The *Annales* School and Archaeology

Edited by
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Leicester University Press
(a division of Pinter Publishers)
Leicester, London

© Editor and contributors 1991
First published in Great Britain in 1991 by Leicester University Press
(a division of Pinter Publishers)

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Editorial offices
Fielding Johnson Building, University of Leicester, University Road,
Leicester, LE1 7RH, England

Trade and other enquiries
25 Floral Street, London, WC2E 9DS, England

British Library cataloguing in publication data
A CIP catalogue record for this book is available from the British Library.
ISBN 0-7185-1354-1

Typeset by Acorn Bookwork, Salisbury, Wiltshire
Printed and bound in the United States of America

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1 The contribution of an *Annaliste/* structural history approach to archaeology

John Bintliff

Introduction

How many times have I sat at the beginning of a symposium listening to the organizer setting out a new and trendy approach to transform the theory and practice of archaeology? More often than not, the new ideas are poached from other disciplines, and in presenting to you the importance of the *Annales'* paradigm, the fruits of France's leading group of historians, I am surely perpetuating an academic syndrome. Indeed, did not David Clarke at the beginning of our British version of New Archaeology (1968) and more recently Ian Hodder (1986), demand that archaeology create its own independent world of theory and practice?

Actually I should like to stand this argument on its head. Whilst agreeing that recovering and collating our data, primarily buried material culture, are tasks best left to our discipline to deal with, with the mutual agreement of other disciplines, I have exactly the opposite opinion about the interpretation of past societies from this and other evidence (such as standing structures and written sources). Here I can already introduce a fundamental battle cry of the *Annales'* scholars—the call for a truly interdisciplinary collaboration on exploring and analysing past societies. To the outsider, intellectual demarcation between subjects supposedly studying the same phenomenon—human societies—

seems bizarre, yet few scholars in archaeology, history, sociology, social anthropology, geography or psychology make any regular attempt to follow the literature and development of ideas and approaches across this spectrum of related disciplines that converge on human community studies.

You may ask: how can I have the time to read journals beyond my discipline, and in any case, who says I could learn anything useful by so doing? The answer to those questions is devastatingly simple: if we analyse the development of ideas in archaeology since the latter part of the nineteenth century, it is quite clear that the same underlying concepts and intellectual approaches characterize our subject, for periods of a decade or more, as can be found in the other disciplines; but usually intellectual movements in all these subjects are out of phase with one another. The diffusion time for a major new concept may take so long to pass, e.g. from sociology to archaeology, that by the time its potential is being proclaimed at, say, a TAG (Theoretical Archaeology Group) Conference, sociologists have exhausted its value and found it seriously wanting, and perhaps are already entering a new conceptual approach. This time-lag of innovation is certainly the case with the *Annales'* paradigm. The first moves were made towards this new approach in the discipline of history in France around the turn of the century, whilst the full-blown paradigm was victorious in the French history establishment by the late 1940s (cf. Stoianovitch 1976); Anglo–American enthusiasm amongst historians for *Annales'* scholarship can be dated to the early 1970s, and British geographers were recommending the *Annales'* approach as the solution to the so-called post-positivist *problématique* from the late 1970s. Meanwhile in contemporary archaeological debate we can still hear speakers or read articles forcefully arguing for the application of positivist New Archaeology in historical archaeology (and rightly so, for without it, post-positivism is irrelevant).

So the problem is simply poor communication and blinkered attitudes to our real context as one branch of the human sciences. By introducing the *Annales'* approach as a major theoretical initiative in archaeology, I am therefore not only aiming to help us catch up on trends visible some years ago in related disciplines, which were at that time facing the same intellectual problems that we are just becoming aware of, but also offering the *Annales'* paradigm as a powerful argument for a closer merging of sister disciplines (archaeology, history, social anthropology, sociology, geography, psychology, and so on), since the *Annales'* way of approaching the past blends all these subjects into a single, elaborate methodology for understanding pre-modern societies.

The 'crisis' in archaeological theory

Several prominent authorities since the early 1980s have identified a growing dissatisfaction with the rate of progress in our understanding of

past communities (Barrett 1983, Trigger 1984, and most eloquently, Ian Hodder 1986). Without rejecting the very clear achievements of the New Archaeology movement of the 1960s and 1970s, many scholars feel that this particular major initiative of disciplinary renewal has done its job and is rapidly running out of the potential to create new ideas and approaches. Moreover, there have arisen an impressive number of problems and limitations to our knowledge with which New Archaeology offers little help.

Careful study of several sister disciplines (Bintliff 1986) has shown a dominant trend where such dissatisfaction can be derived. History and geography both passed through a phase, beginning some ten years before New Archaeology (that of the New History and the New Geography), but likewise characterized by the urge to quantify, to emulate the hard sciences, to discover general laws or functions for human activity and the natural world. Generalizing and positivistic attitudes typified these movements of the 1960s to the mid-1970s. At the time the impact of the 'new' formats was overwhelmingly favourable in disciplines where all too often research work had atrophied into descriptive and particularizing approaches, occasionally enlivened by imaginative but poorly documented and unverifiable flights of literary insight that passed for interpretation.

However, by the late 1970s in most of our sister disciplines the demand began to be voiced for a new generation of theory to tackle the growing body of research problems beyond the scope of the 'New' format approaches. In particular we might mention:

1. That the typical models and processes that had proved so innovative in the 'New' subjects worked well at the level of the society and regional community, but had little to say about the individual in the present or the past
2. Likewise, that the 'New' subjects had been successful at analysing major trends and developments occurring over generations or centuries or even longer, but had shied away from dealing with short-lived events
3. That positivistic, pro-hard science 'new' format subjects assumed that data collection and interpretation were objective and 'for all time' rather than reflecting the personal, time-conditioned, subjective needs of individual researchers.

This package of criticisms forms a recognizable intellectual movement in our sister disciplines, and has belatedly been introduced into archaeology in the last few years under the terms 'post-positivism' or 'post-processualism' (Hodder 1986).

The special relevance of this brief profile of developments in the human sciences rests on the growing recognition in several of the relevant disciplines that the *Annales'* paradigm may offer a methodology

to tackle the critical agenda just listed, yet at the same time the *Annales'* methodology can be seen as complementary, rather than contradictory, to the central concepts and approaches of the 'New' format subjects (cf. for example, Hobsbawm 1980).[1]

It certainly can be said that although the traditional descriptive and narrative approaches, such as characterized archaeology, history and geography before the 1960s renewal programme, were limited in their theoretical underpinnings and offered little to each other in a broader understanding of the human species, yet they presented a view of the real world that was familiar and compatible with our experience of it. In contrast the 'New' format subjects revealed a whole new world of processual dynamics yet somehow failed to recreate a world as a 'participant/observer' would experience it. The *Annales'* paradigm succeeds in its best work precisely through its explicit combination of experienced life and externally analysed life.

The *Annales* paradigm: Part I (Figure 1.1)

Stoianovitch, in his book (1976) on the growth of the *Annales'* school, employs the Kuhnian paradigm model, with the approval of that central figure in the movement, Fernand Braudel. The pre-paradigmatic stage is typified by disruptive activity by the younger generation, unhappy with traditional French history, geography and social science. What is being rejected is the nineteenth-century German tradition of scholarship, with its emphasis on great men and the development of national character, or the determining influence of the physical environment on human

Pre-Paradigm: *Annales de Géographie* (1891–)
(Vidal de la Blache)
L'Année Sociologique (1896/8)
(Emile Durkheim)
Revue de Synthèse Historique (1900–)
(Henri Berr) → Marc Bloch, Lucien Febvre

Paradigm Coheres: *Annales d'Histoire Economique et Sociale* (1929–)
Feudal Society (Bloch) (1939–40)
The Problem of Unbelief in the Sixteenth Century (Febvre (1942)

Paradigm Triumphant: *La Mediterranée* (Fernand Braudel) (1949)
→ Emmanuel Le Roy Ladurie *The Peasants of Languedoc* (1966)
Montaillou (1975)
Carnival (1979)

Figure 1.1 The *Annales* paradigm.

affairs. Landmarks in a new perspective from around the turn of the century are the founding of *Annales de géographie* by Vidal de la Blache (1891), the *L' Année sociologique* by Durkheim (1896–8), and the *Revue de synthèse historique* by Henri Berr (1900). All three scholars proclaimed new generalizing disciplines. Two historians closely associated with Berr's journal were Marc Bloch and Lucien Febvre. Their enthusiasm for these new horizons focused particularly on demolishing the dominant political history of the Sorbonne and replacing it with social and economic history, and on opening up the practice of history to new intellectual ideas from neighbouring disciplines. Bloch indeed coined the term 'nouvelle histoire' (new history), and what he and Febvre were aiming for was to transform Anglo–American history only decades later, after the Second World War, into our own 'New History'.

In 1929 the *Annales*' paradigm cohered with the founding by Bloch and Febvre of the journal *Annales d'histoire économique et sociale*. This was later renamed *Annales, Economies, Sociétés, Civilisations*, as cultural forces became more important in the *Annales*' way of explaining history. In general, Bloch's subsequent work concentrated on social and economic history, as in his classic *Feudal Society* (1939–40), whilst Febvre pursued cultural history through an examination of the role of ideology and perceptions of the world in past societies—so-called *mentalités* (cf. his studies of Luther (1928) and *The Problem of Unbelief in the 16th Century* (1942)).

Although many of the new ideas of the *Annales*' approach to history were in place by the Second World War, the full triumph of the school over its rivals in France can be dated from 1949, the year of the publication of the most famous *Annales*' book, *The Mediterranean and the Mediterranean World in the Age of Philip II* by Fernand Braudel (a heavily revised edition becoming available in English only in 1972), the rising star of a second generation of *Annales*' historians. From then till the mid-1970s the *Annales*' approach has dominated French history, and increasingly influenced and interacted with Anglo–American history, especially through the subsequent founding of the latter's 'new history' movement from the 1960s, which shows many typical features of the *Annales*' programme. A cluster of prominent scholars, many belonging to a third generation of *Annales*' scholarship, has maintained the paradigm's eminence, e.g. Jacques le Goff, and, in particular, Le Roy Ladurie, whose meticulous studies of medieval life have become popular best-sellers (*Montaillou* 1975, *Carnival* 1979).

I want to deal rapidly with a first group of key ideas associated with the *Annales*. We have already seen the stress on generalization, the search for wider insights believed to be attainable beyond observing the unique and particular. This is intimately tied to an explicit call for interdisciplinary collaboration between all those disciplines concerned with human society—a new human science.[2] The former reliance on scholarly authority and textual criticism of the sources is to be replaced

with the search for vast new data bases amenable to statistical treatment, e.g. tax documents, parish records. The traditional emphasis on great men and battles must yield to the trends of population demography, the analysis of class structure, patterns of diet and health.

These aspects of the *Annales'* programme are familiar to us from our own New Archaeology movement of recent decades, and produced an equivalent rash of insights and new worlds to discover and conquer within French and later international history. However, we must not forget that these things were happening before the Second World War with Bloch and Febvre.

The *Annales* paradigm: Part II (Figure 1.2)

Let us move on to one of the key concepts developed by the *Annales'* school, one which has had negligible effect hitherto on archaeology: this is the powerful model of time, or more accurately *duration* (*durée*) explored in unbelievable detail by Braudel in his book *The Mediterranean and the Mediterranean World in the Age of Philip II* (1949).

Braudel sees historical time as dominated by at least three groups of processes moulding the visible development of human societies. All three operate contemporaneously but at different wavelengths in time. The reality observed when we reveal how a particular era or region

HISTORY OF EVENTS	SHORT TERM—EVENEMENTS	Narrative, Political History; Events; Individuals.
STRUCTURAL HISTORY	MEDIUM TERM—CONJONCTURES	Social, Economic History; Economic, Agrarian, Demographic Cycles; History of eras, regions, societies; Worldviews, ideologies, (*Mentalités*).
	LONG TERM—STRUCTURES OF THE 'LONGUE DUREE'	Geohistory: 'enabling and constraining'; History of civilizations, peoples; Stable technologies, world views (*Mentalitiés*).

Figure 1.2 Braudel's model of historical time.

underwent historical change is the final result of an inner dialectic between these different temporalities.

Braudel's *The Mediterranean* is a vast undertaking explicitly organized according to this scheme. Part I deals with the physical geography of the Mediterranean and the constraints and possibilities it offers for human development. To quote Febvre:

> he first studies the permanent forces that operate upon the human will and weigh upon it without its knowledge, guiding it along certain paths; thus we have an entire analysis never attempted before of what we mean when, almost negligently, we pronounce the word 'Mediterranean', and it is seen as a guiding force, channelling, obstructing, slowing-down and checking or, on the other hand, heightening and accelerating the interplay of human forces. (Febvre 1949 (1973), p. 37)

Such forces act at the longest wavelength of time, so that change in them is almost imperceptible—hence Braudel refers to them as the dynamic of long duration or *longue durée*. Long-term forces can include dominant and slowly changing technologies or persistent cultural features such as ideologies and world views (*mentalités*).

Part II of *The Mediterranean* deals with forces—so-called *conjonctures*, moulding human life, which operate over several generations or centuries, the medium term or *moyenne durée* (to use Hexter's (1972, p. 504, n. 31) term by analogy). Common examples are demographic, agrarian and other economic cycles, and the waxing and waning of socio-political systems. To quote Febvre again: 'These are impersonal, collective forces, but this time they are dated . . . as being the very ones which operated in the sixteenth century' (p. 37).

Both long and medium-term dynamics are largely beyond the perception of past individuals, they act as structures—from which has arisen the term structural history for this model—which form a constraining and enabling framework for human life, communal and individual.

Part III of *The Mediterranean* is the world of *events* (*événements*)—the record of human actions and individual human personalities, of participant/observer experience of the past, Ranke's 'wie es eigentlich gewesen war', in other words the world of the past as analysed by traditional political and narrative history. Febvre again:

> The third part—events, the tumultuous, bubbling and confused flood of events, often directed by the permanent forces studied in the first part and influenced and governed by the stable forces listed in the second part, only here chance comes into play embroidering her most brilliant and unexpected variations on the loom of events. (1949 (1973), p. 37)

This ambitious programme of Braudel offers a way forward for several of the central problems posed recently by the so-called Post-positivist critique in geography, history, archaeology and other human sciences.

As a theory of how the world works, and how we can reconcile in a single methodology the general and particular, the event and the millennial trend, the individual and the community or society, Braudel's Structural History is a landmark with inexhaustible potential.

However, we have to add immediately that for most specialist commentators, for all its encyclopaedic brilliance, Braudel's *The Mediterranean* fails to carry through the intentions of the model, and what is critical, it is exactly in the relations between the particular and the general, the short-term and the longer term, that the book is found wanting. Thus Hexter has written:

> The book falls short of its author's intention in one major respect: it does not solve the historiographical problem that it poses: how to deal with the perennial historiographic difficulty of linking the durable phenomena of history with those that involve rapid change. (1972, p. 533)

Significantly, it is Braudel's disciple Le Roy Ladurie who penned the following insights in 1972, looking back on a generation of *Annales'* scholarship moulded by Braudel's neglect of the world of events and individuals:

> Present-day historiography, with its preference for the quantifiable, the statistical and the structural, has been obliged to suppress in order to survive, which is a pity. In the last few decades it has virtually condemned to death the narrative history of events and the individual biography . . .. The Muse of History . . . has turned towards the study of structures, the persistent patterns of the 'long-term', and the collection of data amenable to serial or quantitative analysis.
>
> In France these preferences, now firmly established, first appeared in the work of Bloch, Febvre and their friends, disciples or successors in the Annales school of history. Fernand Braudel, when he was writing *The Mediterranean*, relegated the events of war and diplomacy to the final section: the heart of the book is essentially the archaeology of a sea—with its strata of millennial or merely secular temporality.
>
> [Yet] even the most logically composed structures (in rural history for instance, where these things are less complicated than elsewhere) have their phases of disequilibrium, their swings and cycles, their moments of reversal or restoration, and their secular pendulum movements which can be regarded as the very stuff of today's historical narratives. (1972 (1979, pp. 111,113))

Ladurie praises the positive achievements of Structural History, but none the less is moved to comment:

> such victories at the frontiers of knowledge, won by the historians of the last half-century, are irreversible; but they would be even more satisfactory if history really was entirely logical, intelligible and predictable from start to finish; if the event or chance happening really could be exorcized once and for

> all . . . [T]he . . . transition from one structure to another, the mutation, often remains, in history as in biology, the most perplexing zone, where chance appears to play a large part. (op.cit. pp. 113,114)

A second, vital criticism of Braudel's application of structural history is that he chooses to see the fundamental structures of medium and long-term as geographic constraints and stimuli or the impersonal swings of population history and economic cycles. The world of *mentalités*, of collective ideologies, is little explored.[3]

Actually Braudel was fully aware of his neglect of the short-term and the individual, indeed could be said to have done so deliberately in response to his philosophy of life. The 'event' is not exactly ignored in *The Mediterranean*, Part III, but is explained only by reference to conjunctures and long-term structures. The short-term, to quote from the book itself, is life: 'as it was felt, described and lived by contemporaries whose lives were as short and short-sighted as ours . . .. Resounding events are often only momentary outbursts, surface manifestations of larger movements and explicable only in terms of them' (Braudel 1972, p. 21).[4] And in a more obviously philosophical vein, Braudel writes:

> confronted by man, I am always tempted to see him as enclosed in a destiny which he scarcely made, in a landscape which shows before and behind him the infinite perspectives of the *longue durée*. In historical explanation as I see it, at my own risk and peril, it is always the *temps long* that ends up by winning out. Annihilating masses of events, all those that it does not end up by pulling along in its own current, surely it limits the liberty of men and the role of chance itself. By temperament I am a structuralist, little attracted by events and only partly by conjuncture. (Braudel 1972, p. 520)

Hexter (1972, pp. 504–10) has not implausibly linked this fatalistic philosophy of Braudel to his prolonged imprisonment by the Germans during the Second World War, during which indeed he wrote the main draft of his masterpiece. Condemned to inaction his reflection may have turned to the more durable features of human existence that extend beyond the short and often violent lives of individuals. Not surprisingly Braudel had little sympathy for the contemporary philosophy of Jean-Paul Sartre, with its emphasis on the event as the essence of human being; while Braudel languished in a prison camp, Sartre was active in the intense day-to-day drama of the French Resistance.

The *Annales* paradigm: Part III

The *Annales'* school contains a wide range of opinion and a diversity of approaches, although Structural History is a core concept for most of its research. The problems just outlined have in fact been much more

effectively tackled by other well-known *Annales'* historians before and after Braudel's *The Mediterranean*.

None the less, before we turn to these authors for help in our current difficulties, we must pause to reflect that dissatisfaction with Braudel's view of life is in itself a statement of ideology, an expression of world views that allow more scope to events and individuals in forming history than Braudel acknowledges. Braudel, adequately for his philosophy, employs participant observers in the sixteenth-century Mediterranean in the role of demonstrating the effect of long and medium-term forces; indeed his book has been described as a wonderful picaresque tour around the Mediterranean in all its physical and human complexity.

Mentalités (Figure 1.3)

The first complementary approach to Structural History amongst the *Annales'* group that we shall look at is the research done on *mentalités*: ideologies, collective systems of belief, world views, which both reflect and can transform human life in the opinion of *Annales'* scholars other than Braudel. Mutual feedback between what human groups think and believe and historical processes on all three time levels of Structural History is a key concept taking us beyond the mechanistic and deterministic tendencies of both Braudelian history and New Archaeology.[5]

Already in the work of Berr who founded the *Revue de Synthèse Historique* (1900–) we can find a strong interest in intellectual history and psychohistory. Febvre developed this into the formal term *mentalités*, showing again and again how contrasted past societies could be, one to another, and to our own society, in matters of thought and behaviour. Adopting the approach of the geographer Vidal de La Blache ('possibilism') Febvre (cf. his 1920 *La Terre et 1'Evolution Humaine*), dismissed German geographical determinism (e.g. Ratzel), by asserting (1) 'There

(1) MENTALITES
- Berr
- Bloch
- Febvre—'Histoire totale'
- Braudel—Communication model

(2) HISTOIRE PROBLEME
- Braudel
- Hexter—Morgan
- Ladurie—*The Case of the Chouan Uprising* (1972)
  - —*Carnival* (1979)

Figure 1.3 The *Annales* paradigm: 'beyond Braudel'.

are no necessities, but everywhere possibilities'; and (2) the influence of the environment on human affairs is only indirect, mediated through social structure and ideas (Burke 1973, p. vii). Marc Bloch, although he generally left the development of cultural history to Febvre and concentrated on economic and social history, was still enthusiastic about exploring the role of *mentalités* in history, writing in 1929:

> social realities are a whole. One could not pretend to explain an institution if one did not link it to the great intellectual, emotional, mystical currents of the contemporaneous *mentalités* . . . . This interpretation of the facets of social organisation *from the inside* will be the principle of my teaching, just as it is of my own work. (cited in Burguière 1982, p. 430)

Interestingly Bloch and Febvre did have different views about the way *mentalités* developed (Burguière 1982). Bloch's view has tended to dominate in *Annales'* scholarship since the 1970s, as 'anthropological history'. He looked to the social and economic context of a *mentalité* as appropriate explanatory conditions for its origin and role in history, exploring the effect on human concepts of the logic of everyday life (e.g. through the realities of health and hygiene, diet, and the class basis for certain ideologies). Such contextual belief systems can be found expressed in collective representations (mythologies, symbols), but their history is seen as essentially one of subconscious growth and maintenance, as it were of the 'neural equilibrium' of individuals adjusted to their socio-economic and physical environment. The recent popularity of Bloch's version of *mentalités* owes much to the continuing growth in quantitative history and major research projects on the history of human diet, disease and social structure.

