van de Archeologische kaart van Nederland; Romeinse Tijd
Sheets of the Archaeological Map of the Netherlands; Roman Period

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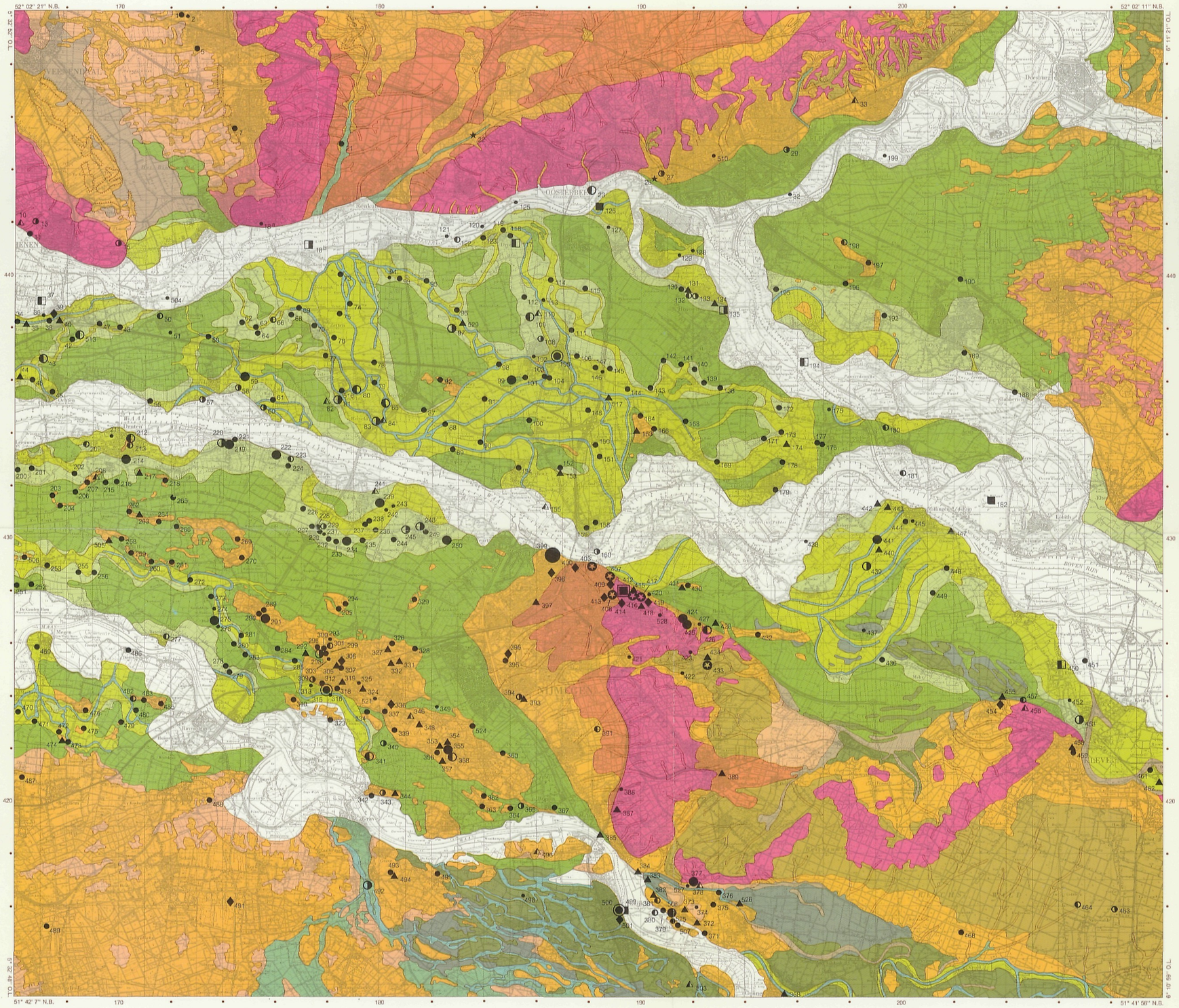
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Archeologische kaart van Nederland 1:100.000
Archaeological Map of the Netherlands 1:100,000

Blad: Oostelijk rivierengebied
Sheet: Eastern River Area

Midden-Romeinse Tijd
Middle-Roman Period

Archeologische vindplaatsen
Archaeological Sites



LEGENDA GEOLOGIE
LEGEND GEOLOGY

Holocene afzettingen
Holocene Deposits

- Na-Romeinse stroomafzettingen en huidige rivierlopen
  Post-Roman channel deposits and present river courses
- Rivierloop of restgeul
  Channel or fossil channel
- Voor-Romeinse en Romeinse stroomafzettingen
  Pre-Roman and Roman channel deposits
- Voor-Romeinse en Romeinse oeverafzettingen
  Pre-Roman and Roman bank deposits
- Komafzettingen
  Flood-basin deposits
- Veen
  Peat
- Beekafzettingen
  Brook deposits

Pleistocene afzettingen
Pleistocene Deposits

- Rivierafzettingen, relatief laag gelegen oppervlak
  Fluvial deposits, relatively low lying surface
- Rivierafzettingen, relatief hoog gelegen oppervlak
  Fluvial deposits, relatively high lying surface
- Dekzand, relatief laag gelegen oppervlak; mogelijk bedekt met veen
  Coversands, relatively low lying surface; possibly covered by peat
- Dekzand, relatief hoog gelegen oppervlak; rivierduinen zijn inbegrepen
  Coversands, relatively high lying surface; river dune sands included
- Dekzandkopje of -rug
  Coversand hill or ridge
- Löss
  Loess
- Fluvioperiglaciale afzettingen
  Fluvio-periglacial deposits
- Fluvioglaciale afzettingen
  Fluvioglacial deposits
- Stuwwal
  Ice-pushed ridge

Diversen
Miscellaneous

- Droog dal
  Dry valley

LEGENDA ARCHEOLOGIE
LEGEND ARCHAEOLOGY

Militaire nederzettingen
Military Settlements

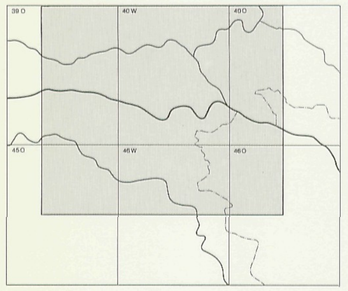
- Legioensvesting
  Legionary fortress
- Fort (en vicus)
  Fort (and vicus)
- Waarschijnlijk fort (en vicus)
  Probable fort (and vicus)
- Mogelijk fort (en vicus)
  Possible fort (and vicus)

Burgelijke nederzettingen
Civil Settlements

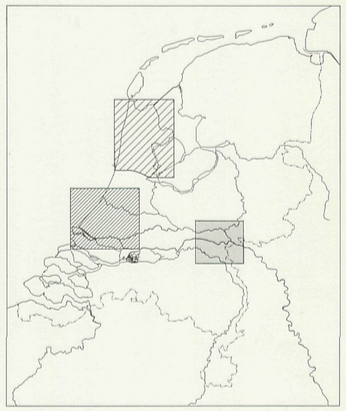
- Stad (Ulpia Noviomagus Batavorum)
  City (Ulpia Noviomagus Batavorum)
- Grote nederzetting met stenen gebouw(en)
  Large settlement with stone building(s)
- Nederzetting met stenen gebouw(en)
  Settlement with stone building(s)
- Nederzetting, waarschijnlijk met stenen gebouw(en)
  Settlement, probably with stone building(s)
- Nederzetting, mogelijk met stenen gebouw(en)
  Settlement, possibly with stone building(s)
- Nederzetting
  Settlement
- Waarschijnlijke nederzetting
  Probable settlement
- Mogelijke nederzetting
  Possible settlement
- Nederzetting van bijzondere aard
  Special purpose settlement