Febvre, on the other hand, although he shared with Bloch the belief that to understand an epoch, i.e. a specific organization of society, from the inside, one should approach it through its mental framework, favoured much more scope for a constant interaction between individual human consciousness and group consciousness as reflected in *mentalités*:

> Let the historian install himself at the intersection where all influences crisscross and melt into another: in the consciousness of men living in society. There he will grasp the actions, the reactions, and will measure the effects of the material or moral forces that exert themselves over each generation. (Febvre 1928, cited in Burguière 1982, pp. 430–1)

Here the focus has visibly shifted: first, to conscious formulations and away from Bloch's search for the *implicit* meaning of collective behaviour; and second, to a more subtle model of group *mentalités* as both influencing and influenced by the conscious thought of certain individuals within a community. Actually Febvre was to criticize Bloch's *Feudal Society* for abolishing the individual and relying on collective sensibilities. Indeed for Febvre the acid test of his search for *mentalités*

in history was through their reflection in the thought of the individual, as shown by his studies of historical personages such as Luther. It is also fundamental to Febvre's model that influential human personalities can recast the ways of thought of their times and community, just as much as they are to a large extent moulded by the world views in which they are born and grow up.

Febvre's concept of the individual's place in society is therefore highly illuminating, being summed up by Burke as: 'ideas cannot be understood without relating them to their social milieu' (1973, p. IX). In an essay of 1938 Febvre provided the following discussion on this theme:

> the historian's real subject matter? It is generally supposed to consist of two things. First, the confused movements of masses of unknown men doomed, one might say, to do the donkey-work of history. Second, standing out against that murky background, the guiding action of a certain number of individuals known as 'historical figures'.

The reason this curious picture has arisen has to do with historians taking the easiest way out in trying to

> 'organize the past', to bring light and order into all the constantly shifting, fluttering, flashing facts which, apparently subject to no laws, collide, mingle and compel one another all around every man at every moment of his life, and so at every moment the life of the societies to which he belongs.

Febvre rejects this dichotomy and develops a counter-solution. On the one hand

> an individual can only be what his period and social environment allow him to be . . . social environment impregnates the author of any historical work in advance and sets him, broadly speaking, within a framework, predetermining him in what he creates. And when he has finished, either his work dies, or, if it is to live, it has to submit to the active, formidable co-operation of the masses and to the irresistible, compelling weight of the environment.

To follow then, the meaning of historic actions, the historian must 'reconstruct the whole physical, intellectual and moral universe of each preceding generation', doing what has become another *Annales*' catchword—total history (1938 (1973, pp. 2,4,9)).

However, on the other hand we find in Febvre a human 'possibilism' (and even 'existentialism') paralleling that enshrined in the physical environment. Human individuals are certainly free to act, and to good purpose in moulding their own and others' lives. Yet the random, chance effects of individual thought and action, individual psychology and emotion, are still constrained by the social and cultural context as well as by the physical context of the 'author of historical action'.

Whatsoever is said or done, then, by individuals or during short-lived events, is of no wider importance unless it creates or reflects a significant trend at the group level and in the medium to long-term.[6]

Braudel in *The Mediterranean* seems to envisage a similar relationship. According to Stoianovitch, Braudel

> also provides . . . [a] conception of communication, very close to that of the historical psychologist Ignace Meyerson, who defines a human act or work as a form endowed with temporal, spatial, and social limits; inserted in a series of acts and works, assigned a signification; and having the purpose of perpetuating itself and its signification. Indeed, the object of every act, work, or institution is to communicate itself through time and to maintain or extend its spatial limits. Braudel's *Mediterranée* is, among other things, a study of the work and works of the larger Mediterranean of the later 16th century understood as acts of communication. (1976. p. 66)

It seems to me that this potent model gains by ignoring the implication that all historic acts are motivated by a desire to make a mark on history. Many far-reaching developments have been after all unintended consequences of more restricted intentions. What I take this communication model to offer us is a means of relating people and events to wider and longer-term trends, by the retrospective analysis of the consequences of particular events or actions as successful or unsuccessful forms of human communication (intended or otherwise).

I find this model especially helpful for archaeologists, not least prehistorians, where it might be argued that our initial point of data perception is usually only at the level where the isolated personality, action or event has succeeded in making wider communication through the material culture record.

The communication model, deprived of any obligatory corollary of original human motivation, also enables us to circumvent the vexed issue of attaching an interpretation of why particular people did particular things in the past, a topic I have dealt with in detail elsewhere (Bintliff 1988). Suffice it to say here, that despite Ian Hodder's recent attempts to inject Collingwood's theory of mind-reading into archaeology (1986), the experience of historians on this question raises serious doubts about the validity of that approach, even for highly literate cultures.

Problem history *(L'Histoire problème)*

A second major *Annales'* approach to fulfilling the total potential of structural history is that of problem history. Once again, it is found implicitly in Braudel's *The Mediterranean* but explicitly in other *Annales'* scholars.

Hexter made exactly this point in his major review of 'Braudel's World' (1972), in discussing the failure of Braudel to make the explicit link between events and structures:

> Yet that problem is not insoluble. One solution lay within Braudel's reach, and in fact time and again he reached for it and caught it. He caught it, for example, when he asked, 'Why did banditry flourish in the Mediterranean toward the end of the sixteenth century?' 'What accounts for the considerable flood of Christian renegades into the service of the Turk and the Barbary States?' 'Why did the Spanish ultimately expel the Moriscos?' One answer to the problem of bonding event, conjuncture, and structure is provided by *histoire problème*. (1972, p. 535)

Hexter goes on to demonstrate by an example how problem history, implicit in Braudel and explicit in other historians, can create a fully operationalized Structural History even at the neglected level of the short-term event. He summarizes a study by Edmund Morgan (1971) of the seventeenth century decline of the English colonial settlement at Jamestown USA. That study has as its taking-off point, participant/observer images of impoverished men at play in the colony, but by its completion, Hexter notes triumphantly:

> What binds together the event—the bowling scarecrows of 1611; the conjunctures—the curves of population, wages, and prices in England, of sugar, tobacco and slave prices in the Atlantic world; the structures—the established image of colonial settlement, the ingrained English patterns of work and leisure, the 'military mind' of the sixteenth century? What does it? The problem precisely defined at the outset: why were the settlers of Jamestown unready for hard work? (Hexter 1972, p. 537)

The third generation *Annales'* historian Emmanuel Le Roy Ladurie has also devoted increasing attention to solving the problem of the interplay of different time-scales in the past.[7] In an article specifically entitled 'The "event" and the "long-term" in social history: the case of the Chouan uprising' (1972 (1979)), he also chooses the path of problem history to accomplish the task, which means:

> starting from a given structure, the existence of which is well attested and empirically evident, but the origins of which are shrouded in mystery, and to look for the initial traumatic event which may have acted as a catalyst for its emergence. The event itself would then have to be relocated . . . within the structures prevailing at the time of its occurrence. This is indeed the approach adopted by Paul Blois in his book . . . *Peasants of the West* (op.cit., pp. 115–16)

Blois (1971) seeks the origin of the traditional political and cultural conservatism of parts of western France: no modern factors explain the phenomenon, but a traumatic historical event—the Rebellion of 1793–9

—marks the appearance of the dominant regional tendency, culminating in a strong Gaullist stance in recent times. Blois demonstrates that the decisive event is the emergence of class consciousness amongst the peasantry, whose aggressive feelings, originally directed against feudal oppression, were very soon to be turned against the local bourgeoisie: 'In the space of a few years, the passing event has produced a lasting *mentalité*—a short-term phenomenon has produced a long-term structure' (op.cit., p. 123). There is also an underlying factor of environmental 'possibilism', based on the difference between more fertile land in the west of the region studied, and poorer land to the east, producing through the catalyst of the regional rebellion opposing tendencies towards conservative and radical political stances. As a result of their different fates during the French Revolution and immediately after, the west turned its back on the new Republic, while the east embraced its values, which were those of an urban bourgeoisie: 'The Chouan event had acted as a contingent catalyst, as a bridge between the socio-economic structures of the *ancien régime* and the politico-cultural structures of the present day' (op.cit., p. 129).

Ladurie's own books since the 1960s (and note that significantly he took over editorship of the *Annales* in 1967), have focused on merging the everyday realities of life in the Middle Ages and Early Modern era, so-called 'thick description' (wherever possible quantified) with the medium to long-term structural components such as economic and demographic cycles, climatic cycles, and cultural world views or *mentalités*. An increasing attention to connecting the event and the individual to wider and longer trends is observable as we move from his 1966 *Peasants of Languedoc*, to the tours de force of *Montaillou* (1975) and *Carnival. A People's Uprising at Romans 1579–1580* (1979).

In the introduction to *Carnival*, Ladurie tells us he had always wanted to study a small town, but found himself shifting from a longer time perspective at Romans to concentrate on a two-week period in 1580, the time of the annual carnival, which seemed to illuminate the dialectic of forces in the town's development to perfection, not least because the festival developed into a class massacre.

> The Carnival in Romans makes me think of the Grand Canyon. It shows, preserved in cross-section, the social and intellectual strata and structures which made up a *très ancien régime*. In the twilight of the Renaissance it articulates a complete geology, with all its colours and contortions. (p. 370)

The Carnival is a 'decisive but fleeting moment' (p. 367) in a time flow of social contradictions—in fact part of a longer-term perspective of several hundred years of popular revolts, which later climaxed in the French Revolution. The 1580 Carnival also occurred at the junction period between two strong *mentalités*, the preceding Protestant movement and the soon-dominant Counter-Reformation. The Carnival brought to a

head a growing stress between the major classes in town and country, growing because of radical changes in the underlying regional economy associated with agrarian capitalism: hence the conflict pits on the one side the urban nobility and the upper middle class, against the lower middle class and the peasantry; the question of popular power is at issue. The Carnival not only marks a major resolution of these structural contradictions, in favour of the upper classes, but the Carnival symbolism directly reflects these economic, political and ideological conflicts and its divisive satire led directly into genuine class war. The success or failure of popular revolt in 1580 also twists, finally, on the political and strategic skill of the principal protagonists heading rival factions in the town; the immediate result cannot be predicted, and depends on personality and chance. In one short period, then, Ladurie reconstitutes the history of Romans from the interplay of events, personalities, *mentalités*, and cyclical conjunctures.

Here I want to make a very important point, one developed in much more detail in my introductory paper to *Extracting Meaning from the Past* (1988). Recent research in evolutionary zoology and biology, cosmology and theoretical physics, denies any predictability of direction that a dynamic natural system may take, yet affirms a fundamental tendency for endlessly recreated structure and stability. If change is initiated by apparently random or unique events, lasting effects can only be achieved if random input is integrated into internally stable and externally adaptive structures with very non-random and repetitive forms. Thus we can postdict, but not predict, what happens in the past. This wide-ranging theory is not a model developed by the *Annales* school but in other disciplines, yet it is entirely complementary and reinforcing both to the *Annales'* concepts of historical causality and to the *Annales'* model for the interaction of individual/event phenomena and community/longer-term trend phenomena.

This brings me conveniently to a final methodological problem, one which was raised briefly earlier in this chapter (cf. Note 3). Historians who have been investigating in depth the operationalizing of the overlapping temporalities of Structural History point out that the ideal scheme of three major oscillating trends is a useful model, but in reality there may be many more 'waves' to allow for, perhaps in combinations quite specific to individual eras and regions (cf. Le Goff 1989, p. 405: 'In fact what the historian grasps is a multiplicity of possible time-spans, most of them social or collective'). There seems to be a danger of dissolution of any methodological framework if the structures of Structural History become potentially numberless and lacking in recurrent periodicity. None the less I feel it can be argued with little difficulty (cf. Bintliff 1989 for examples) that phenomena of the short and long-term, on the time-scales postulated by Braudel, do indeed have a forceful role in the pattern of history: the world of experienced *événements* because we as human actors react undeniably to scenarios encapsulated within our

lived experience (cf. Le Goff 1989, p. 405: 'the span of an individual human life is itself a significant *durée*'). As for the *longue durée*, prehistorians and historians continue to find compelling reasons to focus on the distinctive characteristics of long-lived eras such as the time of the hunter-gatherers, the first settled farmers, chiefdom and proto-state societies, early urban and imperial societies, feudal and industrial societies, etc. It is perhaps in the medium term that our recognition of dominant trends remains less predictable and open to empirical revelation; in the final section of this chapter I shall however try to demonstrate that medium-term periodicities can be recognized of similar time-scale even in cross-cultural perspective.

None the less, if we were to accept a more flexible operational sphere for analysing the central trends of Structural History, I find a musical analogy the most helpful way of illustrating how we might proceed to dissect, analyse, then reanimate the vital structures that created the historical past in its major features. Let us imagine the numerous active forces at work in a given dynamic historic situation: climatic fluctuations, harvest yields and prices, peasant and proletarian unrest, technological factors and changes, population structure, religious and other ideological trends, the creative or disruptive input of influential individuals of whatever class, pressures and opportunities from neighbouring societies, the constraints and opportunities of local geography or local *mentalités*, etc. Let us further imagine that these and other factors, evidence for which hopefully is available, are instruments in a great unruly orchestra. Now the conductor arrives—he (or she) is the historian. His score is the narrative history, in 'thick description' of that society as it is known to have developed over a particular span of time. Faced with the vast and disparate assembly of his giant symphony orchestra, it is none the less his task to animate each instrument, but at the right moments, at the right level of volume, so that in concert, the combined forces of these diverse *timbres* and tempos create as closely as possible the real complexity and yet at the same time the real interplay of harmony and conflict (order and disorder) as laid down in the narrative score. As we all know, some conductors fail to harmonize their musical forces and the score does not achieve convincing realization.

For the reader of a major work of history, likewise, the historian must persuade us that his skilful interweaving of historic forces convincingly accounts for the main features of the historic narrative under analysis. It is also true both of a musical performance and a work of history that new insights and even alternative interpretations are not only always possible but are the essence of creativity. Some of us may be satisfied by the tempo and orchestral balance offered by a Marxist historian, others by the performance of a 'new Right' historian, and so on.

In a recent and brilliant essay on the revival in history-writing of individual biographies, Jacques Le Goff argues cogently that the time is

ripe to build in this fashion on the remarkable progress made in contextualizing a particular human lifespan and life achievement as a result of *Annaliste* Structural History and the history of *mentalités*. Interestingly, he also uses a musical metaphor:

> it [now] becomes possible to approach a specific and unique person, and to write a true biography through which a historically explained individual can emerge out of a given society and period, intimately linked to them yet also impressing on them his own personality and actions. From the chorus of human voices, a particular note and style can be made to stand out. (Le Goff 1989, p. 405)

In summary, then, I hope I have been able to demonstrate the immense potential for archaeological interpretation of the *Annales'* paradigm and Structural History in expanding and deepening those advances in understanding past societies achieved by the New Archaeology movement (cf. also Bintliff 1989).

In particular, the key items on the agenda of so-called post-positivism regarding the place of individuals and events in the past seem satisfactorily dealt with. As for the remaining item, the relativity of knowledge and the subjectivity of scholarship, this has readily been acknowledged by the *Annales'* school, indeed it is seen by them as that which keeps history alive. Febvre long ago commented that each age has necessarily the history that it needs: 'Organizing the past in accordance with the needs of the present, that is what one could call the social function of history' (1949 (1973, p. 41)).[8]

I have not had the opportunity to show all the ways in which these insights and models will be operationalized in archaeology; however, that is precisely the role that the other contributors to this volume have been set. I have already suggested, however, that whereas the full gamut of *Annales'* approaches may be employed in historical archaeology (and note that the unreal division between history and archaeology for literate societies was voiced long ago by Febvre (1938 (1973, p. 10)), we may find ourselves in a more limited situation in prehistoric archaeology. Although we do indeed find in the record of individual stratigraphic levels and individual dwellings or on individual pots, the durable mark of the short-term and the human personality, it is totally unrealistic to hope that this tiny palimpsest of all human action should coincide with those primary 'acts of communication' in Braudel's model that created vital influences on whole communities of people and whole eras. Yet, employing the communication model we can be very much more optimistic, and suggest that it is much more reasonable to hope we can detect these unique and particular, quasi-random inputs at the level where they have successfully interacted with those medium to long-term structures which we can document with so much greater ease in prehistory. Using problem history we can look especially at structural

change as the locus for tracing the bow-waves of events. Material culture is also very far from being mute about world views and ideologies, as Ian Hodder has been teaching us (1982a,b), although this was also stated long ago by Febvre in criticizing the division between history and archaeology. In 1949 (1973, p. 35) he wrote: 'the concept of pre-history is one of the most ridiculous that can be imagined'. Thus structural change is to be analysed not only in terms of demography and environment, social and political transformation, but in the hearts and minds of past societies as symbolized in the changing components of material culture.

The potential of the *Annales'* paradigm: a case study

I will end this introductory chapter by explaining how I can see my own way forward to new insights in a field project Anthony Snodgrass and I have been directing in Central Greece for the last ten years, the Boeotia Project (cf. Bintliff and Snodgrass 1985, 1988). Let me start with problem history and events. Several ancient inscriptions from particular years in the late third century BC and the early second century BC indicate some sort of crisis in the ancient province of Boeotia. A wealthy citizen of the city of Thespiae lent a large sum to the city of Orchomenus but could not obtain repayment. The tiny city of Chorsiae honours a man of Thisbe who ignored a federal law that prohibited the transport of food between cities—he brought corn to Chorsiae when all the region had famine. Yet another inscription from Thespiae honours an Athenian who is carrying out emergency training of under-age youths for the defence of Boeotian cities. Greek and Roman historians reinforce this picture in their narrative of these decades. Thus after 196 BC the pro-Roman upper classes in Boeotia complained of 'the people's present hostility towards them and the general lack of gratitude shown by the masses', linked to a massacre of Roman soldiers.

A strong clue that these events are interacting with medium-term trends comes from the history of Polybius, who lived through the latter part of this period at first hand. His account is also significant in shedding light on contemporary attitudes to these disturbing problems and the perceived causes:

> In Boeotia, after the peace between the Romans and Antiochus had been signed, the hopes of all those who had revolutionary aims were cut short, and there was a radical change of character in the various states. The course of justice had been at a standstill there for nearly twenty-five years, and now it was common matter of talk in the different cities that a final end must be put to all the disputes between the citizens. The matter, however, continued to be hotly disputed, as the indigent were much more numerous than those in affluent circumstances. (XXII,4)
>
> Certain strategi (generals) even provided out of the public funds for the indigent, the populace thus learning to court and invest with power those

men who would help them escape the legal consequences of their crimes and debts. (XX,6) [Polybius would clearly have felt at home in Thatcher's Britain!]

Incident upon all this was another unfortunate mania. For childless men, when they died, did not leave their property to their nearest heirs, as had formerly been the custom there, but disposed of it for purposes of junketing and banqueting. (XX,6)

Polybius explicitly links these short-lived events and contemporary attitudes to trends traceable over the preceding 200 years of Boeotian history, though he is unclear about the exact reasons for this:

For many years Boeotia had been in a morbid condition very different from the former sound health and renown of that state. After the battle of Leuktra



Figure 1.4 Settlement map for the period 600–200 BC in the area covered by the Boeotia Survey. Thespiae and Haliartos are independent cities. Askra is a small town belonging to Thespiae. The remaining sites are almost all small to medium-size farms.