Overige vindplaatsen
Other Sites

- Grafveld
  Cemetery
- Klein grafveld of graf
  Small cemetery or single grave
- Waarschijnlijk klein grafveld of graf
  Probable small cemetery or single grave
- Geïsoleerde vondst(en)
  Isolated find(s)
- Depot, schatvondst
  Hoard



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Sheets of the Archaeological Map of the Netherlands; Roman Period

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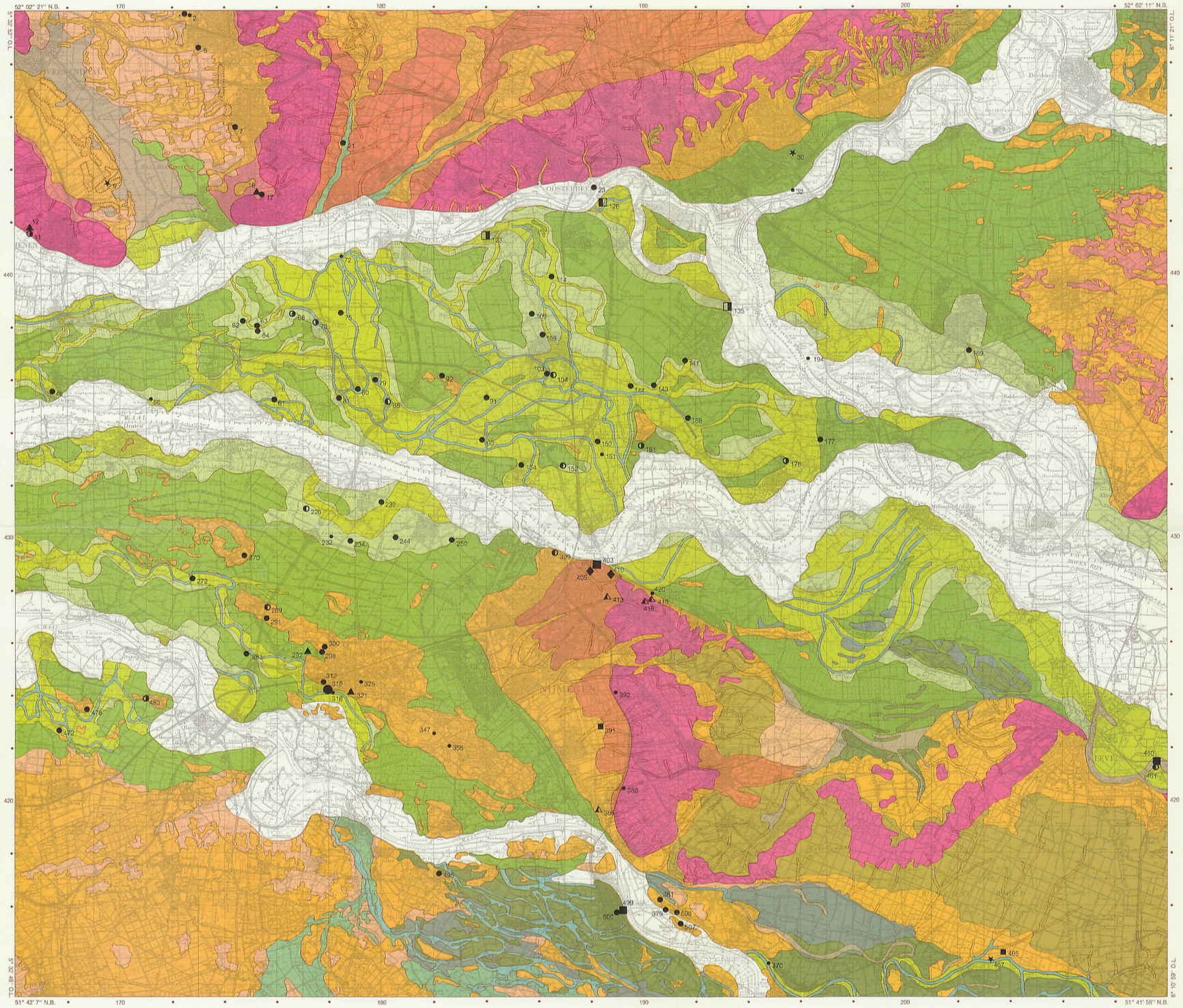
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Archaeological Map of the Netherlands 1:100,000