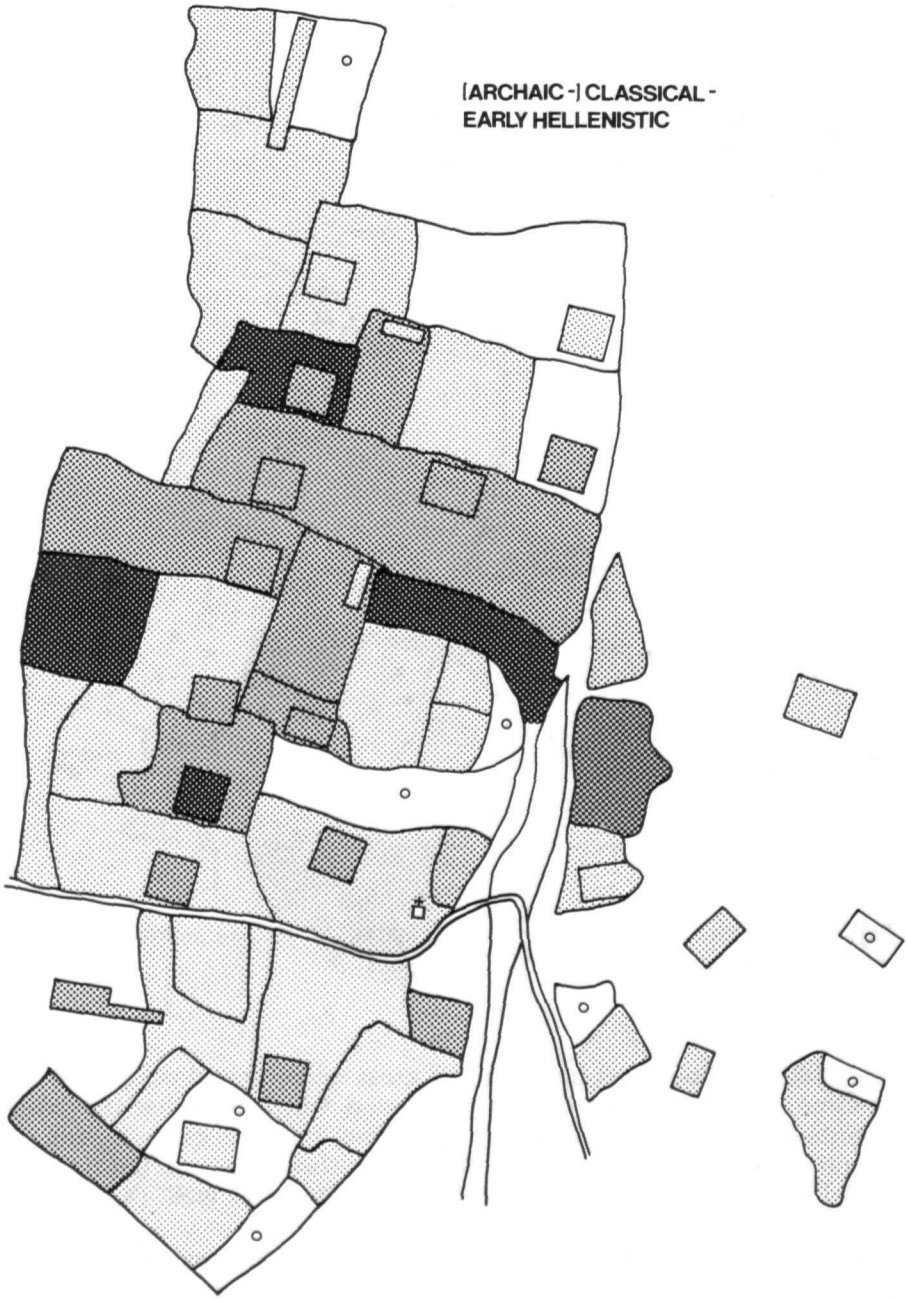


Figure 1.5 Extent of the small town of Askra during the period 600–200 BC, as indicated by the surface distribution of pottery of that period within the modern field system over the ancient site. The degree of shading reflects the relative density of surface pottery, whilst a zero sign over a blank field shows that no definite finds of this period were found during archaeological surface survey.

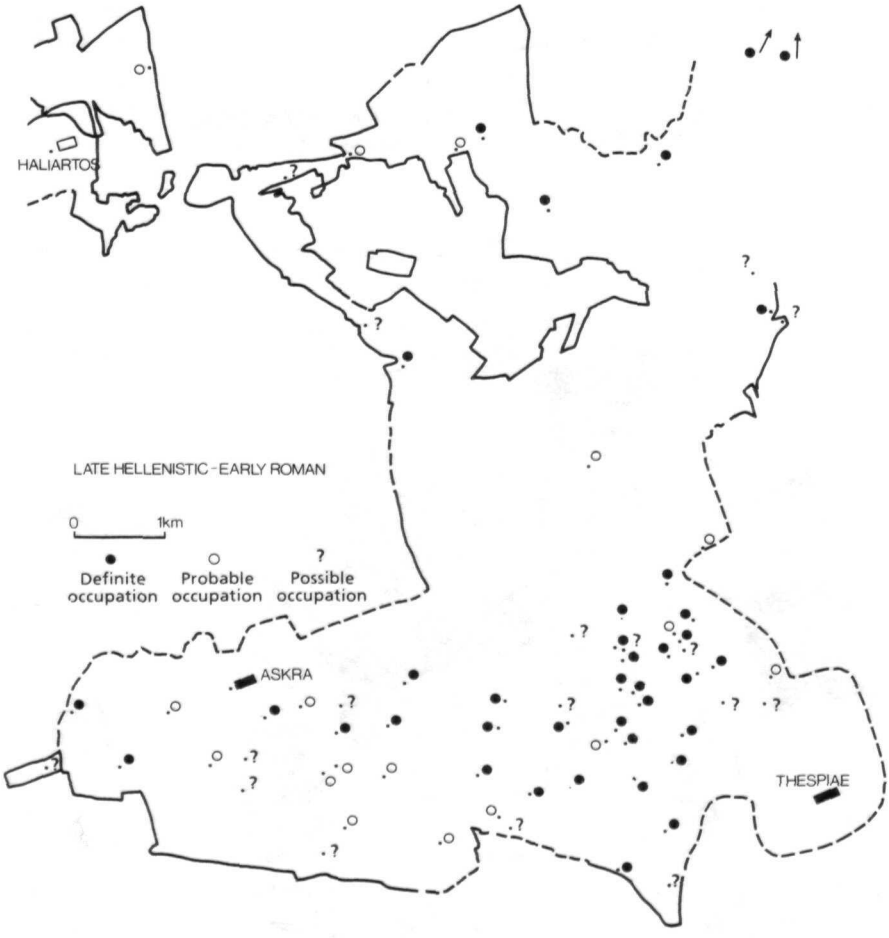


Figure 1.6 Settlement map for the period 200 BC–AD 300 in the area covered by the Boeotia Survey.

> (371 BC) the Boeotians had attained great celebrity and power, but by some means or other during the period which followed they continued constantly to lose both the one and the other . . . and in subsequent years not only did this diminishment go on, but . . . they did all they could to obscure their ancient fame as well. (XX,4)

In case we should think this deterioration over the *moyenne durée* is a purely Boeotian phenomenon, Polybius writes elsewhere:

> In our times the whole of Greece has suffered a shortage of children and hence a general decrease of the population, and in consequence some cities have become deserted and agricultural production has declined, although neither wars nor epidemics were taking place continuously. (XXXVI,17)

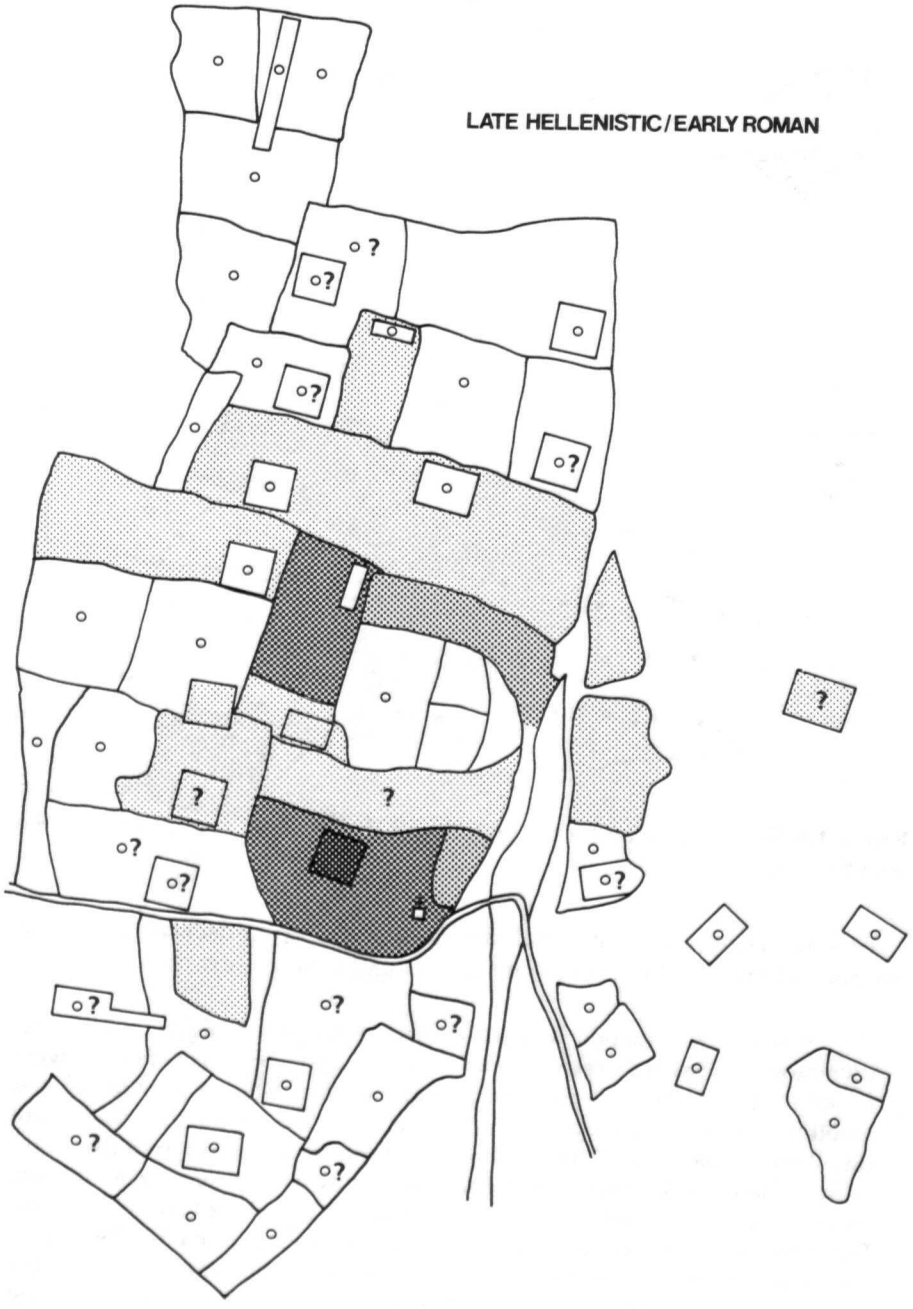


Figure 1.7 Extent of the small town of Askra during the period 200 BC–AD 300.



Figure 1.8 Settlement map for the period AD 300–700 in the area covered by the Boeotia Survey.

This is written in a general discussion of the causes of historical events, where Polybius sees Fate and Chance as irrelevant:

> the cause of this situation was self-evident and the remedy lay within our own powers. This evil grew upon us rapidly and overtook us before we were aware of it, the simple reason being that men had fallen a prey to inflated ambitions, love of money and indolence, with the result that they were unwilling to marry, or if they did marry, to bring up the children that were born to them: or else they would only rear one or two out of a large number, so as to leave these well off and be able in turn to squander their inheritance. For in cases where there are only one or two children and one is killed off by war and the other by sickness, it is obvious that the family home is left unoccupied, and ultimately, just as happens with swarms of bees, little by little whole cities lose their resources and cease to flourish.

Polybius believed these elements of crisis had a direct cause in the moral degeneration of the Greeks since the fourth century BC, but even

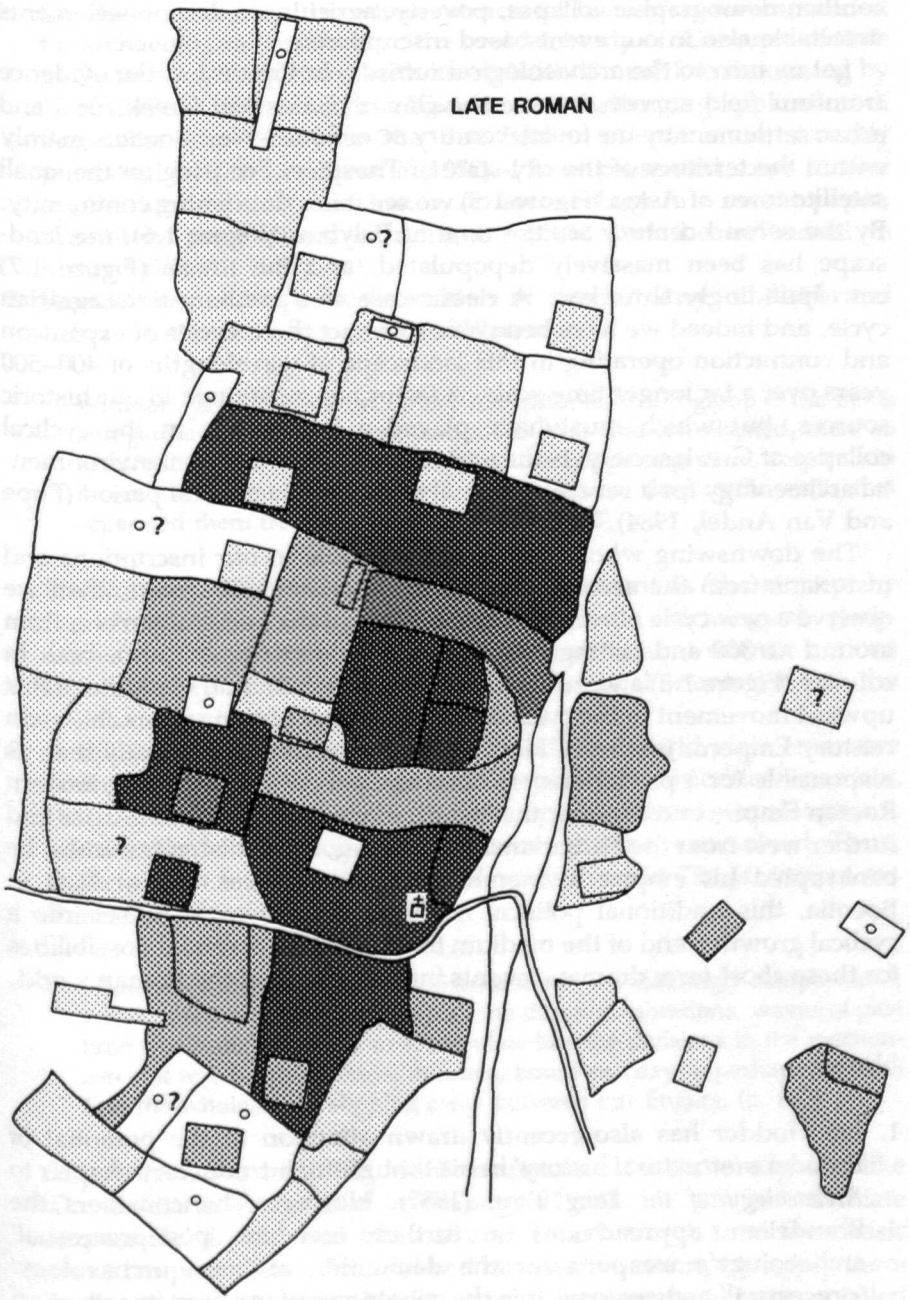


Figure 1.9 Extent of the small town of Askra during the period AD 300–700.

in his account there seem to be underlying factors involving class conflict, demographic collapse, poverty, agricultural decline—elements detectable also in our event-based inscriptions.

Let us turn to the archaeological records. In Figure 1.4, the evidence from our field survey, we see the climax of classical Greek rural and urban settlement by the fourth century BC of South-West Boeotia, mainly within the territory of the city-state of Thespiae. Focusing on the small satellite town of Askra (Figure 1.5) we see it is a flourishing community. By the second century BC, the time of Polybius (Figure 1.6), the landscape has been massively depopulated, and the towns (Figure 1.7) correspondingly shrunken. A classic case of a medium-term agrarian cycle, and indeed we have been able to detect these cycles of expansion and contraction operating in this landscape at wavelengths of 400–500 years over a far longer time-scale. A factor not mentioned in our historic sources, but which must have played a patent role in the cyclical collapse of Greek society, is the widespread evidence from environmental archaeology for a severe erosional phase at this general period (Pope and Van Andel, 1984).

The downswing whose effects are traceable in our inscriptions and historians from the second century BC, lasts some 500 years, then we observe a new cycle of recovery and growth in the Late Roman era, from around AD 300 and lasting into the early seventh century AD, both in country (Figure 1.8) and in townscapes (Figure 1.9). In the middle of this upward movement in the cycle comes the career of that remarkable sixth century Emperor Justinian. His forceful personality is generally seen as responsible for a prodigious effort on the part of the surviving Eastern Roman Empire to reconquer the lost provinces in Italy, North Africa and further west from the Barbarians. In so doing, some historians write, he bankrupted his empire in manpower and resources. Yet at least in Boeotia, this traditional political history approach must be set into a cyclical growth trend of the medium term, creating essential possibilities for these short-term dramatic events in the twilight of the Roman world.

Notes

1. Ian Hodder has also recently drawn attention to the potential of Braudel's 'structural history' in his thoughtful introductory chapter to *Archaeology of the Long Term* (1987). However, he considers the Braudelian approach as a further tool in 'post-processual' archaeology's weapons for the demolition of 'new' archaeology/ 'processual' archaeology. It is the contention of my own introduction to this present volume that the broader *Annales'* methodology to history reveals the fruitful complementarity of 'processual' and 'post-processual' approaches in the interpretation of the archaeological past. Amongst other, rare, references to the *Annales'* approach in the

archaeological literature I might single out the paper of 1982 by Gledhill and Rowlands, where 'structural history' is recommended for archaeological application; the probing study of 'Concepts of time in Quaternary Prehistory' with its explicitly Braudelian theorizing by Geoff Bailey (1983); and the elaborate and challenging application of Braudelian time-scales to the Bell Beaker phenomenon, recently published by Jim Lewthwaite (1987). I have presented some further applications of structural history to archaeology in a recent paper, 'Cemetery populations, carrying capacities and the individual in history' (Bintliff 1989).

2. Braudel has retrospectively written of the initiative of Bloch and Febvre in the pre-war period as follows:

> Without even standing on tiptoe, the historian could glimpse the fields and gardens of neighbouring disciplines. Was it so complicated, then, so extraordinary, to set out to see what was happening there, to plead in favour of a community of the human sciences, despite the walls that separated them from one another. (1976, p. 12)

Stoianovitch summarizes the *Annales*' movement as 'the attempt by French scholars to adapt economic, linguistic, sociological, geographic, anthropological, psychological, and natural-science notions to the study of history and to infuse an historical orientation into the social and human sciences' (1976, p. 19).

3. A third important criticism of the Braudelian world view emanates from Braudel himself, but has been amplified by his critics—namely, that the strict division of the three temporal process wavelengths is arbitrary and their resolution into 'common time' unresolved. Thus, at the end of his long treatment of *conjonctures* in *The Mediterranean*, Braudel confesses to finding that

> History becomes many stranded once more, bewilderingly complex and, who knows, in seeking to grasp all the different vibrations, waves of past time which ought ideally to accumulate like the divisions in the mechanism of a watch, the seconds, minutes, hours and days—perhaps we shall find the whole fabric slipping away between our fingers. (p. 893)

If however, we treat *The Mediterranean* as a revolutionary text like David Clarke's *Analytical Archaeology* (1968), i.e. an 'intermediate impossible' anticipation of a maturer 'total history', then the classic three temporalities of Braudel and his massive attempt to apply them to the sixteenth-century Mediterranean may be treated not as failed final analysis, but rather as a simplified initial working model of the kind of structure and the kind of interpretations we should be elaborating. This is precisely the approach taken in Jim Lewthwaite's recent 'Braudelian' analysis of the Bell Beaker phenomenon (Lewth-

waite 1987). This problem is dealt with in more detail later in this chapter.

4. On Philip of Spain, whose presence hovers over the whole of Braudel's *The Mediterranean* but whose life is treated in detail only in its final part, Hexter has summarized Braudel's viewpoint as follows:

> Philip was not the master of structural time, the *longue durée*; the creations of that time were silent constraints on all he did. Nor does the time of *moyenne durée*, of *conjuncture*, accommodate itself to the ephemeral span of his reign. The discernible rhythms of economies, societies, and civilisations lie to both sides of him, before he began, after he ended. (Hexter 1972, pp. 520–1)

5. J.R. Hall's criticism of the *Annales'* School, is that they 'parted company with sociologists like Max Weber, who emphasized the importance of subjectively meaningful action. This they did by the simple task of looking at processes which, they argued, lay beyond texts and beyond individuals' intentions' (1980, p. 114). He ignores the crucial role of *mentalités* for *Annales'* historians other than Braudel. In fact with one minor exception, Hall's paper quotes exclusively from Braudel's *œuvre* in his critique. André Burguière, in his article 'The fate of the history of *mentalités* in the *Annales*' (1982), states on the contrary that 'the notion of the history of *mentalités* . . . is no doubt the single element in the *Annales* program that contributed the most to making the program popular and . . . to giving it the image of a brand name of quality' (p. 426). Stoianovitch, in his analysis of the *Annales'* 'paradigm', likewise has commented:

> The entire *Annales* movement reflects this contemporary desire not only to see the history of the past as it may have been seen by those who experienced it, as a succession of localized events, but to see it as such and yet give it a new meaning in terms of a plurality of simultaneously reinforcing and contradictory meanings—in terms of the knowledge and insights of the interpreters. (1976, pp. 65–6)

6. Already in 1900, in his introductory programme to the *Revue de Synthèse Historique*, Henri Berr wrote:

> However legitimate and however important sociology may be, does it exhaust all History? We think not . . . . Sociology is the study of what is social in history; but is everything in History social? [The historian should study] the individual particularities . . . by which even the most general transformations in society are explained.

According to Burguière (1982, p. 429) this meant for Berr not the arbitrary action of individuals but the intellectual characteristics of an era mediated through and created by the work and thought of certain

individuals. Of Febvre, Burguière similarly points out:

> How does one combine . . . different sections of the psychic and mental universe, which each has its own history, its own rhythm of evolution, if not by analysing the destiny of an individual? That is why Lucien Febvre . . . returned always to the necessity for the historian to consider the relationship between the individual and his milieu, his era. (1982, p. 434)

> Now the history of *mentalités* such as Lucien Febvre conceived it, with his need to locate disjunctions in the equilibrium between mental representations and feelings in the psychology of the subject, corresponded broadly to the question of existentialism. (op.cit., p. 437)

Febvre's elaboration of his 'event–individual/trend–society' integrated model is critical: 'We speak of great chains of events or of great proceedings. Why "great"?' In answering this question Febvre shows that, obviously, great figures in history are 'fathers' to works of importance, but when we analyse those works of man that transcend a tiny group and unite or steer larger communities, it is the wider effect, the acceptance by others or influence on others, that creates the significance (Febvre 1938 (1973, pp. 2–3)).