Blad: Oostelijk rivierengebied
Sheet: Eastern River Area

Laat-Romeinse Tijd
Late-Roman Period

Archeologische vindplaatsen
Archaeological Sites

Appendix 4. Geological situation and archaeological sites of the Late-Roman Period in the eastern river area.



LEGENDA GEOLOGIE
LEGEND GEOLOGY

Holocene afzettingen
Holocene Deposits

- Na-Romeinse stroomafzettingen en huidige rivierlopen
  Post-Roman channel deposits and present river courses
- Rivierloop of restgeul
  Channel or fossil channel
- Voor-Romeinse en Romeinse stroomafzettingen
  Pre-Roman and Roman channel deposits
- Voor-Romeinse en Romeinse oeverafzettingen
  Pre-Roman and Roman bank deposits
- Komafzettingen
  Flood-basin deposits
- Veen
  Peat
- Beekafzettingen
  Brook deposits

Pleistocene afzettingen
Pleistocene Deposits

- Rivierafzettingen, relatief laag gelegen oppervlak
  Fluvial deposits, relatively low lying surface
- Rivierafzettingen, relatief hoog gelegen oppervlak
  Fluvial deposits, relatively high lying surface
- Dekzand, relatief laag gelegen oppervlak; mogelijk bedekt met veen
  Coversands, relatively low lying surface; possibly covered by peat
- Dekzand, relatief hoog gelegen oppervlak; rivierduinen zijn inbegrepen
  Coversands, relatively high lying surface; river dune sands included
- Dekzandkopje of -rug
  Coversand hill or ridge
- Löss
  Loess
- Fluvioperiglaciale afzettingen
  Fluvio-periglacial deposits
- Fluvioglaciale afzettingen
  Fluvioglacial deposits
- Stuwwal
  Ice-pushed ridge

Diversen
Miscellaneous

- Droog dal
  Dry valley

LEGENDA ARCHEOLOGIE
LEGEND ARCHAEOLOGY

Militaire nederzettingen
Military Settlements

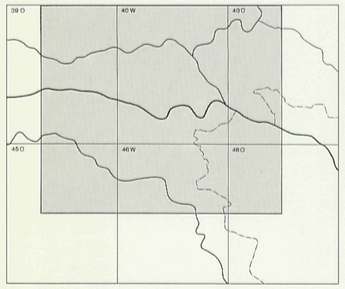
- Fort
  Fort
- Waarschijnlijk fort
  Probable fort
- Mogelijk fort
  Possible fort
- Klein fort
  Small fort

Burgelijke nederzettingen
Civil Settlements

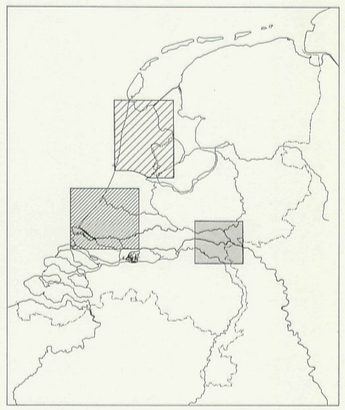
- Grote nederzetting
  Large settlement
- Nederzetting
  Settlement
- Waarschijnlijke nederzetting
  Probable settlement
- Mogelijke nederzetting
  Possible settlement

Overige vindplaatsen
Other Sites

- Grafveld
  Cemetery
- Klein grafveld of graf
  Small cemetery or single grave
- Waarschijnlijk klein grafveld of graf
  Probable small cemetery or single grave
- Geïsoleerde vondst(en)
  Isolated find(s)
- Depot, schatvondst
  Hoard



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Bladen van de Archeologische kaart van Nederland; Romeinse Tijd
Sheets of the Archaeological Map of the Netherlands; Roman Period

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Topografie: verkleinde topografische kaart 1:50.000, uitgave 1977-78.
Topography: scaled down version of the topographic map 1:50,000, edited 1977-78.

Ontwerp en uitvoering/Production: W.J.H. Willems, J.F. van Regteren Altena, J.C.A. Hulst, H.J. Bloklander.

Druk/Printed by: Europeesch Cartografisch Instituut (ECI), Rijswijk (Z.H.).

Archeologische kaart van Nederland 1:100.000
Archaeological Map of the Netherlands 1:100,000

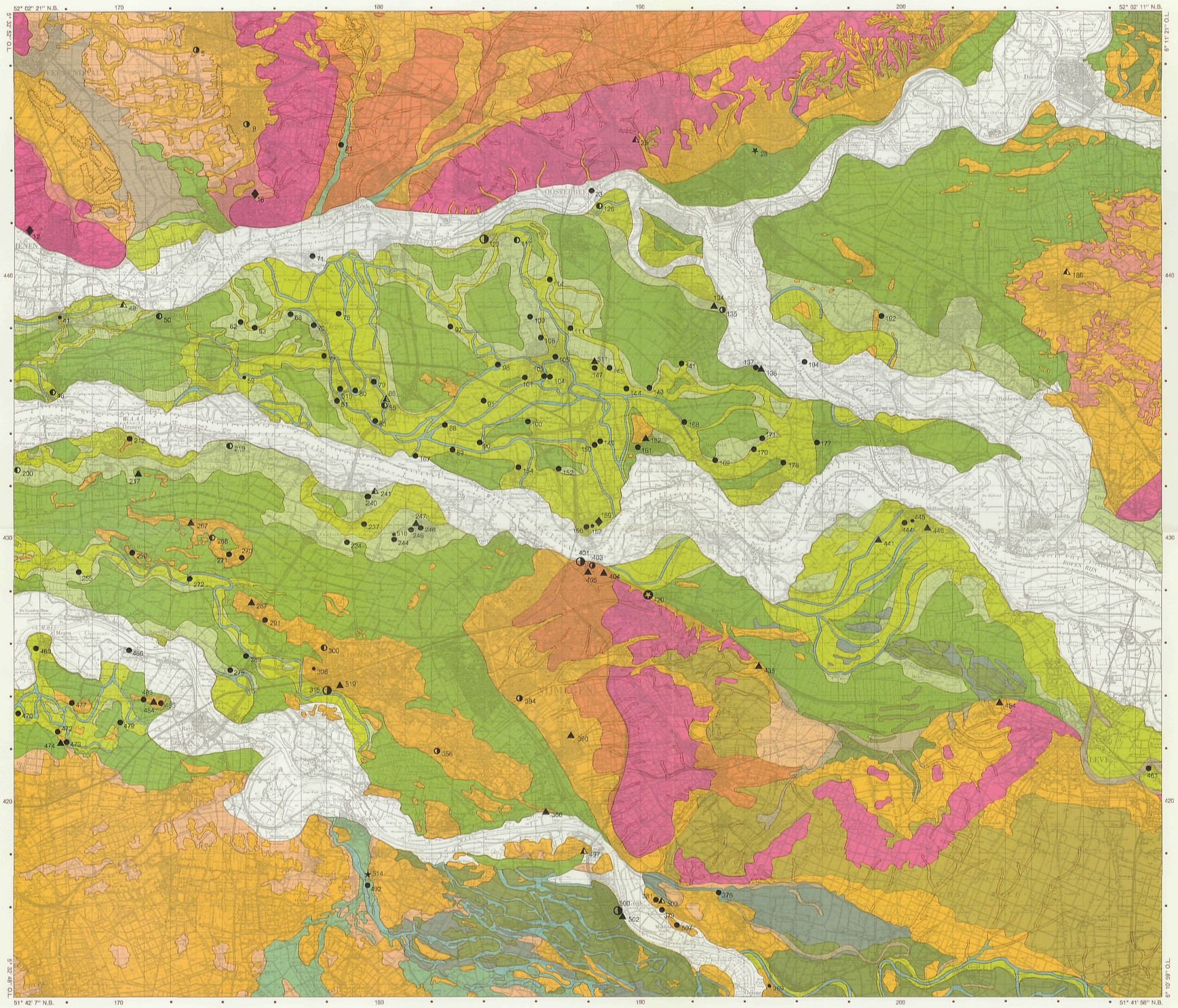
Blad: Oostelijk rivierengebied
Sheet: Eastern River Area