Jacques Le Goff, of the youngest generation associated with the *Annales'* school of history, has most recently addressed the all-important question of the role of the individual in a discipline of history indelibly enriched by the insights of Structural History and the study of *mentalités* (Le Goff 1989). In particular, he focuses on the contemporary revival in popularity of biographical studies, entitling his essay provocatively, 'After *Annales*: the life as history':

> Revivals are in fashion among historians today, particularly in France. We have seen the revival of narrative, the revival of the historical 'event', the revival of political history; and above all, the revival of biography. [And why?] The historian of structures had become sated with abstractions and starved of concrete reality. He wanted to become a real historian, as described by Marc Bloch, one who, 'like the ogre in the fairy tale', knew that 'when he smelled human flesh, he was approaching his quarry'. That quarry moreover was no longer 'man in society', or humanity viewed collectively, it was the individual, a particular historical character . . .. This though, is surely the opposite of the kind of history which has tended to dominate historiography for the past half-century, under the influence of Marxism, of the social sciences and in particular of what is known as the '*Annales* school'.
>
> The thrust of the *Annales* school was to study the development of economies, societies and civilizations in their underlying movements, to concentrate on analysis of structures and trends rather than on the narrative of events, and on the study of social groups rather than of individuals—still less great men. It was a history open to the other social sciences—human geography, economics, sociology, ethnology—that are

> concerned with collectivities; it was a history radically departing from the traditional positivist history of the late nineteenth and early twentieth century, when the dominance of political, diplomatic and military history did indeed draw attention to the men more likely than most to be the subject of a biography: kings, ministers, diplomats, generals.
>
> Finally, when Fernand Braudel suggested to historians that they pay most attention to the deepest level of history, that of structures which change only very slowly over time, he was driving us even further from consideration of the brief span of a human lifetime, and condemning biography to bow off the historiographical stage.

Le Goff, however, goes on to argue cogently that many *Annales*' historians, even from the beginnings of the movement, sought to probe the interaction of the historical individual, even the 'great man' with the structures that were their more important concern:

> While it is true that the revival of biography is part of a certain reaction against *Annales*-type history, it would be wrong to see the founding fathers of the first two generations of *Annales* as the sworn enemies of biography or of 'great men', as they were of political history and old-style historical narrative. Lucien Febvre wrote a biography of Luther, and chose to illustrate the religious universe of the sixteenth century through the individual consciousness of Rabelais. As his great disciple Braudel was to do forty years later, Febvre took as a point of reference for his doctoral thesis a 'great historical figure', Philip II of Spain. In Braudel's famous work, of course, the hero is the Mediterranean, and not Philip II, but this is precisely where Braudel parted company with Febvre, since the marginalization of the great man and of biography is more characteristic of the Braudelian phase of *Annales* than of the earlier period. Marc Bloch too, we must recognize, was moving away from biography and from individual psychology. He was the great pioneer of the history of *mentalités*.

In fact, Le Goff continues, the contemporary revival of biography is a logical development for historians trained in annaliste structural history:

> But should not the collective not in turn send us back to the individual? For a historian, is the individual not inevitably the member of a group and biography, the study of individuals, the indispensable complement to the analysis of social structures and collective behaviour? Now that history has been so profoundly renewed, can the historian not return, better equipped both scientifically and mentally, to those inevitable subjects of history—to the 'event', to politics and to the individual, including the 'great man'—subjects mishandled in the past by the reductive, positivist history which the *Annales* school, to its great credit, vigorously opposed? (Le Goff, 1989, pp. 394, 405)

7. Another younger generation *Annales* scholar of note is Jacques Le Goff, who likewise has criticized the limitations of the Braudelian

world view (cf. also Note 6 *supra*). In particular (cf. Stoianovitch 1976, pp. 93–5) Le Goff argues that Braudel should have applied his framework of three time categories to political history, and to do this we have to consider the message systems by which a society comprehends power both ideally and in practice—political semiology.

8. The relationship between *Annales'* history and Marxist history has been much discussed. I would follow the viewpoint that structural history, total history and problem history, as a package, offers a fundamental primary methodology for analysing 'what is going on' in the past in a very systematic yet non-tendentious way. If one wishes to interpret further the structures revealed by the *Annales'* approaches, using a Marxist framework (or any other a priori interpretative model), one can proceed to this in a second stage of the analysis. So, for example, Eric Hobsbawm provides the following Marxist gloss on the concept of *mentalités*:

> what I think we ought to do is see *mentalité* not as a problem of historical empathy or archaeology or if you like as social psychology, but as the cohesion of systems of thought and behaviour which fit in with the way in which people live in a particular society, in their particular class and in their particular situation of class struggle. (Quoted in Gismondi 1985, p. 211)

For a related viewpoint in Social History see J.A. Henretta (1979), 'Social history as lived and written'.

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2 Two Italys, one valley: an *Annaliste* perspective

Graeme Barker

Introduction

In 1935 the painter, doctor, and writer, Carlo Levi, was banished by Mussolini from Rome to the village of Gagliano in Basilicata, in the instep of the Italian peninsula: 'I felt as if I had fallen from the sky, like a stone into a pond' (Levi 1947, p. 27). Ten years later in Rome, he looked back in *Christ Stopped at Eboli* to

> that other world, hedged in by custom and sorrow, cut off from History and the State, eternally patient, to that land without comfort or solace, where the peasant lives out his motionless civilisation on barren ground in remote poverty, and in the presence of death. . . . No one has come to this land except as an enemy, a conqueror, or a visitor devoid of understanding. The seasons pass today over the toil of the peasants, just as they did three thousand years before Christ; no message, human or divine, has reached this stubborn poverty. . . . Of the two Italys that share the land between them, the peasant Italy is by far the older; so old that no one knows whence it came, and it may have been here forever. . . . There should be a history of this Italy, a history outside the framework of time, confining itself to that which is changeless and eternal, in other words, a mythology. This Italy has gone its way in darkness and silence, like the earth, in a series of recurrent seasons and recurrent misadventures. Every outside influence has broken over it like a wave, without leaving a trace. (Levi 1947, pp. 12–13)

Levi's description of pre-war peasant life in one south Italian village was published two years before Fernand Braudel's (1949) sweeping history of the entire Mediterranean world in the sixteenth century, the extraordinary book that became the cornerstone of the *Annales* movement. Their wartime experiences had been rather similar, Braudel as a

prisoner of war and Levi as an exile, and though the two men—and the two books—could not have been more different, the perspectives to which they arrived on the lives of ordinary Mediterranean people have much in common. The lot of the south Italian *contadino* (peasant) as witnessed by Levi in Gagliano finds many echoes in the *Annaliste* paradigm of history: the interplay between different forces operating at different time-scales in particular landscapes.

In *Annaliste* terms, Levi deliberately minimises the role of *événements*, the short-term occurrences of traditional narrative and political history such as the succession of invasions of southern Italy by Greeks, Romans, Arabs, Normans and the rest which 'has broken over it like a wave without leaving a trace'. His narrative emphasizes instead the significance of generational changes, Braudel's *conjonctures*, the 'recurrent seasons and recurrent misadventures' not only in family life but

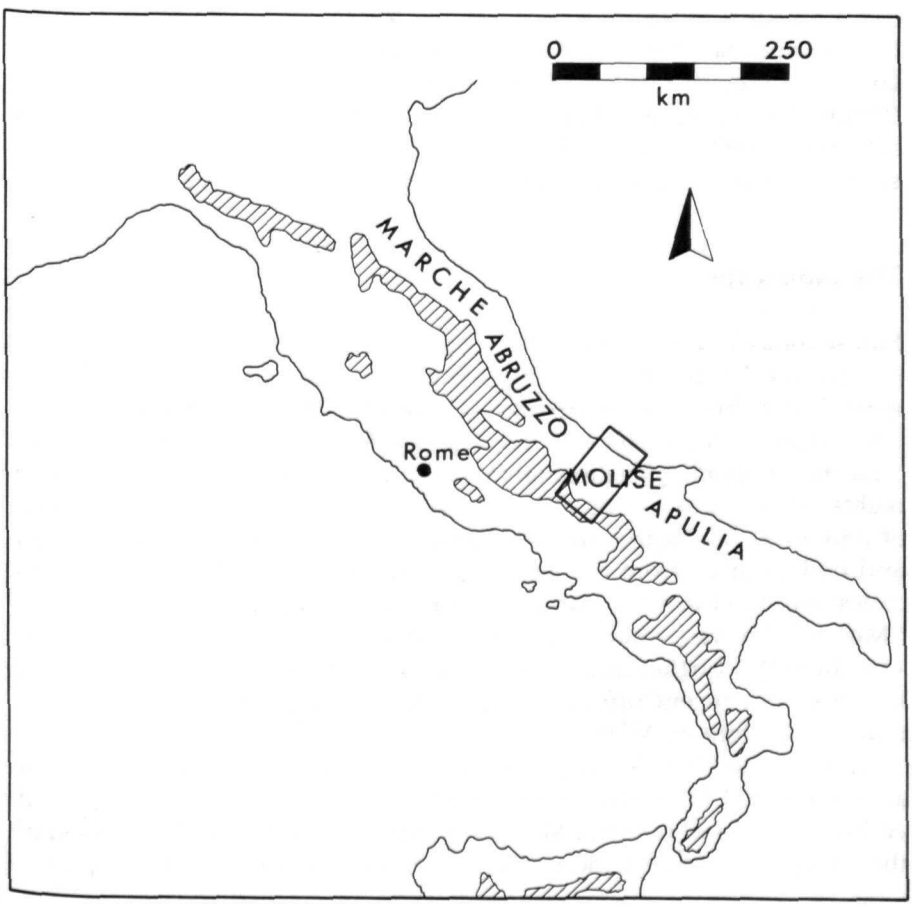


Figure 2.1 Italy, showing the location of the *regione* of Molise and the approximate area of the Biferno Valley survey.

also in terms of trends in social and economic history on the regional scale. More powerful still to Levi were the forces militating against change inherent in the landscape and in the peasants' attitude to it, respectively the *longue durée* and *mentalité* of the Braudelian paradigm: factors such as the extreme climate, the poor and unstable soils, the inadequate water supply, the limitations of peasant technology, and, in the face of the vicious circle resulting from these of low inputs and poor yields, the ability of the peasant farmer 'hedged in by custom and sorrow' to yield, endure, and survive, but not to go under.

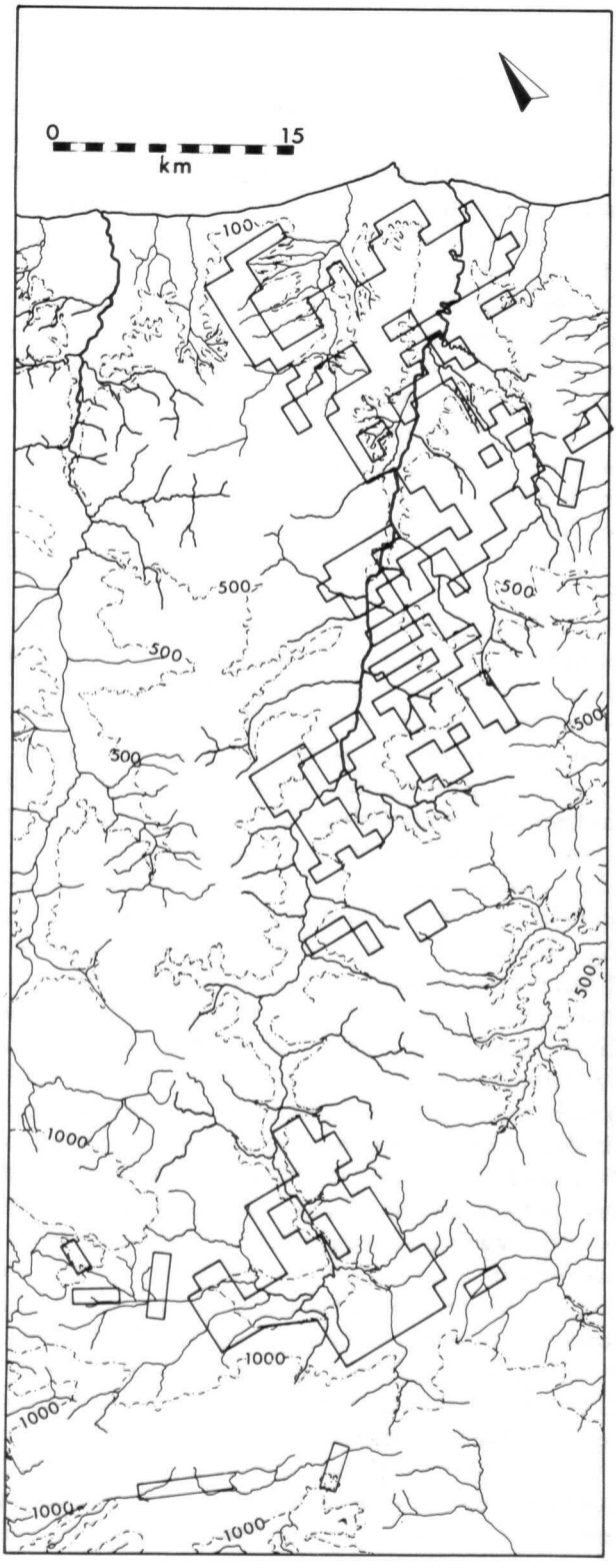
Christ Stopped at Eboli is a wonderful and moving book, a shocking account of the plight of the southern Italian *contadino* before the last war, which was extremely influential in post-war government planning to invest in the *Mezzogiorno*. But is its story of a peasant people 'motionless' in remote poverty simply the licence of a great writer, or is there a grain of truth in Levi's vision—or nightmare—of five thousand years of changeless toil 'cut off from History and the State'? And how useful is the *Annaliste* perspective in any modern assessment of the history of the Italian *contadino*? This chapter describes one such history in one particular landscape, the valley of the Biferno river in the region of Molise (Figure 2.1). It argues that, whilst the details disagree with the broad sweep of Levi's canvas, the story of this valley provides persuasive support for the relevance to archaeology of Braudel's vision of history.

The landscape

Molise today is a classic example of the contradictions and frustrations of the Italian *Mezzogiorno*: unemployment is high, real poverty widespread, the church and Christian Democratic Party pre-eminent. (Or as Levi wrote in his preface to the 1963 edition of *Christ Stopped at Eboli*, 'paternal, prevailing institutions which, despite their claim to exclusive reality, are dead and gone'.) It is also one of the most spectacular regions of Italy in its landscape, though hardly visited by tourists (both foreign and Italian) apart from the beach resorts. Sophisticated Italians tend to caricature Molise as a cultural backwater, a culinary disaster area. ('Molise? Ah yes, they say it is very beautiful . . . I must go there sometime.') A further indication of the isolation of this part of Italy is the fact that in a recent official survey Molise was the only Italian region found to be free of AIDS.

The Biferno river is the principal river in Molise, with a catchment area some 100 kilometres long by 30 kilometres wide (Figure 2.2). It embraces a typical range of Mediterranean environments. At the head of the valley is the Matese mountain range, the southernmost massif of the

Figure 2.2 The Biferno Valley, showing topography and the zones selected for the archaeological survey; contours in metres.


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main section of the Apennines, the spine of the peninsula. At the centre of the range is a typical *altiplano* or upland plateau 1,500 metres above sea-level, one of the traditional areas of summer grazing for transhumant flocks, with the surrounding ridges rising to 2,000 metres above sea-level. At the foot of the Matese is an ancient lake basin, where there is the modern town Boiano (Roman Bovianum), and where several streams gather to form the Biferno river. The river flows north-east for some 60 kilometres to the sea at Termoli. The geology of the middle section of the valley consists of soft sands and clays interspersed with limestone outcrops, an extremely unstable landscape prone to massive erosion and landslips, where communications were very poor until the construction of a new highway in the early 1970s. Thirty kilometres from the sea, the river passes through a narrow gap between limestone outcrops which was dammed some fifteen years ago. The geology of the lower section of the valley below the dam is dominated by Pliocene sands and, at the lowest elevations, alluvial terraces of Pleistocene and recent formation.

From the mid-1970s I have co-ordinated a multidisciplinary investigation of the valley combining archaeological survey and excavations, documentary research, and palaeoenvironmental studies (Barker 1985, and in preparation). The project has had as its principal focus the interplay from prehistoric times to the present day between, on the one hand, the relationship of human settlement to the natural landscape and, on the other, the relationship of human settlement in the valley to the outside world—precisely the interplay between *événements, conjonctures, mentalités,* and the *longue durée* of the Braudelian paradigm. During the 1970s some 350 square kilometres were investigated by systematic field-walking, almost a third of the total catchment, making this project the second largest systematic survey in the Mediterranean after the South Etruria survey conducted by the British School at Rome in the 1950s and 1960s (Potter 1979; Ward-Perkins et al. 1968). At the same time a series of occupation sites of each major period of settlement from neolithic to medieval was investigated by a combination of geophysical survey and excavation, the latter with a particular emphasis on the recovery of faunal and botanical remains (e.g. Barker 1975, 1976; Hodges et al. 1980). Extensive geomorphological studies have continued to the present, and a palynological analysis of a deep core from the Matese *altiplano* is in progress.

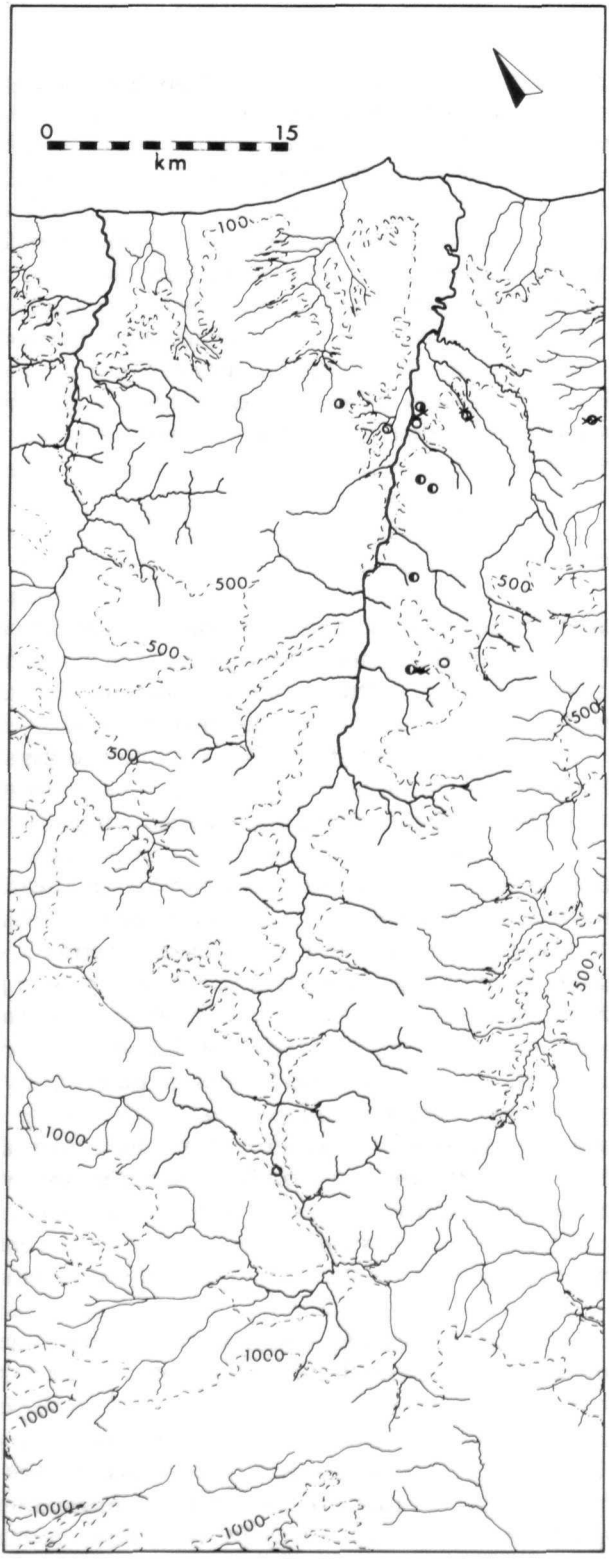
Since 1980, moreover, one of the classical sites located by the survey has been excavated on a large-scale by Dr John Lloyd (then at Sheffield, now at Oxford University) (Lloyd and Rathbone 1985), who has also co-ordinated the study of the classical period of settlement for the Biferno project (Lloyd and Barker 1981). The Superintendency of Antiquities for Molise and Perugia University have conducted important excavations at several major cemeteries and settlements in the valley dating to the Iron Age and classical period. In the same years Dr Richard Hodges (then at

Sheffield University, now Director of the British School at Rome) has greatly enhanced our understanding of the early medieval data gathered by the project in the valley (the study of which he is co-ordinating) with his large-scale excavations of the San Vincenzo abbey and its satellite settlements in Upper Molise, the mountains immediately north of the Matese (Hodges 1982; Hodges and Mitchell, 1985). Molise is also particularly fortunate in having the oldest (at least most securely dated) palaeolithic site on the European continent at Isernia La Pineta midway between the Biferno valley and San Vincenzo, some three-quarters of a million years old (Coltorti et al. 1982). From being an archaeological backwater fifteen years ago, Molise in general and the Biferno valley in particular have become one of the most intensively studied archaeological landscapes in Italy.