Merovingische Tijd
Merovingian Period

Archeologische vindplaatsen
Archaeological Sites

5

Appendix 5. Geological situation and archaeological sites of the Merovingian Period in the eastern river area.



LEGENDA GEOLOGIE
LEGEND GEOLOGY

Holocene afzettingen
Holocene Deposits

- Na-Romeinse stroomafzettingen en huidige rivierlopen
  Post-Roman channel deposits and present river courses
- Rivierloop of restgeul
  Channel or fossil channel
- Voor-Romeinse en Romeinse stroomafzettingen
  Pre-Roman and Roman channel deposits
- Voor-Romeinse en Romeinse oeverafzettingen
  Pre-Roman and Roman bank deposits
- Komafzettingen
  Flood-basin deposits
- Veen
  Peat
- Beekafzettingen
  Brook deposits

Pleistocene afzettingen
Pleistocene Deposits

- Rivierafzettingen, relatief laag gelegen oppervlak
  Fluvial deposits, relatively low lying surface
- Rivierafzettingen, relatief hoog gelegen oppervlak
  Fluvial deposits, relatively high lying surface
- Dekzand, relatief laag gelegen oppervlak; mogelijk bedekt met veen
  Coversands, relatively low lying surface; possibly covered by peat
- Dekzand, relatief hoog gelegen oppervlak; rivierduinen zijn inbegrepen
  Coversands, relatively high lying surface; river dune sands included
- Dekzandkopje of -rug
  Coversand hill or ridge
- Löss
  Loess
- Fluvioperiglaciale afzettingen
  Fluvio-periglacial deposits
- Fluvioglaciale afzettingen
  Fluvioglacial deposits
- Stuwwal
  Ice-pushed ridge

Diversen
Miscellaneous

- Droog dal
  Dry valley

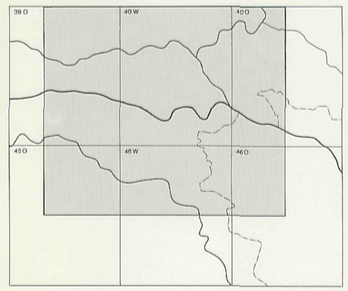
LEGENDA ARCHEOLOGIE
LEGEND ARCHAEOLOGY

Nederzettingen
Settlements

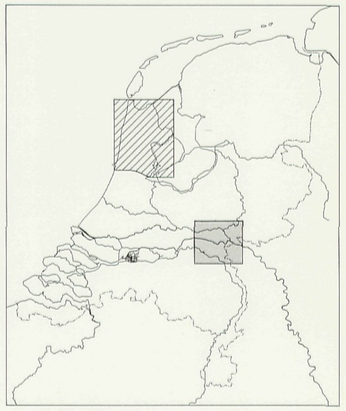
- Mogelijk grote nederzetting
  Possible large settlement
- Nederzetting
  Settlement
- Waarschijnlijke nederzetting
  Probable settlement
- Mogelijke nederzetting
  Possible settlement

Overige vindplaatsen
Other Sites

- Vindplaats van bijzondere aard
  Special purpose site
- Grafveld
  Cemetery
- Klein grafveld of graf
  Small cemetery or single grave
- Waarschijnlijk klein grafveld of graf
  Probable small cemetery or single grave
- Geïsoleerde vondst(en)
  Isolated find(s)
- Depot, schatvondst
  Hoard