Prehistoric settlement

The chronological imprecision of most prehistoric data is such that we can hardly ever hope to discern short-term events in prehistory, and the search for *événements* by cultural prehistorians earlier this century tended to create a narrative prehistory peopled by Beaker Folk, Urnfielders, Marnians and all the rest, that is now almost universally recognized to have been pseudo-history rather than prehistory. We make better use of the material at our disposal in evaluating it for trends and processes. In the Braudelian paradigm for historical time-scales, medium-term processes or *conjonctures* can operate on a scale as small as a human generation, a score or so of years, but the trends which prehistorians observe in settlement forms, socio-economic complexity, demographic levels, symbolic behaviour or whatever—our phenomena closest to Braudel's use of *conjonctures*—normally encompass hundreds of years if not (in more remote prehistory) thousands of years, in chronological terms moving towards his *longue durée*. Only in the twilight zone of protohistory can we hope to recognize trends encompassing scores of years. However, whilst the prehistoric and historic timescales are so different, the trends in prehistoric settlement which can be observed in the Biferno valley are a good example of the interplay between social and economic behaviour, demographic levels, landscape and landscape change which is the cornerstone of the *Annaliste* model of the past.

To begin, not at the beginning, but with the first farmers c. 4500 BC, the dozen or so sites with neolithic pottery as well as lithic material located by the survey are all in the lower valley, and on the lightest soils (Figure 2.3). They have yielded a range and chronological sequence of pottery typical of neolithic settlement in Abruzzo to the north and Apulia to the south. Excavations at two of the Biferno sites found pit clusters typical of neolithic settlements in Italy. Such settlements vary


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greatly in size from (commonly) a few pits to (rarely) sites covering several hectares. Whilst there is little clear evidence whether the latter represent major nucleated settlements or are palimpsests of small but repeated occupations, it is noteworthy that there is no evidence for such size hierarchies in the Biferno valley. The pit complexes have traditionally been interpreted as vestiges of subsurface dwellings, but modern excavations make it clear that they were mostly quarry pits surrounding above-ground huts of timber, wattle and daub. The normal settlement unit at this time in the valley seems to have been extremely small.

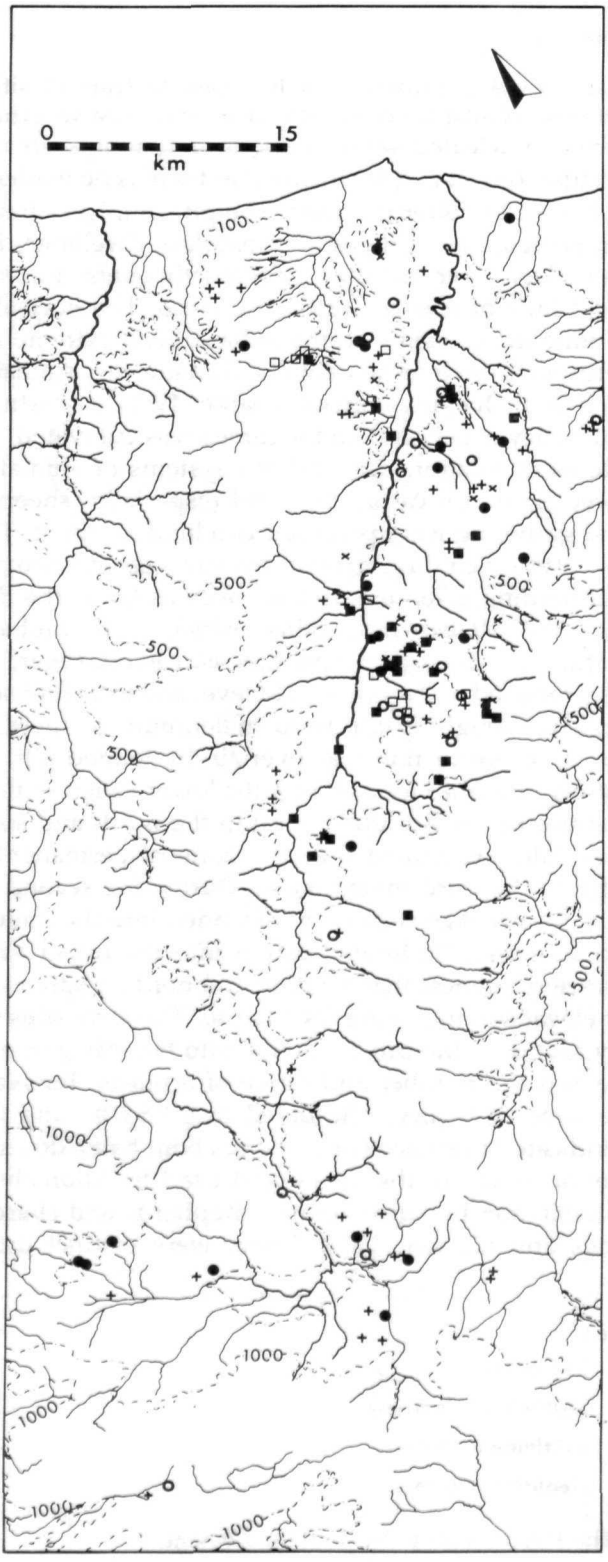
In other respects the Biferno neolithic data are very similar to those of other regions in the Italian peninsula (Barker 1981). A mixture of cereals (emmer and barley especially) and legumes was cultivated, probably in simple systems of rotation. Generalized systems of animal husbandry were followed based on cattle, pigs and (especially) sheep and goats. Hunting and gathering were practised to a limited extent. The distribution of flint scatters including arrowheads suggests that activities such as hunting and herding extended up the valley as far as the Boiano basin but not into the Matese mountains, which were probably heavily wooded at this time like other Apennine *altiplani* (Grüger, 1977). Most pottery was produced at the household level and local flint sources were invariably used, though by the third millennium BC there is evidence that this mode of production was overlain by limited gift exchange of prestige pottery and fine stone linking the lower valley settlements with the communities of the Adriatic littoral to the north and south.

The lower valley remained the only zone of permanent settlement from the fifth to the third millennia BC. During the second millennium BC, however, Bronze Age settlement extended into the middle valley as well and to an extremely limited extent into the upper valley (Figure 2.4). Flint scatters suggest that hunting and herding spread also now to the highest elevations including the Matese. The forty sites with Bronze Age pottery found by the project divide into two categories in terms of surface size and the number and range of artifacts. Excavation at two sites of the larger category, one dated to c. 1300 BC and the other to c. 1000 BC, indicated that each consisted of about half a dozen huts of the kind still found today in the valley and used traditionally in Italy for temporary habitation by farmworkers, shepherds and charcoal-burners (Close-Brooks and Gibson, 1966). There were distinct drop zones of

Key to figure

- ○ Neolithic Settlement
- ◐ Neolithic Settlement (earlier)
- ◑ Neolithic Settlement (later)
- × Sporadic Neolithic Pottery

Figure 2.3 The Biferno valley: Neolithic settlement.


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household refuse and fire sweepings on one side of the huts and working areas on the other. A settlement unit is indicated of perhaps three to five family groups, or fifteen to thirty-five people. The smaller category of site consists of single huts of the same kind, probably satellite camps occupied for shorter periods by part of the communities based on the larger sites (Barker in press).

The principal cereals grown at both sites were again emmer, wheat and barley as at the neolithic sites, but new crops represented were millet, oats and flax. A range of legumes was cultivated, the horse bean and chick pea being additions to the neolithic list. Both botanical samples were dominated by plants which are today weeds of cultivated, fallow, and waste land. Animal husbandry does not seem to have been significantly different from that of the neolithic settlements, with sheep the critical resource kept in an unspecialized management regime for wool, for milk, for cheese, and for meat. There was a basically neolithic technology of flint, antler and bone.

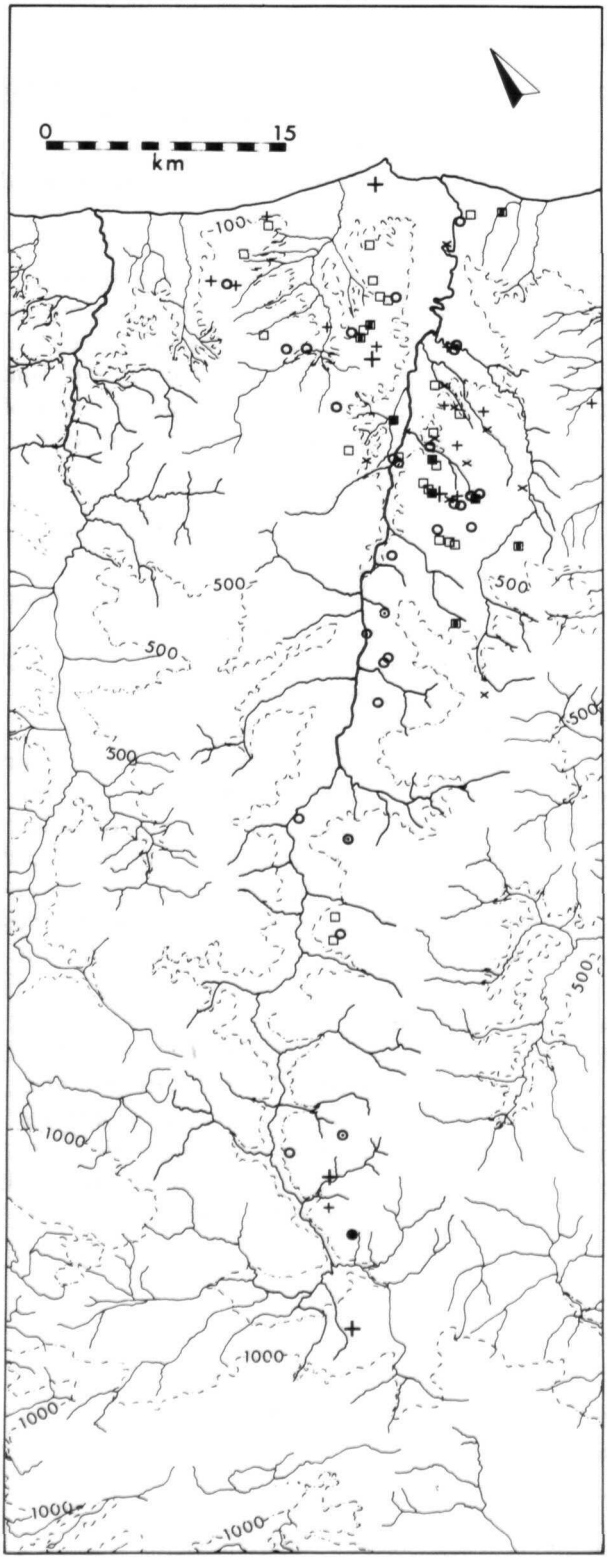
Petrographic studies show that the pottery of the valley was made locally, with the potters selecting the clays and fillers from the immediate locality of the settlements (Nash 1979), though they took their decorative motifs from a common repertoire used throughout the Italian peninsula (Lukesh and Howe 1978). There is no evidence that Molise was involved in long-distance trade with the Mediterranean and transalpine markets, and there is little evidence even for the systems of regional exchange found in other parts of the peninsula at this time. There are few signs of status differentiation in the metal industries and funerary record of the peninsular Italian Bronze Age, and none at all in Molise. The dominant ritual activities known, food offerings by springs in caves, are consistent with the other evidence for an agricultural society with little established hierarchy within individual communities or within settlement systems.

Whilst the model of large-scale transhumance for the Apennine Bronze Age put forward by Puglisi in 1959 was exaggerated, the Biferno project is typical in its evidence for the first systematic use of the Apennine mountains in the later second millennium BC. The pollen record suggests more open country at this time, both on the lowlands and in the central Apennines (Bonatti 1961, 1963, 1970; Frank 1969;

Key to figure

	Apennine	Subapennine
settlement, probable	■	●
settlement, possible	□	○
sporadic finds	×	+

Figure 2.4 The Biferno valley: Bronze Age settlement.


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Grüger 1977), perhaps a reflection of both a trend to aridity and woodland clearance, but either way it seems clear that as land became available, settlement expanded to exploit it. The general trend of expansion up the valley in the second millennium reflects in microcosm what can be observed as a general trend throughout the peninsula of a gradual 'filling out' of the landscape (Barker 1981). As the uniformity of Apennine Bronze Age pottery shows, the mountains were now a means of communication and interchange for the communities living on either side rather than a barrier.

Important changes in settlement and society can be discerned in the valley during the first half of the first millennium BC. The process of settlement expansion noted in the second millennium continued inexorably, and by the middle of the first millennium BC there were numerous settlements of similar size and appearance to the larger category of Bronze Age site in all parts of the valley. Even the most marginal localities were settled, the record even including a settlement at 1,300 metres above sea-level on the Matese. Between the eighth and sixth centuries BC the lower valley in particular came into contact with the Greek cities of Magna Graecia. A few nucleated settlements—the first real villages—were found by the survey in the lower valley, and fortified settlements were established in the upper valley (Figure 2.5). Alongside the new forms of settlement were sanctuaries, where it is likely that the rituals were related to a mixture of indigenous and Greek mythologies on the evidence of their use in the succeeding Samnite period (see below). Cemeteries provide clear signs of a stratified society, though with less marked disparities in grave goods than are found in the cemeteries of neighbouring regions (Di Niro 1981, 1984). The new élites traded with the Greeks for prestige goods, particularly luxury table services associated with wine-drinking. Grape pips as well as cereals and legumes were found at both of the Iron Age sites investigated by the project.

As the survey shows, the valley environment had a major influence on the course of prehistoric settlement. The first farmers selected the

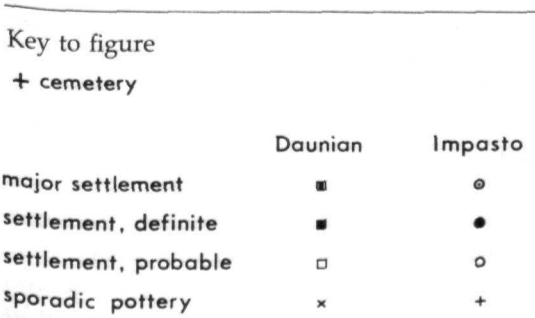


Figure 2.5 The Biferno valley: Iron Age settlement.

best locations for the agricultural system they practised (the lightest soils in the lower valley), using the middle and upper valley only for hunting and pastoralism. By the second millennium BC, farmers were living in the middle and upper sections of the valley as well as the lower section, and some were having to make do with secondary locations—poorly drained heavy soils by water-courses in the lower valley, for example, or infertile stony soils in the upper valley. However, the agricultural system practised by these settlements was fundamentally the same as that of the first farmers, in its mix of crops, legumes and stock, though the faunal samples from our excavations suggest there may have been increasing reliance through time on animal secondary products. In the first half of the first millennium BC, settlement expanded to the limits of marginality in the upper valley, strongly suggesting that population pressure was one important factor in the transformations in settlement and social organization of this period. Yet whilst the prehistoric record inevitably emphasizes the constraints imposed by the valley environment on agricultural settlement, its role in the *longue durée* of some four thousand years, it is clear that the other critical stimulus in the Iron Age transformation was external, the impact of the Greek world—its ideas as much as its products—on indigenous society.

Classical settlement

Molise in the second half of the first millennium BC was part of ancient Samnium (Salmon 1967). According to the written sources, the Biferno Samnites were part of two tribal groups, the Frentani in the lower valley and the Pentri in the hills. They were further divided into district *pagi*, and lived in enclosed villages—*oppida* or *vici*. A few aristocratic families controlled political, military, and religious offices. The rising population led to well-documented emigration from the valley in the fifth and fourth centuries BC, southwards to Apulia and Basilicata and westwards across the Apennines to Campania, where the Samnites came into conflict with the newly aggrandizing power of Rome, culminating in the three bitter Samnite Wars between 343 and 290 BC (which the Samnites lost). The following two centuries witnessed a huge rise in the number of rural sites, filling out the countryside to a level unparalleled in the valley's history at any time before the early modern period (Lloyd and Barker 1981). The survey found a range of settlement entirely unsuspected from the classical sources—huts, farms, villas, cult centres, villages and towns.

Faunal material from our excavations indicates that the meat diet was drawn almost exclusively from cattle, pigs, sheep and goats. There was a rather intensive pig-rearing regime at the major farms such as Matrice, together with a generalized system of sheep and goat husbandry, with the only cattle eaten being plough cattle at the end of their working

lives. Cereals, legumes and vines were grown in the upper valley, and these were now augmented by olive cultivation in the lower valley. There is little evidence for a well-developed system of food production in the countryside to supply the *oppida*. Instead, the *oppida* and farms both sent prime meat animals to the rural sanctuaries—mainly young pigs and sheep, but cattle also to the principal sanctuary in Molise, Pietrabbondante. Given the difficulties for Roman farmers of maintaining even a plough team of oxen, killing prime cattle as part of the temple rituals was as conspicuous a form of consumption as any of the more permanent monuments of aristocratic display such as the increasingly elaborate defensive walls built round the *oppida* and sanctuaries.

Documentary evidence suggests that whilst the Samnites worshipped universal gods such as Jupiter, Mars and Apollo, the dominant framework of Samnite religion consisted of clusters of local deities in each *pagus*. The local dynastic family mediated between these deities and the ordinary people. The local deities were invariably symbols of the natural world, above all of the agricultural system. Our most detailed insight into how this local religion functioned has been provided by the Agnone tablet, a small bronze plaque found near the village of Agnone in Upper Molise in the last century and now in the British Museum. Inscribed in the local Oscan language and dating to about 250 BC, it lists the divinities who had altars in the sanctuary or sacred grove where it was displayed.

The grove was sacred to the cereal goddess Ceres, but the plaque also names over a dozen other minor deities associated with her, deities with influence over particular aspects of cereal husbandry such as rainfall, ripening, reaping, and threshing. The adjective *pius* linked on the tablet to the name of Jupiter, the dominating god of sky and weather, suggests that those in charge of the propitiating rites were in some kind of bargaining or *quid pro quo* relationship with the gods (Salmon 1967, pp. 176–7). It seems likely that the Samnite élites used the sanctuary rituals to mediate between the potentially hostile deities of the natural world and the ordinary people, to ensure success for the crops and animals (and human fertility too) on which life depended. The principal animal mentioned in the documents on animal sacrifices is the pig, the most frequent animal in the faunal samples from the Pietrabbondante and Campochiaro sanctuaries. Presumably the feeding rituals at the sacrifices served to articulate and maintain the social order by legitimating the authority of the Samnite aristocracies to treat with the divine world in the eyes of the peasant farmers who supplied the animals.

Whilst the valley escaped more or less unscathed from the Hannibalic Wars, the Social War of 91–82 BC (in which the Samnites sided disastrously against Rome for the last time) brought massive devastation and huge loss of life to Samnium. It is very difficult to recognize short-term phenomena using survey data—for example, rich Samnite sites with many fine wares can be dated much more precisely than the majority of sites which have few fine wares. However, the major fall in

the rural population of the valley, which can be discerned in the survey record of the late republican period, probably does reflect the *événement* of the aftermath of the Social War. With this final imposition of Romanization, Latin swiftly replaced Oscan as the dominant language, the élites embraced Roman styles of dress and behaviour, and their surplus wealth was channelled increasingly away from the sanctuaries and fortified settlements and into prestige monuments at the three major urban centres which were established on the Roman model in the lower, middle, and upper valley (Larinum, Fagifulae, and Bovianum respectively), and at Saepinum, a major new town to the east of Bovianum just beyond the watershed.

With the political reorganization by the Emperor Augustus in the early first century AD, the lower valley was formally separated administratively from the upper valley and linked with the south in Regio II, with the upper valley linked with the adjacent intermontane basins in Regio IV. Central funds were pumped into the major urban centres to encourage their development as market, cult, and craft centres, and they continued to be the focus of the political and social ambitions of the rich. The number of Roman sites found by the survey was only a third that of the number of Samnite sites (Figure 2.6): here as elsewhere in Italy large estates expanded at the expense of peasant farmers (Hopkins 1978). The process was less drastic in the lower valley, where a settlement infrastructure of major and minor towns, villages, villas and farms continued, but in the upper valley the Roman landscape consisted almost entirely of the new towns and major villas in their vicinity. The Biferno villas were far less lavish than their counterparts in Campania and Etruria (Lloyd and Barker 1981), but inscriptions suggest that most of them were similar in organization, run by bailiffs with a slave work-force for a wealthy aristocrat who lived most of the time in the town. For the Samnite aristocracies, the focus for displaying status and wealth moved from the sanctuary to the market-place, though they continued to supply the animals for slaughter at the sanctuary rituals that probably remained one of the few occasions each year when the Samnite peasant ate good-quality meat. By the late first century AD, at the time of the most prestigious urban building in the valley, the plight of the peasant farmers in Samnium became so severe and so politically threatening that the Emperor Hadrian had to initiate a food relief scheme for the rural poor.