Bladen van de Topografische Kaart van Nederland 1:50.000
Sheets of the Topographic Map of the Netherlands 1:50,000



Bladen van de Archeologische kaart van Nederland; Vroege Middeleeuwen
Sheets of the Archaeological Map of the Netherlands; Early Middle Ages

- Dit blad/*This sheet*
- Bladen in voorbereiding/*Sheets in preparation*

Bijlage bij/Appendix to: W.J.H. Willems, Romans and Batavians. A Regional Study in the Dutch Eastern River Area, I; in: *Berichten van de Rijksdienst voor het Oudheidkundig Bodemonderzoek* 31, 1981.

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Published in 1982 by the State Service for Archaeological Investigations, Kleine Haag 2, 3811 HE Amersfoort, the Netherlands.

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Appendix 6 to: W.J.H. Willems, Romans and Batavians. A Regional Study in the Dutch Eastern River Area, II, in: *Berichten van de Rijksdienst voor het Oudheidkundig Bodemonderzoek* 34, 1984.

Fig. 94 Overview of the excavation with, from top to bottom, the eastern profile, the plan at c. 45 cm below surface, a plan of various features in the southern part of the trench at c. 9 m NAP, and the western profile:

1 stone (mostly tuff blocks)

2 robber trench with fragments of stone and broken tiles

3 gravel

4 wood

5 sods

6 very dark fill with settlement debris

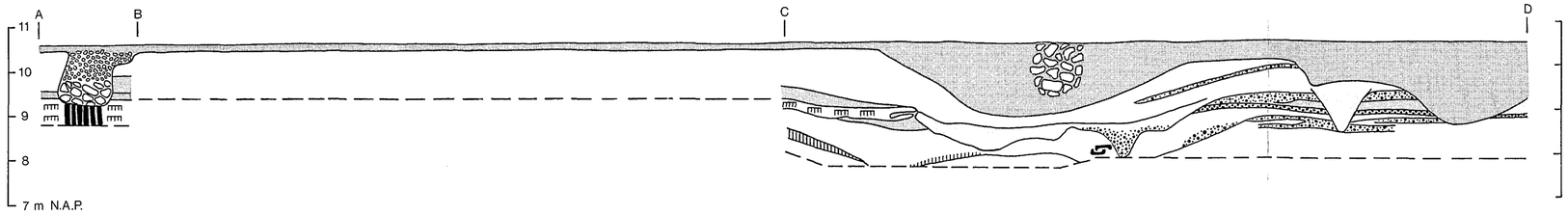
7 dark fill

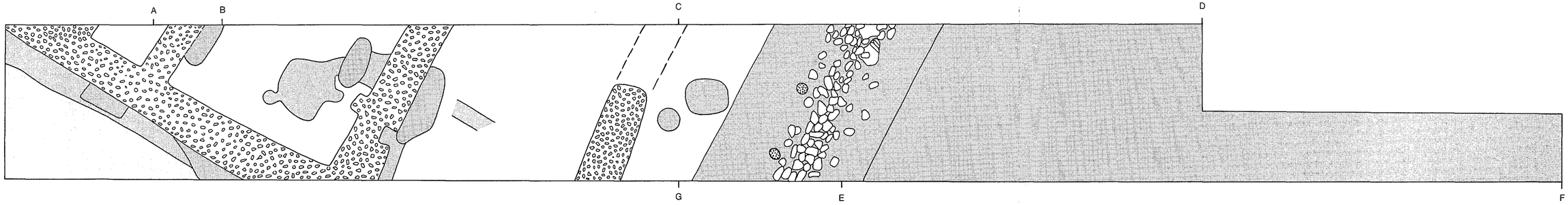
8 greyish fill

9 black layer or fill with charcoal

10 only slightly polluted brown or blueish layer or fill

11 humic brown sand







± 9 m N.A.P.