In its mix of crops and animals, and local variations in these, Roman farming in the valley was much the same as Samnite farming, but it differed fundamentally in that the villas and farms were now part of a market economy operating variously on a local, Italian and imperial scale. Surplus production for the market-place is clear in the faunal and botanical residues. The locations of the villas suggest that the local urban markets were important, and it must be significant that new market facilities were built at towns like Saepinum. Beyond the valley, the

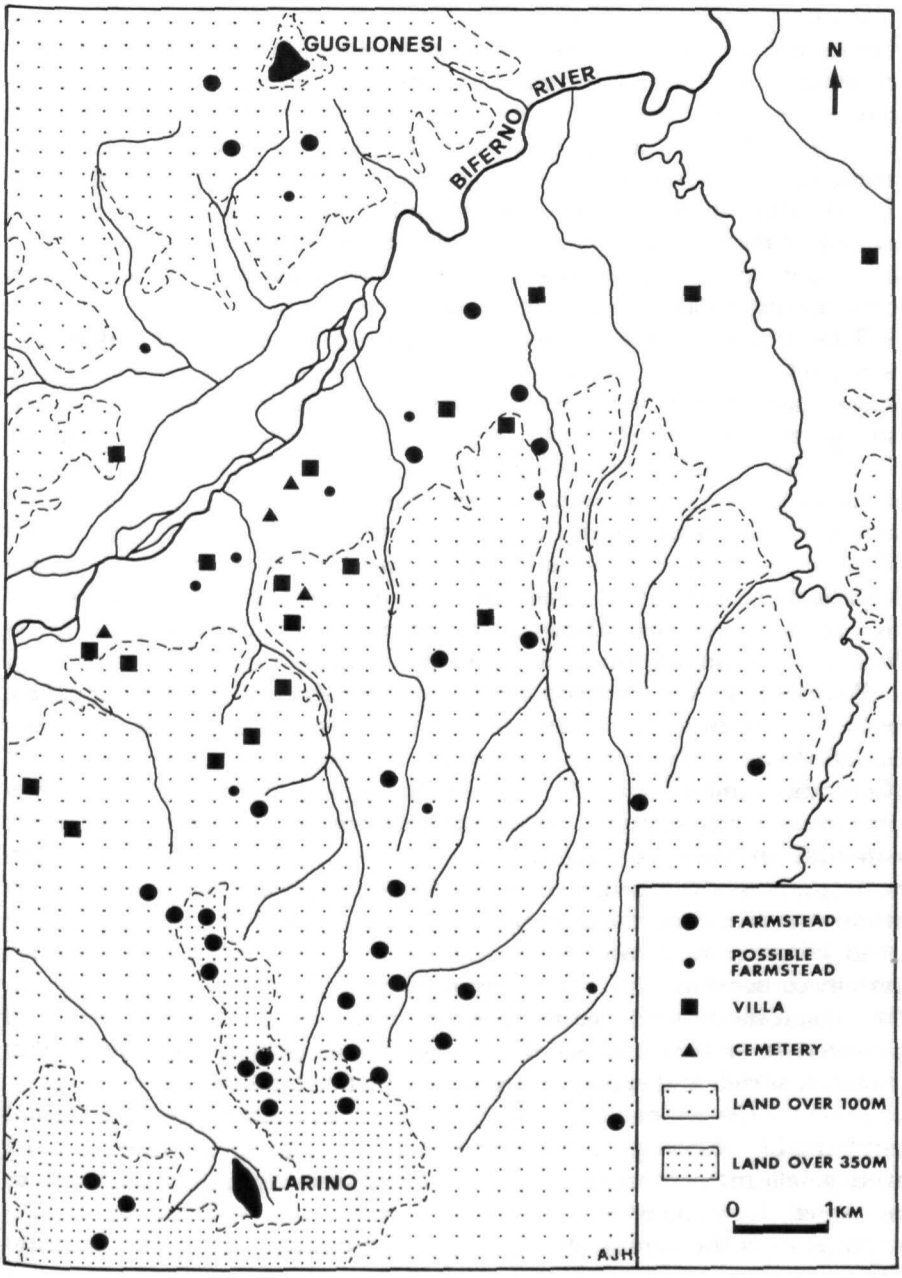


Figure 2.6 An example of classical rural settlement in the lower Biferno valley, between the modern (and ancient) settlements of Guglionesi and Larino.

towns of Campania and Rome itself must have been important markets. The Matrice villa, like other villas in southern Italy, was engaged in the intensive production of pigs for an outside market, and pigs dominate the faunal samples of Capua, Naples, Pompeii, Ostia and Rome. Pig products of all kinds were immensely popular with Romans of all social classes, and in the reign of Aurelian, when there was a free issue of some 10,000 kilograms of pork each day to the Rome poor, veritable armies of pigs were driven overland from southern Italy and shipped across from Sardinia to meet the demand. There was a highly organized system of meat-marketing, and material I studied from a fourth-century deposit in central Rome was the closest I have ever seen to the refuse from a modern abattoir (Barker 1982).

The cities of Campania and Rome also provided huge markets for wine, oil, cheese and wool (Potter 1987). A number of inscriptions in Molise attest to the involvement of the leading families in long-distance sheep transhumance, the flocks being kept mainly for wool (Pasquinucci 1979). Large-scale fulling facilities were established at Saepinum, a town on one of the main drove roads from Apulia to the central Apennines, and shepherd camps with fine Roman pottery have been found on the Matese and in the mountains further north. Most of the larger farms kept flocks of sheep principally for their manure, and inscriptions show that in the lower valley the landowners were also renting out any spare land to transhumant flocks for the same reason.

Beyond Italy, the *pax romana* at first created artificial trade monopolies with the western provinces, allowing the great slave villas such as Settefinestre in Etruria to specialize in intensive wine production (Carandini and Ricci 1985). Molise too was not unaffected by the opportunity, the Matrice villa for example being refurbished and greatly extended in the early first century AD, new facilities including major wine presses. In return, luxury goods reached the Biferno aristocracy from central and northern Italy, France and Spain.

In Etruria, there was a major economic crisis when the western provinces became wine producers rather than simply consumers, and the Italian monopoly collapsed. Settefinestre switched to the intensive production of pigs and wool as replacement cash crops. Whilst the political, social, and economic trends in the Biferno valley clearly reflect changes in the wider Roman world, the project's data do not indicate anything like the same specialized farming of cash crops. Local markets were small, major markets a long way away, and the agricultural system remained fundamentally broad-based and thus resilient to drastic change, until the general empire-wide catastrophes of the fifth century AD.

The archaeology of classical settlement in the valley clearly demonstrates the interweaving of the different strands of the Braudelian paradigm in the historical process. Unlike the case of the prehistoric data

from the valley, the impact of specific events can now be identified in the record of classical settlement, such as the foundation of the new Roman towns and the destruction of life and property in the aftermath of the Social War. At the level of medium-term changes or *conjonctures*, we can identify in the survey data the demographic trend in expansion from the fifth to the second centuries BC which was the primary reason for the Samnite migrations across the Apennines to Campania (and the resulting conflict with Rome). The reasons for such a trend at that moment in the valley's history are not clear. Certainly the adoption of polyculture broadened the agricultural base and allowed greater agricultural productivity, but as so often in the study of agricultural change in antiquity, the precise relationship between technological improvement, economic intensification, and population increase is unclear (Boserup 1965).

Conjonctures can also be recognized in the social and economic trends represented by the survey and excavation data. Particularly clear is the expansion of the villa estate at the expense of the small farmer, when the Roman aristocracies invested the wealth acquired through the growth of empire into land. Yet at the same time, the archaeology of the Biferno valley suggests that the responses made in this part of Italy reflected local factors first and foremost, above all the constraints and opportunities of the valley as an agricultural resource—the *longue duŕee*. In the lower valley, where conditions were the most favourable in terms of arable land, markets and communication systems, the Samnite settlement hierarchy of towns, villages, hamlets, large and small farms continued more or less unchanged into Roman times. In the upper valley, good land was at a premium, local markets existed in the new towns (and the transhumant systems running through them), and access across the Apennines to the markets of Campania was relatively easy, a combination which saw the growth of the villa estate and the virtual extinction of the small farmer. In the middle valley, where arable land was limited, erosional conditions were severe and communications extremely difficult, no towns were established and the subsistence farmer survived more or less unaffected by the urban economy, veritably 'cut off from History and the State' in Levi's evocative phrase.

The classical period also witnessed critical changes in *mentalité*. With the imposition of romanization, the Samnite élites embraced new lifestyles and new symbols of power, the sanctuary taking second place to the city. To what extent the peasants embraced the new order is unclear, but on the evidence of the Agnone tablet for the agricultural basis of traditional Samnite religion, and the plight of the rural poor as witnessed by Hadrian's food relief schemes, the likelihood must be that the *pax romana* certainly exaggerated—if it did not begin—the process of alienation between the peasant farmer and the ruling élite that so divides the *Mezzogiorno* today.

Medieval and modern settlement

From the late sixth to the tenth centuries AD, the Biferno valley was part of the territory of the Beneventans, a regional dukedom established following the Lombardic invasions. As in the rest of Italy, the period was characterized above all by a massive decline in population. Bovianum, Saepinum, and Larinum were probably deserted by about 550 AD, except as homesteads for the élite. The countryside was almost deserted, with rural settlements being small, defended, and widely scattered. Only one site dating to this period was found in the valley, S. Maria in Cività (D85 in the survey record), occupied from the sixth to the ninth centuries (Hodges et al. 1980). Our excavations revealed substantial fortifications, traces of an imposing church, and a surprisingly rich array of imported goods. Furthermore, whilst the community—estimated at about fifty adults—kept a range of stock on a subsistence basis, the abundance of cleaned and stored cereals with no evidence for harvesting debris suggested that by the ninth century the site was part of a more complex system of production than subsistence farming (Van der Veen 1985). The location of the site is also striking, controlling one of the few crossings of the middle Biferno (the Roman route in fact from Larinum to Trivento).

Hence Richard Hodges argues that the site was a *curtis* settlement or village belonging to the estate of a monastery such as San Vincenzo in upper Molise, which was extremely powerful at this time because of Beneventan and Carolingian patronage, the church being the cornerstone of the legitimation of Carolingian power. There was a *pars domenica* at the settlement for the lord and a *pars massericia* for the peasants, who probably had to supply cereals and other foodstuffs to the lord, who was in turn supplying the monastery. There is even evidence that the Beneventans, who needed gold for tribute to the Carolingians, were trading cereals for gold with the Aghlabid dynasty in North Africa. The ultimate consequence of this commerce, however, was the destruction of both San Vincenzo and the D85 village by Arab raiders in AD 871.

By the tenth century, the basic pattern of the modern hill villages had been established in the valley, invariably on the ridge tops. The eleventh and twelfth centuries were marked by considerable settlement expansion, with numerous communities of several hundred persons being established in marginal situations on the middle slopes. The lower valley in particular prospered because of overseas trade, the middle and upper valleys being less wealthy but as densely settled, with the valley as a whole sharing in the apogee of ecclesiastical art and architecture.

From this period onwards, however, the valley was characterized by Levi's 'remote poverty'. The early affluence of this half of Italy under Carolingian patronage, the Byzantines, the Arabs, and the Normans, generated a social order dominated by feudal lordship and the church that was too inflexible and rigid, that had outlived its purpose (Hodges

and Whitehouse 1983). By contrast, regions such as Tuscany which formed in the aftermath of the Carolingian collapse developed a social order embodying new rights and opportunities for the peasantry—typified for example in the extremely high quality of Tuscan peasant architecture in the eleventh and twelfth centuries—which were much more appropriate to the new medieval states of western and southern Europe. The 'other Italy' can be traced back in part to the importance of lordship in areas such as the Biferno valley in the ninth century, and the inability of this stultifying feudalism to change. There was a second phase of village expansion in the fifteenth, sixteenth, and seventeenth centuries, an ensuing contraction to the present system of villages, and then massive emigration of the impoverished peasantry to the Americas and Australia in the last century.

Like classical settlement in the valley, the post-Roman archaeology and history can be analysed fruitfully in terms of the *Annaliste* perspective. On the one hand, specific events such as the Lombardic and Arab invasions and a series of major earthquakes have certainly shaped the course of settlement; others, such as the Eighth Army's advance across the valley in 1943, left no trace beyond short-term damage in some of the coastal villages. On the medium scale, one of the most powerful influences on settlement has been the complex cycle of demographic expansion and contraction—the huge contraction of the early medieval period, the sequence of fluctuations in medieval and modern times, the massive emigration from the valley this century which has created larger populations of *Molisani* in Sydney, Buffalo and Bedford than in their home villages.

Over the same time-scale, the components of the arable and pastoral systems of land use have barely changed since the introduction of polyculture and long-distance transhumance in classical times, but the nature and organization of agricultural production have changed fundamentally and repeatedly, often in close harmony with demographic cycles though also in response to external economic forces. An important long-term factor has been agricultural technology, and the limitations it imposed on what soils could be cultivated, factors in essence unchanged until the introduction of heavy ploughs and caterpillar tractors in the post-war period. The new technology has transformed patterns of land use, but it is also provoking rates of erosion out of all proportion to anything in antiquity, which if unchecked are likely to have profound effects on settlement and farming in future years. Another very different but no less critical trend shaping peasant life over the same time-scale has been the power of the church as *mentalité*, whether operating at the level of local patronage or directly from Rome, Levi's 'paternal, prevailing institutions' binding the peasants to 'this shadowy land, that knows neither sin nor redemption from sin, where evil is not moral but is only the pain residing forever in earthly things' (Levi 1947, p. 12).

Finally there has been the *longue durée* of the valley itself, with its distinct internal variations in climate, topography, agricultural resources and natural communications, offering different constraints and opportunities for settlement and land use. As the project has shown, the valley environment has been far more than a simple backdrop to historical process, shaping the nature of relationships between people living in different parts of it and in turn between them and the outside world. Although I selected it as a coherent natural unit in 1974 for the landscape project I have described here, it has been chastening to discover that the valley only became such an integrated unit with the construction of the 'Bifernina', the new road running down its entire length completed two years before the project began—an *événement* likely to be at least as profound in its long-term effects as any of the past.

In detail, of course, Levi's vision of five thousand years of motionless poverty is clearly an exaggeration: in the case of the Biferno valley, we can plot a complex settlement history characterized by expansions and contractions, successes and failures, prosperity and recession, openness and isolation, and a changing relationship between the peasants and the ruling families. But in essence, too, Levi touched on the truth of what life has been like for most ordinary people for millennia, and particularly since romanization crystallized the division between the two Italys.

> I was struck by the peasants' build: they are short and swarthy with round heads, large eyes, and thin lips; their archaic faces do not stem from the Romans, Greeks, Etruscans, Normans, or any of the other invaders who have passed through their land, but recall the most ancient Italic types. . . . History has swept over them without effect. (Levi 1949, p. 137)

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3 Structural history and classical archaeology

Anthony Snodgrass

To put forward structural history, or indeed any other kind of history, as a model for archaeological approaches to the past is surely to privilege the status of historical archaeology. The claim should not be pressed too far, nor the privilege usurped entirely: it is perfectly possible, as Chapter 2 in this volume testifies in its treatment of the Mediterranean Neolithic, Bronze and Iron Ages, to apply to prehistory some of the insights gained from the *Annalistes*. In particular, one can frequently set periods of fairly rapid change against more durable factors, and perceive something approximating to the Braudelian dialectic operating between them.

However, events, and more especially individuals, can seldom be brought to life without the help of documentation. They may be the least decisive elements in the processes of history, operating often with transient effect and under the constraints of hidden forces; but the thousands of pages devoted to them in the works of the school show that their role, in the *Annaliste* vision of history, is nevertheless an indispensable one. Without them, the history that we can reconstruct is somehow incomplete: a keyboard (to use an image of Braudel's) without music. It seems to follow that the existence of some kind of historical narrative is a necessity for rounded history: it provides the foreground, while our research—whether as historians or as archaeologists—is directed mainly at the greater forces which lurk behind it. We may seldom be able, like Le Roy Ladurie, to turn a microscope on to a Montaillou, or to focus on a two-week period in 1580 in the town of Romans, and so bring to life the 'people without history'; but it will still be helpful to devote part of our attention to what may be called the 'official history' of the epoch, in the way that Braudel did in *La Mediterranée*.

Such at least is my reading of the *Annaliste* message; and it means that

certain kinds of archaeology are especially equipped to adopt that message. One such kind is classical archaeology; indeed I believe that it is uniquely well-placed for that purpose. But it would be invidious and unfruitful to try to press the claim by detailed comparisons. Instead, I shall attempt to set out a case, more or less in isolation, for the potential of classical archaeology to bring about an archaeological fulfilment of the *Annaliste* aims. At the same time, I must acknowledge, and try to account for, the evident fact that, so far, it has made very little progress towards such a goal.

The initial strengths of classical archaeology are, first, that it disposes of a huge body of archaeological data capable of being brought into contact with an adequate counterpart in the historical sources. Second, even in its strictest definition it covers a time-span roughly equivalent to that of temperate Europe from the time of Charlemagne to the present day, and a geographical area that is decidedly larger: this range is long enough and wide enough to observe every level of the *Annaliste* scheme of history in operation. Third, the historical documentation is not only quantitatively adequate for this purpose, but also in part qualitatively appropriate: that is, it represents the 'official history', but it also offers fairly frequent glimpses, through the study of inscriptions or of forensic speeches, or through the excavation of a Pompeii, of the other world of the people without history.

Why, then, is classical archaeology so seldom seen in this light? Why does it suffer from a reputation that is almost entirely at odds with any such aim? The answer lies partly in its origins. Classical archaeology grew up in an atmosphere that is paralleled by certain other branches of the subject (biblical archaeology, Egyptology), but that is independent of any of them. It came into being as a branch of classics: and the whole discipline of classics has been dominated, from its beginnings, by the factor which in turn brought it into being, the survival of the ancient written sources. That this should be true of literary, philological and philosophical studies is not merely acceptable but mandatory: the ancient sources form the very life-blood and subject-matter of these studies.

That it should be true of ancient history is less obviously justifiable. It is of course the existence of these sources which makes the epoch an historical one, and it is reasonable to use them as a starting-point in an analysis of any part of that epoch. The decisive factor, however, has been the collective interests, priorities and prejudices of these sources. In general, they represent the quintessence of the *histoire événementielle*. The historical writers amongst them, like every historian before the eighteenth century, saw the past (and equally strongly the present) as being composed of discrete, decisive events. Even those authors who, like Thucydides, enjoy the reputation of being exceptions to this rule, exemplify it at every turn. Thucydides for example finds it appropriate

to evaluate the Heroic Age, at the beginning of his first book, by means of a close analysis of the Trojan War.

By adopting wholesale these same interests, priorities and prejudices for themselves, modern writers on classical history for a long time cut themselves off from the kind of revolution in thought which the *Annalistes* were bringing about in medieval and modern history. No amount of skill in handling, balancing, synthesizing and adjudicating between the statements of individual ancient writers—and it is this *Quellenkritik* which has been the outstanding contribution of classical history—can enable one to surmount this limitation. The only means of doing that is to admit to the inquiry other classes of evidence that lie outside the world view of the ancient writers: that is, economic as well as political and military, environmental as well as anthropocentric, social as well as individual, rural as well as urban, barbaric as well as civilized.

In recent decades, many students of ancient history have seen this need and, to the great benefit of their subject, applied themselves to it. In doing so, they have also broken free from that bondage to the historical event, in which so many outsiders—including the archaeologists, and especially the 'new archaeologists'—apparently still believe that all history still languishes. This view is wildly out of date; it is out of date even for ancient history, as I have already implied. Only the most traditional ancient historians would today use the argument that, because they depended ultimately on sources who themselves had seen the past in terms of 'events', a similar obligation still rested on their subject.

How far can even this extenuation be applied to classical archaeology, which has pursued approaches of this kind equally assiduously, and for even longer? By what logic should the content of written sources be taken as binding on a branch of archaeology? Such questions are easily answered once we realize that classical archaeology did not, and to some extent still does not, see itself as a branch of archaeology. Rather, it has developed along two more or less separate lines.

The first is what we have already glanced at, its historically preordained role as a branch of classics. The second, which has given it a measure of independence, but which is, if anything, more notorious in the eyes of other archaeologists, is its function as the study of ancient art-history. Here the degree of independence is at best qualified and partial; but it nevertheless exists, as can be seen by comparing two of the traditional fields of activity of classical archaeologists: the study of pottery and the study of sculpture.

The former was a subject of no interest whatever to the ancient writers: its growth into a major subdiscipline has been achieved by quietly ignoring this fact, and by devoting to the topic a range of labour and talent which outsiders find baffling rather than awe-inspiring. To its exponents, it offers two main kinds of satisfaction: first, like any

highly specialized technique, it attracts merely by the level of knowledge and expertise which it has developed, and which therefore is demanded of any new practitioner. But second, despite growing up independently of the traditional interests of classicists, it has found itself becoming more and more useful to, and in demand by, other branches of classics. The apparent chronological precision which it has achieved offers obviously an attractive tool to historians; and, more widely still, the wealth of illustrative material has made it an indispensable adjunct to the study of everything from drama and religion to warfare and sexual attitudes. The result may not be generally recognized as 'archaeology', but it must be judged a striking success in its own, different terms.

Compare with this the study of Greek and Roman sculpture. Here the original inspiration may have come from the recovery of actual works of art; but the vital enabling factor has been the survival of a modest body of written evidence on Greek sculpture, most of it secondary in every sense, but still ancient. The importance of this second factor is shown by the path that this branch of study has followed. A voluminous tradition of scholarship has grown up, well over a hundred years old and still going strong today, in which the role of individual artists and schools is paramount. These artists and schools are largely known to us from what the ancient writers say about them; and where (as in the Archaic Greek period or in the original creations of the Roman Imperial period) such evidence is largely lacking, research has followed quite different paths.

For the classical period, however, we have critical biographies of sculptors of whose work no particle survives in the original; monographs devoted to lost statues; persuasive interpretations of surviving ones, designed to manoeuvre them into a niche in the written record; general art histories which order their material by means of a framework of great names. Here is a branch of scholarship which, in contrast to pottery studies, has taken its cue primarily from the ancient sources. That is not in itself a condemnation; yet it may not be a coincidence that, even after a considerable expenditure of learning and intelligence, our knowledge of classical Greek sculpture seems somehow more precarious, less serviceable, than that of Greek pottery. This may in part arise from the very different quantitative scales of the two subjects: but the fact is that a new find of Greek sculpture can be relied upon to project the learned world into a state of disarray which has absolutely no counterpart in ceramic studies.

However, classical archaeology has never—or at least at no time in the past 250 years—consisted solely of ancient art-history. There has also been a long-standing and robust tradition of excavation and fieldwork. Whatever may be thought of the methods sometimes employed in these endeavours, few will cavil at the equally robust tradition of detailed publication of excavated sites, which has survived delays and deaths and straddled world wars; the library shelves, with their thirty-

volume reports, bear massive testimony to a vigour which is hard to match in any other branch of archaeology. This is classical archaeology in its other main role, that of the materialist arm of classical history.

However, just as in classical history, there has been an unspoken assumption of the primacy of the ancient sources. Whereas in prehistory we expect to hear claims that a newly discovered site is representative and typical of its milieu, and even perhaps that it occupied an especially prominent place within that milieu, in classical archaeology it has usually been the other way round: sites have been chosen in the first place because they are known to have held a prominent position in the documented record. Excavators justify their choice by appealing to that record, and in their publication they make primary use of any data that the ancient sources offer. The notion of seeking out a site because of its very obscurity, in the manner of Le Roy Ladurie's search for 'people without history', would strike the traditional classical archaeologist as very odd. Even the publications of prehistoric Aegean sites have often striven for similar goals, but with Greek legend taking the place of written history. The underlying assumption throughout has been that the ancient writings intrinsically generate greater interest, and therefore deserve more attention, than anything which can emerge from archaeological findings alone. In this spirit, one branch of classical excavation has concentrated almost exclusively on urban sites, because they alone will feature in the historical record; while the other has directed its prime attention to sanctuaries which (as the written sources long since taught us) were the richest repository of works of art.

Classical literary scholars would in many cases warmly approve these assumptions and choices, and would offer an immediate justification for them in the literary quality of the ancient sources. This justification, perfectly valid for them, becomes much weaker once we turn to historical scholarship: for what kind of historian is it who evaluates his sources according to their literary quality? By a kind of extension, almost a sleight of hand, the fact that some of the ancient sources constitute great literature has been used as a way of privileging them all.

The really cold-blooded critic will want to ask another, more searching question: how do some of these writings earn the dignity of being treated as 'sources', when they belong to periods separated by several centuries, and sometimes also by major cultural distances, from the events that they describe? Will future historians, several millennia hence, regard Carlyle as a 'source' for the life of Frederick the Great, or even Winston Churchill for the history of the Protectorate? If they do, it will be a sign that they are as short of real, primary sources as we are for classical Greece and Rome. The irony is that, after all the deference with which they have been treated, it is the very paucity and indirectness of the ancient authorities which leads modern historians to take a rather sceptical view of the activities of their classical colleagues, or at least of

those who pursue their subject in a traditionalist manner. Ingenuity may be admired, but the admiration can be combined with fundamental doubts about the validity of the whole exercise.

This retrospective breast-beating has a purpose. For it is retrospective: it describes attitudes to the subject which many younger practitioners today would utterly repudiate. The purpose is in fact to set up a contrast between past approaches in which the methods of structural history could have no place, and those of the present and future ones in which I argue that these methods could find a uniquely favourable setting. This could happen in archaeology as in history, not merely simultaneously but concertedly: because one of the most important changes now taking place in classical studies is the breaking-down of the barriers between these two disciplines. I am constantly in contact with research students, both in Cambridge and beyond, who would be hard put to it to say in which of the two subjects they are working. This is happening precisely because both are moving sharply away from the traditional 'guild practices' of their predecessors. It may be freely admitted that the historians were the first to do this: for at least a generation now, they have been developing their discipline in quite new directions, some of them not easily matched in other branches of historical studies, others already consciously or unconsciously modelled on the work of the *Annaliste* school. Let us take first a primarily historical example.

Classical Greece was, on almost any definition of the term, the first slave society in Europe. Indeed, in the strict sense of a society 'where chattel-slavery was institutionalised as a . . . major form of surplus-producing labour' (Morris 1987, p. 173), it was perhaps the first in the world. The emergence of this chattel-slavery is a process which falls squarely into the class of 'broader movements of economies, social structures, political institutions and civilisations' which Clark (1985, p. 182) characterizes as the objective of one branch of the *Annaliste* inquiry—the study of the *moyenne durée*, of the time of *conjonctures*.

For some years past, the later development of Greek slavery, if not its initial emergence, has been subjected to intense study by one school of ancient historians, to the point where other, more traditionalist authorities have tried (unavailingly) to cry 'Halt!' (e.g. Starr 1977). The methods used have been, in part, very similar to those used by the *Annalistes*. The phenomenon of Greco-Roman slavery has been looked at in its long-lived entirety; some attention has been given to its eventual replacement, after a career of well over a thousand years, by what was to emerge as medieval serfdom; and the perspective has been lengthened still further by comparative study of modern slave-owning societies like the American South before 1865.

If this is not the study of the *longue durée* in its primary sense of the timeless physical constants of Mediterranean life, it certainly approximates to it in being the investigation of one of the unchanging realities, the unquestioned preconditions of many centuries of Mediterranean

social history. The *conjonctures* are the processes whereby a series of new social systems incorporated the element of chattel-slave ownership, or whereby major external changes, such as the conquest or exploitation of new barbarian territories, brought new populations into the existing system. The *histoire événementielle* is provided by the occasional 'windows' opened on to the regime of slave-based production, principally by a few well-documented major slave revolts.

Evidence of many kinds has been used with, latterly, archaeology and even art history making substantial contributions (e.g. Carandini and Ricci 1985; Himmelmann 1971). This example recalls the *Annaliste* approach in another way also: it deals with a question that was largely passed over in silence; not only by the ancient sources who provide the framework for the traditional historiography, but also (and in consequence) by generations of modern students of Greek and Roman history.

No one who has read the writings of Moses Finley, on this (e.g. Finley 1980) as on other social and economic questions, could possibly continue to cherish such a primitive vision of the historian's craft as Lewis Binford, for example, apparently does. Does he really still believe that historians work entirely, or even mainly, from 'chronicles, letters diaries or any other sort of literary record' (1983, p. 20—emphasis added)? Finley was famous (in some eyes, notorious) for his scathing treatment of much literary evidence. But then, I doubt whether many New Archaeologists have read Moses Finley.

Nor, until quite recently, did many classical archaeologists: this is an illustration, and perhaps a partial explanation, of the slow and reluctant response of the discipline to new approaches, by comparison with that of ancient history. This delay is much to be regretted, but fortunately the opportunity has not yet passed. This opportunity is, I think, found at its best in the sphere of the *moyenne durée*. It is on this level that the recent historians of Greece and Rome have achieved some of their most powerful advances, by addressing such issues as settlement change, state formation, demographic cycles, famine and price movements. All that is today needed is for the classical archaeologists to join their historian colleagues in this preoccupation with the *conjonctures* of Greek and Roman history. The possibilities would then be almost limitless for, whereas the ancient documentary sources are deficient by the standards of modern history, this lack could be made good by the incomparable wealth of the archaeological data. Gradually, that whole vast repository of excavation finds and specialized monographs could be unlocked and put to new uses.

Consider, for example, the fifty-year-long diversion of prehistory with the concept of 'a culture', and with the equation between an archaeological culture and a linguistic or ethnic entity. If this concept is now at long last discredited—and in another session of the TAG (Theoretical Archaeology Group) Conference, where this paper was first given, Colin Renfrew was heard to describe it as 'a complete failure'—

then surely there are some lessons which can be learned from the case, and applied for the future benefit of archaeology.

First of all, it is salutary to see why the concept offered such attractions for so long. The explanation offered by Christopher Chippindale, in a paper at the same conference, must be along the right lines: that the prehistorian's 'cultures' were devised as counterparts, or 'shadows', for the tribes, states and empires of the historian. Prehistory was therefore assimilating itself to the history and especially the ancient history, of its day: tribes, states and empires are the essential basis for political, military and constitutional narrative. The eventual rejection of this model by prehistorians finds its counterpart in the roughly contemporaneous adoption of wider aims on the part of ancient historians.

Suppose, however, that in the heyday of the 'culture/people equation', the model had been systematically tested against classical cases, where the archaeological assemblages offered a fair comparison—burial-forms and grave-goods, simple domestic architecture, metalwork and pottery—but where the linguistic and ethnic outlines could be fairly accurately delineated in advance. Early Rome and its relationship with the other towns of Latium, or to the Etruscan city-states, would have been an excellent test case; so too would the Greek peninsula at the dawn of the historical period.

What would have happened? The conclusion seems inescapable that the 'culture/people' model would have been modified into a very much more sophisticated version than the one that prehistorians were content to use. In the Greek example, the geographical extent of the Greek language would have suggested an initial culture boundary; but within it, the investigation would have brought to light, and been forced somehow to accommodate, every kind of contradiction in material culture—between inhumation and cremation, between single and multiple burial, between innovatory use of iron and lingering dependence on bronze, between painted and unpainted pottery, between stone and mud-brick construction. Worse still, the dichotomies would not have run in parallel, but cut across each other. At the end, a reasonable response would have been: 'Where are your "cultures" now?'

Alas, it never happened. But the value of such a test case is not only retrospective. It could still be put to use now, to examine the kinds of *conjoncture* that are involved in the emergence of these regional boundaries in material culture, in the later Bronze and earlier Iron Ages; in their partial submersion, with the rise of the much smaller territorial subdivisions that came with the city-state; in their latent survival and partial re-emergence when the city-states in turn declined; and so on, right down to the question of their operation in the social and political life of modern Greece. The *longue durée* would come into the picture too, with questions about the effect of natural barriers and agricultural fertility on the formation of the boundaries. Only for earlier prehistory, with the pursuit of questions such as the large-scale shift in the concentration of settlement between the Neolithic and Bronze Ages, have the

possibilities of such inquiry been at all fully explored in Greek archaeology. This is merely one set of problems in one small corner of the Mediterranean world.

The value of early Greece and Rome as test cases derives from their combination of relatively small geographical scale and relatively rich archaeological record. Their destined historical importance need not, at this stage, enter into the picture: it is precisely because they exemplify, if not universal, then at least very widespread kinds of human development and endeavour that they have so much potential utility. Another kind of medium-term process where they could be used to advantage would be the bronze–iron transition—perhaps the only change in several millennia which was eventually to affect every corner of Europe, vast areas of Asia, and northern Africa too. The role of the Mediterranean Sea in furthering this change was certainly critical, and possibly even more profound than archaeologists have yet appreciated. The introduction of iron-working was brought about, in one area or another of the ancient world, by every kind of cultural transmission imaginable: independent technological advance and imitation; warfare and exchange; cultural dominance and conquest; adaptive response to circumstances; aesthetic and even perhaps religious preferences.

The attention of archaeologists, since the time of Gordon Childe, has been diverted from this extraordinarily rich, significant and potentially rewarding problem by another (and admittedly even more fundamental) question: that of the initial adoption of metallurgy. But when Childe's work on the bronze–iron transition is resumed, the attractions of the subject will also be rediscovered. Because of its occurrence in protohistoric or even fully historical times, the bronze–iron transition can be investigated with a precision that is quite impossible with its stone–metal predecessor. What provenance studies have done for the latter, technical analysis of early ironwork can do for the former, and the conclusions are likely to be much more far-reaching.

Finally, here as elsewhere, examples such as early Greece and Rome would act to interlock archaeological change with historical process. What connection is there between the adoption of iron and the subsequent emergence of Rome from the status of an obscure town in central Italy? Would the Greek city-state have been a possibility with a bronze-based economy? Is the knowledge of iron reflected in the Homeric poems applicable to a wider range of societies, beyond the horizons of Greek Ionia? At present, few would feel able to offer even tentative answers to such questions, let alone the wider ones which soon arise from them: how far is any Iron Age really an iron age? Did iron really 'democratize' agriculture and warfare, as Childe at first thought? Why does the last of the 'Three Ages' also have the least clear definition of the three, in archaeological terms?

All of these, except the last, are questions which, in the classical context, can be addressed by a combination of archaeological and historical approaches—but with archaeology firmly in the driving seat.

All of them (even the Homeric one) could be confronted with bodies of readily quantifiable evidence, arranged in an accurate framework of relative chronology. If this is not inspiration enough, then just imagine what use Braudel would have made of the bronze–iron transition, had it only occurred within the time-span covered by his study. On the level of *histoire événementielle*, there would be vignettes about Hesiod warning his brother to stay away from the blacksmith's shop in their native village, or traders in the Odyssey exchanging native iron for foreign copper, with perhaps a sideways glance at the later Chinese texts disparaging the 'ugly metal'. The *conjonctures* might include a picture of iron halting, then reversing the old ascendancy of Asia over Europe, or of iron enabling the Mediterranean world to turn its back on Asia and fulfil its destiny in Europe; culminating, in the *longue durée*, with the Age of Iron at last transcended by the growth of nuclear weapons, devised in large part by European scientists, but transferring the control to two great powers, one of which lay outside Europe and Asia and the other of which straddled them both. All of this would be described, as I cannot describe it, in terms so persuasive that no one would call it an exaggeration.

The work of the *Annaliste* school is innovatory in more ways than the one on which all commentators have concentrated—that is, the attempt to orchestrate the different rhythms and time-scales of history. Let us look for a moment at one feature that is in part an effect of the emphasis on the geographical constants of the *longue durée*, in part a result of the search for 'people without history'. I mean the emphasis placed, most obviously in the work of Le Roy Ladurie, on the inhabitants of the countryside and the small towns. If this runs counter to the concern of traditional history with great men and great decisions, it does so even more obviously with traditional ancient history, which has followed the ancient authors in their assumption that nothing worth commemorating, except battles, could take place outside the major cities of their world. More resoundingly still, it contradicts the emphasis of traditional classical archaeology, which has been drawn irresistibly to the great sites of history, and has lingered there even when others were casting their eyes further afield.

Part of the attraction of the rural sector of civilization for the *Annalistes* lies in its slow pace of change and uncomplicated structures, as exemplified in the quotation from Braudel cited by John Bintliff (this volume, p. 9) (and see Barker, Chapter 2 of this volume). The rural population is often a witness to the transient or even non-existent effect of the apparently momentous decisions taken at the top of the hierarchy. The small towns, for their part, can act as spokesmen for the passive recipients, as opposed to the active initiators, of policy: more articulate than the countryside, they can offer anything from over-zealous implementation to dogged resistance, in respect of the decisions taken by rulers in their capitals. They may breed martyrs and rebels, but any

'great figures of history' that they produce can become such only by deserting them.

How could this difference of emphasis be paralleled by a change of attitude in archaeology? Once again, it reflects a distinction which only makes sense in the case of urbanized, that is fairly developed, societies. For much of prehistory, the urban/rural division does not exist. So we are led back once again to fields like classical archaeology, where on the contrary it is one of the vital dichotomies in the societies studied. For its investigation in the Greco-Roman world, archaeology is bound to be an indispensable tool, since the documentary record tails away steeply as we move from the major conurbations. More and more people, even in classical archaeology, are today realizing that this investigation is worth pursuing, and for much the same reasons as the *Annalistes* have pursued it. The man-made rural landscape is the embodiment of the *longue durée* and *moyenne durée* in action: and when Braudel writes, on the final page of *La Méditerranée*, 'In historical analysis as I see it, rightly or wrongly, the long run always wins in the end' (1972/3, p. 1244), this expresses in broader terms what Aneurin Bevan meant by his remark, 'Where the countryside is neglected, it always takes its revenge' (1952, p. 38).

Intensive archaeological field survey is the obvious means whereby the archaeology of historical societies can rectify fairly rapidly, this imbalance in past generations of research. Significantly it is this which John Bintliff has chosen to illustrate 'the potential of the *Annales* paradigm' in archaeology (this volume, pp. 19–26. By addressing itself, as is today most usual, to all periods of human settlement in the area surveyed, it automatically brings both the *moyenne durée* and the *longue durée* into its sights. This is difficult to achieve by excavation anywhere, and virtually impossible by excavation in the rural landscape. John Bintliff provides examples of *moyenne durée* from archaeological field survey across whole landscapes.

Let me offer an example of the *longue durée* from similar data. Any site which has been more or less continuously in occupation from neolithic times to the 1980s—and there are such sites—is likely to have become, by the historical period, a fairly important urban centre. However, the real limitation in excavation lies, not in the vertical but in the horizontal dimension. The fraction of a large urban site that one can realistically hope to excavate is small, and that of a rural landscape far smaller still. The ability of excavation to generate regional insight and knowledge is very restricted, if indeed it exists at all (cf. Cherry 1984). For the present and the foreseeable future, there is no realistic archaeological alternative to field survey, as a means of bringing the rural sector into the historical limelight. This explains the sudden recent popularity of the technique in Old and New World prehistory, and now in the archaeology of pre-classical, classical and post-classical Italy and Greece.

With the small towns, the prospect is not quite so one-sided. Histo-

rians, with the indispensable aid of epigraphy, can make considerable headway here, once they disavow the time-honoured pursuit of historical 'importance'. Moses Finley, in his last book (1985, pp. 62–6), lambasted the tradition of monographic studies of individual cities, in which every available morsel of fact had been swept together on the single pretext that they all concerned the same place. This is not what is needed now; and anyway most of the cities chosen for this treatment had been, once again, the historically important ones. What is wanted is the comparative study of small town with great, and small town with small, on a level of knowledge that is as evenly balanced as possible between the places compared. Historical documentation, inscriptions and, in this case, carefully planned excavation would all play major roles.

The size of the void that has to be filled in classical archaeology is brought out by reflecting on the case of ancient Greece: the number of city-states which existed, each with its own urban centre, in the heyday of the system runs into hundreds: more than 200 were members of the fifth-century Athenian League alone. What proportion of these are known quantities archaeologically? In the colonial sector, the figure is respectably high, partly because Greek colonization (leaving out Alexander's conquests) affected about fifteen modern countries, and each of these has a vested interest in what was often the earliest historical epoch in their heritage. In the homelands, the proportion is very much lower: there are numerous cases where the very location is in some doubt.

However, the case of classical Greece is a special one, in two respects. First, because of the fragmented political system just referred to, most 'small towns' were not in a position of permanent subordination to a capital, or primate centre: rather, they existed as independent, if obscure, neighbours of the more prominent exponents of the same system. Technically, they were all on the same political footing. Second, the Greek idea of citizenship, irrespective of the political ideology adopted, was one that extended to the whole free adult male population, whether living in the city, in a village, or in an isolated farmstead. The countryside, in other words, had a status that was privileged by comparison with most other regions and epochs. The country-dweller could exercise whatever political rights were extended to his urban counterpart; and, thanks in some measure to the small geographical scale of most city-states, these rights could be effective rather than nominal.

What this means is that the understanding of life in the countryside and small towns of Greece which we shall gain will not be universally valid or automatically transferable to other archaeology: not even to that of the peasants of Republican Rome. The grass roots of Greek civilization belong, as it were, to a rather special type of plant. This is in one sense a disadvantage: a sense which did not apply to the case of the bronze–iron transition which we considered before. In another way,

however, it brilliantly illustrates what the *Annaliste* approach seeks to investigate: the way in which a *conjoncture* such as the rise of the city-state system can, for a limited period of a few centuries, divert the forces of the *longue durée* into a different channel. The later history of Greece itself amply exemplifies how 'the long run always wins in the end': the city-states merge into larger and larger groupings, the small towns become provincial in every sense, the gulf between town and country widens again. The repercussions of all of this will be picked up in a good all-period survey.

All the programmes and approaches suggested here for archaeological implementation have been at their strongest on the middle level, that of the *moyenne durée*, and weaker at the two extremities. This is I think a fair expression of the specific potential of classical archaeology; in prehistory the balance would be different. But let me direct a few words at these weaknesses at the extremities, beginning where Braudel ends, with the level of events and individuals. Of course, we in classical archaeology are lucky in our volume of documentation by comparison with the prehistorians; but by the standards of the medieval and early modern periods where the *Annalistes* have done much of their work, we are at an equally obvious disadvantage. With a few isolated and local exceptions, like the Fayum papyri from Roman Egypt, the excavation of Pompeii and now perhaps the Vindolanda documents, we simply do not have the material with which a whole ancient community can be 'trapped' like the population of Montaillou. And yet, it may be possible to reorder our archaeological data, perhaps along the line of the 'acts of communication' model as advocated by Bintliff (this volume, pp. 13, 29), and so to fill this important gap in a way that even the *Annalistes* themselves have yet to do. Changes of style and of deposition practices, the visible confrontation of new problems and asking of new questions, inspired (and uninspired) experimentation are all 'events' of a kind that archaeologists are used to handling. If there is anywhere in archaeology where they are capable of being moulded into a millennia-long framework, then it is in classical archaeology.

But what of the *longue durée*? Here I am going to make the presumptuous, perhaps offensive claim that a little more consideration of ancient history might have led the *Annaliste* school to revise their interpretations and possibly their descriptions of the physical constants of Mediterranean life. Consider Greece, a 'compact, mountainous peninsula' if ever there was one. In Braudel's work it receives fairly short shrift, as befits the obscure appendage of the Ottoman Empire that it had by then become. However, the opening pages of the second volume are devoted to a phenomenon, the fifteenth-century eclipse of the city-state, which has a curiously familiar ring (Braudel 1972/3, pp. 657–9). At least once before, the Mediterranean had been through a similar process, and of at least one earlier period it could be said, as Braudel says of the fourteenth century, that 'the Mediterranean belonged to its towns, to the city-states

scattered around its shores'. Florence, Venice and Barcelona were re-embodiments of a system that had flourished two thousand years before. In this earlier manifestation it was the Greeks who, for much of the Mediterranean littoral, were the most visible and thus the most imitated exponents of this system.

Two interesting points emerge. First, such a system may not have been peculiar to the Mediterranean, but the repetition suggests that there is something very favourable to it in the Mediterranean environment: favourable, that is, not merely to its existence but to its success. For the surprising thing is surely not that this system made way for the territorial state, but that it flourished so long, twice over, before doing so. Second, there is the role of Greece. Small as Mediterranean peninsulas go, it yet enjoyed this period of ascendancy over much of the Mediterranean while the tendency towards political fragmentation was at its height, in the archaic and classical periods, much as Italy was to do in late medieval times. Its main rivals, the Phoenicians and Etruscans, were also peoples based on small city-states.

'Precarious and narrow-based . . . a fragile form of government, doomed to extinction' cannot be a fair and final epitaph on the Mediterranean city-state. There is surely something else to be said, something that belongs to the *longue durée* rather than to a once-for-all *conjoncture*. A multilateral interplay of forces must have occurred and recurred, in which one suspects that the sea itself was the prime factor. Interrupting the territorial extension of the great empires which, at both these periods, abutted on the Mediterranean without being able to master it, the sea must also have acted as a kind of power filter, perhaps because of the limitations of premodern, oar-powered ships and battle-fleets. The city-states, who could muster fleets strong enough to protect their own shipping, manned by people who had the Mediterranean in their blood, found themselves in possession of a regional monopoly of power which took a lot of breaking. When their downfall came, it came at the hands of other Mediterranean powers which had grown into territorial states, never of complete outsiders.

The Roman Empire was the first such power, and its domination spanned more than half of the long period between the two ascendancies of the city-state. Here we find another curious response in the pages of *La Méditerranée*. Braudel refers to the Roman Empire from time to time, but almost always with a kind of impatience, which betrays the recognition that it forms the exception to almost every rule laid down about the *longue durée* in the Mediterranean. Its conquest of the whole of the basin is once referred to as 'monstrous' (1972/3, p. 166), at other times merely as exceptional (e.g. p. 121, p. 165). However, in the original preface of 1949 (retained in the revised and translated editions), the impression given is almost as if the Empire had never existed. This feeling grows, as one passes from phrases like 'The Mediterranean is not even a single sea, it is a complex of seas' to 'It is not an autonomous

world, nor is it the preserve of any one power' (1972/3, p. 17). Yet the Roman Empire did not merely encompass the whole of the Mediterranean littoral: it drew its life-blood from it, with the continental provinces of Europe and Asia playing shorter-lived, secondary roles. Whatever brought about its fall, it would be hard to argue that it was the intractability of the Mediterranean basin as a power base, even in the long-term. After all, this historical episode lasted far longer than the circumstances that were described and explained in *La Méditerranée*.

These criticisms are not, however, meant to be central or damaging. I make them purely in order to advance my claim that the extension of the *Annaliste* approach to the ancient world would not only have a great impact on that field of study: it might also result in some modifications of the *Annaliste* approach. Indeed, this process is perhaps under way already. Certainly the periodical *Annales* itself devotes articles, and occasionally whole issues, to the classical world. Archaeology has so far played a minor role in the drama, but I am not the only author in this volume to have laid stress on archaeological field survey, and it is a telling sign that *Annales* was the first general periodical actually to solicit a contribution on this technique, once it began to be widely adopted in Mediterranean contexts (Snodgrass 1982).

What really is central to the case is the fundamental Braudelian 'disjunction between what things meant for agents and what they reveal of certain conjunctural and structural realities known only to historians' (Clark 1985, p. 189). This disjunction, contrary to widespread belief, would today find strong support among ancient historians and even classical archaeologists, my strictures on past practices notwithstanding. This is just as well, for when it comes to co-ordination between archaeology and structural history, then classical archaeology, far from being the eccentric or provincial outlier that many prehistorians see in it, must be in the absolute forefront.

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4 The place and role of the *Annales* school in an approach to the Roman rural economy*

J. P. Vallat

If we reread the three volumes of F. Braudel's *Civilization and Capitalism: 15th–18th Century* and the three themes addressed by them—the Structures of Everyday Life, the Wheels of Commerce, and the Perspective of the World—the question arises of whether these themes and working practices are currently applicable to research being carried out by historians into the Roman rural economy. The increase in data over the past decade is very largely due to the attention accorded to archaeology—i.e. to material structures—whether it involves enumerating rural settlements and drawing density maps, or measuring, within the economy as a whole, the nature and importance of trade derived from agrarian or artisanal production, and its circulation within small, medium or large trading radii.

However, the attention given to the quantification of data further highlights certain historical problems, and underlines the difficulty in interpreting this new archaeological material in relation to literary sources which have long been a matter for debate.

Several questions must be considered: the agrarian crisis of the third to second centuries BC; the romanization of the provinces; the social formations of antiquity; societies of orders or of class; and the primitive or modern nature of these societies. By attempting to take several

*Translated from the French by B. Halpin, B. Lovett, J. Pym and D. Whitehouse, Modern Languages Department, University of Bradford.

dimensions of historical reality into account, and by seeking to provide a simultaneous analysis of long-term trends and short-term cycles, dominant characteristics and mutations, is the *Annales* school enabling the ancient historian to write a page in the history of the Roman economy? Is it a question of method(s)? In his debate with Y. Thébert and P. Leveau, M. Benabou clearly pits a method based on nuances, doubts and a wide range of types of information against one of certitude and dogmatism (*Annales = AESC* 1978, 33, p. 83). G.G. Diliguenskij, on the other hand, indirectly condemns the *Annales* school's limited approach to economic and social questions as totally bourgeois and even neo-positivist (*AESC* 1963). Conversely, in the following year (*AESC* 1964), P. Vidal Naquet called certain myths into question, such as oriental despotism or the Asiatic mode of production, which, as we shall see, reappear in the debate about the romanization of Africa.

In this edited volume it is appropriate to recall the considerable contribution of the *Annales* school to this demystification, or rather to the shedding of light upon links between certain concepts and the quite specific historical situations in which they came into being. The way in which the Marxist approach to theories of Asiatic or slave-run modes of production evolved between 1884 and 1931, and then from 1931 to the present day, has thus been dependent on, among other things, the pattern of events in the 1917 Revolution; the founding of a bureaucratic group opposed to the decline of the state; and the formation of a new 'Oriental despotism' in China and the USSR. Similarly, the conceptual research conducted by D. Thorner on the peasant economy in the year when the critical article by P. Vidal Naquet on the work of K. Wittfogel (*AESC* 1964) appeared, is also of central interest to us. It will be sufficient in this chapter to examine three themes in Roman history, the value of which no research project can afford to underestimate, and which the journal *Annales* has dealt with: the agrarian question and the analysis of the countryside, the Romanization and evolution of indigenous societies, and societies of orders and classes. It is not our intention to delve deeper into these issues, but rather, in line with the aims collectively defined by the authors of this book, to draw up an inventory of the points at which the *Annales* school has contributed to these areas of thought.

I The agrarian question: the archaeology of a debate, or a debate on archaeology?

It can be said without hesitation that the agrarian question is one of the principal preoccupations of a number of historians from the *Annales* school, and represents a long-established historiographical trend. With this volume in mind, it is necessary to read and reread the article which M. Bloch devoted to plans of plots of land (*AHES* 1929,1), the methodo-

logical bases of which reappear in his rural history (M. Bloch, 1952). It must honestly be confessed that long lines of historians, especially medievalists and modernists, have already prepared the ground for this research: A. Verhulet, H.G. Hunt, N.G. Svoronos (*AESC* 1956) regarding domanial structures in Belgium, England and Byzantium; E. Julliard (*AESC* 1961) or G. Duby (*AESC* 1960, 1962) in reviews of works on agrarian geography or rural medieval history; M. Roncayolo, P. Courbin, J.M. Pesez and E. Le Roy Ladurie (*AESC* 1965) on the theme of deserted villages, developed when the Ecole des Hautes Etudes published the proceedings of a symposium on the subject. This is history outside the bounds of traditional universities and a type of work in which the *Annales* school is often engaged.

In ancient history the theme of rural history remained in the background, apart from the article by A. Chastagnol on A. Piganiol's book about the cadastral survey of Orange (*AESC* 1965) and R. Chevalier's article on aerial photography (*AESC* 1963). Of course, some subjects relating to agrarian history were dealt with in the context of studies on Africa, as we shall see when we tackle the questions of romanization and indigenous societies.

However, it was not until 1982–7 that a series of articles appeared concerning the countryside and the occupation, appropriation and expropriation of land (Chouquer et al. *AESC*, 1982; Leveau *AESC*, 1983; Vallat and Fiches *AESC*, 1987). The journal not only expressed an interest in this sphere, but by means of a series of reports on identical questions also ensured that a coherent overall examination was carried out. There was plenty of scope, therefore, for the *Annales* school to study the agrarian question.

But, the way in which this issue was tackled cannot be separated from the general context of production in past times. Indeed, from 'The Gracchi' to 'The conquest of the Mediterranean World' (Nicolet 1967 and 1977), it is possible to measure the progress of a debate on the agrarian crisis/question, the two terms themselves conveying the change of perspective. In one, the rural exodus and the urban 'crisis' are seen as the most important of the explanatory factors, whilst in the other, archaeology in particular allows for a reassessment of the scale of the demographic crisis. This development had a very direct influence on English historiography, notably on the works of M. Frederiksen and G. Métreaux (Frederiksen 1984; Cotton and Métraux 1985) where the archaeological dimension of research on northern Campania is reviewed in the light of a more complex range of historical phenomena. It is symptomatic that both books indicate a new consideration of research material which had previously been to a large extent obscured (J.P. Vallat 1980, 1983).

Does this mean, however, that we can speak of influence from an *Annaliste* school in the portrayal of the ancient rural economy, either in the form of a break with the positivist or descriptive school (where

archaeology remains isolated from all context, making up no corpus and remaining a study of beautiful artefacts), or of a break with certain forms of Marxist-type discourse (favouring grand structures and global explanatory schemata at the expense of the fringes of a particular system, or even subordinated groups within the system)? We have to recognize that the latter contribution has been among the most fruitful in the sphere of material civilization, as it has brought together all archaeological data and developed the idea of quantification in all fields of research. Nevertheless the type of language it developed often proves incapable of dealing dialectically with complex societies. The very fact that modes of production are treated principally, if not exclusively, in terms of economic superstructures prevents a thorough analysis of the durability of one economic system in comparison with another, sociological and administrative sluggishness, and the differing speeds at which each component of social formations developed (De Neeve, 1984).

The agrarian question cannot simply be seen as the inexorable decline of the tiny smallholding, associated with the exploitation of the *ager publicus* (public land), principally in the form of *compascua* and *silva* (pasture and woodland). First, the tendency for the population density of the land to decline between the fourth and second centuries BC is not proven. This could be true of certain regions, like northern Etruria, but it is uncertain that this was the case in the south, in the other regions of Italy, or elsewhere in the Mediterranean basin. Certainly, the landholdings which were assigned during the early days of the Republic, consisting of 2–10 *iugera* (10 *iugera* = 2.5ha), and which are recorded by Titus Livius, seem to fade from our literary sources towards the end of the Republic, but Cicero and other commentators on the *Lex Iulia Campana* still talk of plots measured in a few *iugera*. To convince themselves of the decline of the smallholding, some writers categorically state, without having any evidence, that all the recipients must have resold their plots.

Conversely, archaeology proves that until 30 BC certain types of small-scale exploitations could be found in Giardino and Via Gabina (I. Attolini, *Archeologia medievale* 1983; K. Painter, Roman villas in Italy, 1980), and in Fabrizio and S. Angelo (J.C. Carter, *AJA*, 1982). Indeed, often these small farms first appear in the second century and up to the end of the first, with *fundi* (estates) which do not seem to have exceeded 10 *iugera*. However, we must not confuse ownership, pertaining to the allocated plots, with exploitation, a concept which archaeology seeks to define. A study which accepts the complexity of the situation must take these facts into account and try to couple them with other types of development, like that of the *villa*, slave-run or otherwise. Indeed, here too it is necessary to clarify the type of materials at our disposal: the excavators at Posto and San Rocco (Cotton, 1979; Cotton and Métraux, 1985) never declared with any certainty that they were dealing with either slave-run *villae* or important *fundi*. On the contrary, they conclude from their study that the size of the exploitation must have been around 50

iugera—probably less, given the density of equivalent surrounding structures—and that the most likely way of exploiting such units seems to have been to do so directly, with a reduced *'familia'* barely revealed by the excavation itself.

In Settefinestre both the reconstituted *fundus* and the methods of exploitation represented a fairly considerable surface area (several hundred *iugera*) and were exploited using a slave-based method. However, an analysis of the material structures cannot stop there, as it is necessary to understand why this type of slave-run *villa* was different from the other type of *villa* exploited directly by the family. It must also be established why the Etrurian *villa*, the most widely studied villa-type, came into being only at the end of the Republic, and not at the time of the famous agrarian crisis of the third to second centuries BC, the exact period when the slaves are reputed to have invaded the countryside around Cosa and to have poured into all the markets around the Mediterranean.

Can we provide an answer by linking these long-term phenomena with shorter-term trends? Can we consider the components of the rural economy to have reacted in different ways according to general requirements, such as urban consumer or military demand, or, indeed, the expansion of markets due to conquest? Can we assess the ways of accumulating income from property, from the profits earned in monoculture and stockraising to those earned from vine and olive-growing and other commercialized forms of cultivation?

There is no doubt that by examining, in three different yet complementary lights, the questions of the ancient town and the organization of rural space (*AESC* 1983), of the towns and countryside (Aix 1982) and of the origin of wealth spent in the town (Aix 1985) that P. Leveau, P.A. Février, and some researchers associated with them, have undertaken most effectively to bring a 'heuristic fertility' into this debate. As C. Goudineau (*AESC*, 1983, p. 970) remarks, 'operation truth', which began within the framework of the *Towns and Countryside* symposium, challenged hasty generalization, 'the rise of new dogmatisms founded on a primary conceptual reflection associated with microscopic and uncertain documentary evidence'. In a procedure which we can describe as *Annaliste*, the contributors took complex phenomena into account.

In this respect too, the pragmatic and at the same time theoretical article by P. Leveau in the *Annales* sounded like a public declaration. The town and country were not the two terms of a binomial expression, where the passive pole attracted the benefits of its active counterpart. The town was not the only element of romanization and was not synonymous with civilization. It was merely the sign of a certain form of civilization. The countryside was no more univocal than the town, since there were several countrysides, just as there were many urban systems. There is no one model to illustrate the way in which economic forms take hold where the town and certain types of rural exploitations are

linked. There could be a concentric ring of *villae* spreading the influence of one dominant economic system, but this system may also rely on road or caravan networks which came into existence before the town, or on indigenous, Roman or romanized markets.

This approach, apparent in recent research in Libya (Barker and Jones, 1982, 1984) and in Tripolitania (Shaw, 1981, 1984) for example, leads us not only to think in more complex terms, but also to analyse the role of the indigenous élites in the processes of romanization. They appear at times to have mastered the new economic forms and social relationships much better than has hitherto been acknowledged. They were able to abandon their temporary alliance with the dominant Roman classes when this alliance became too contradictory to their own systems of organization. Urbanization cannot, therefore, be totally identified with romanization.

Moreover, the village and the *villa* are 'chronologically definable and historically progressive modes of occupying and exploiting the land'. Town and countryside do not stand in opposition to one another, and there is a dialectic link between certain realities developing at different speeds. Of course, the town depended in general terms for its ware and fare and for the erection of its monuments on the current methods of land cultivation, product processing and distribution. But this dependence was not universally total or actual—much depended on the types of town/countryside networks, and the historical period in question.

In this light, the major historic events (*événements*) only represent an acceleration of long-term trends, and neither the Punic Wars nor the crisis of the Gracchi, nor even the Civil Wars, determined the structural evolution of towns and countrysides. Such events were part of far-reaching changes in economic, social and cultural relationships: military/peasant settlers, citizen/non-citizen, state/citizen, ideals about the style of urban/rural life, among others. These relationships, moreover, are not built up and do not disintegrate at the same rate or at the same time. Continuities and discontinuities are not antagonistic concepts.

It is in this sense that the symposium on *The Origin of Wealth Spent in the Ancient Town* suggested a different viewpoint. First of all, P.A. Février rightly declared that the documentation of the historian of antiquity is not 'finite' or limited; it is the mode of questioning which reveals deficiencies. He went on to say that consideration of the links between town and country needed to be global and varied, and should include such things as topography, production, processing, distribution, trade, consumption and taxation.

This meant that the question of expansion of cultivated land was raised, and the effects this could have had on the environment together with its effects on society, or rather, on the whole range of social formations, as this increase in cultivated land had a direct bearing on dominant/dominated relationships. That is why it has been my wish to present in this chapter both the agrarian question and the romanization

question. The agrarian question cannot be reduced to 'events', even though it has to take account of them. To attach too much importance to the concept of crisis would mean that we should find a state of permanent crisis, and, to keep to Braudel's formulation, no longer understand the mechanisms governing change and the critical points at which various series of events interact. The debate on Romanization and resistance illustrates how difficult it is, however, to break with convention and reject oversimplifications, such as that of the antagonism between the victors and the vanquished. The shadow of M. Finley and the spectre of M. Weber have continued to hover over historians of antiquity and haunt their research.

II The romanization of the countryside: Barbarians or Berbers?

Since the period of decolonization, the archaeology of the various Roman provinces of northern Africa has owed much to the *Annales* school, both in theoretical approaches, and in the continued interest shown in the subject.

In 1956, the *Annales* (Despois 1956) raised the issue of the respective contribution of the indigenous population and the Romans to the agrarian techniques of these regions, expressing it in almost the same terms as current research, i.e. the Berbers did not wait for the arrival of the Romans to carry out agricultural terracing and irrigation. First and foremost, mountainous terrain involves a great deal of either individual or collective working over several generations, thus requiring great care, a certain population density and strong social organization.

Three years prior to the paper, Despois had contributed to *L' Hommage à L. Fèbvre* (1953), the 'panorama of living history', a resounding public declaration by the French school of history following the bitter experience of the *Annales* during the Second World War. It is fitting to recall at this point that the *Annales* school, through its commitment to economic and social history, had aimed at providing its readers with 'matter for reflection, and not merely matter for distraction'. Far more importantly, one of its founders, Marc Bloch, who had been persecuted by the Nazis, none the less rejected the offer of Jewish funeral rites (a misguided attempt at racial solidarity) which would have given him the status of a martyr.

In this context, the work of J. Despois, set against its period, that of the end of the Second World War and the wars of decolonization, is viewed as a new way of approaching the much discussed problems of the Maghreb and the respective roles of the settled populations and the nomads in its development, starting with the relationship between the plains and the mountains. He does not deny the crucial part played by certain geographical realities, a factor P. Leveau was to recall some twenty years later in his own research, and stresses the powerful impact

of political authority on the organization of the countryside, with regard to drainage, irrigation and rights of way, especially towards the far side of the mountains of the Moroccan *dir* region. Although in the public imagination the mountains remain the *bled es siba* (remote and backward areas), and a region of dissent, J. Despois subscribes to the Braudelian notion of stronger, more determining contrasts than those between mountains and plains, nomads and settled peoples—namely, the contrast between the Mediterranean and the Atlantic Maghreb, where settled agricultural life was dominant, as opposed to the arid steppes where nomadic existence alone was possible. These modes of existence complemented each other and made the movement of men and goods a lasting phenomenon.

Although the approach is different, this question of nomadism has been touched upon by E. Demougeot (*AESC*, 1960) in his study of the camel in Roman Africa. Finding it hard to believe in the determining role of marked climatic changes during the historical era, Demougeot feels that there is nothing to discount the presence of the camel, the animal of trade, as early as protohistoric times, whilst the horse was the animal of war. At the time of the Roman Conquest, the Numidian camel was used by Caesar's armies and was featured on Republican denarii before appearing in the iconography at Pozzuoli. Throughout the period of the Empire, the camel or dromedary symbolized the African provinces and the trading caravans journeying towards continental Africa or Arabia. Within the confines of the desert, the working dromedary appeared to have been adopted for general use during the late Empire. The camel was not, therefore, introduced by the Severans, nor can it be solely associated with nomadism and the decline of agricultural conditions in northern Africa.

This question brought the internal development of indigenous societies back into its proper focus, as does the study of agricultural terracing, where J. Despois asks whether this was linked to a strong Berber presence. His answer is a positive one: it was at the edge of the Sahara, the least romanized and punicized area, where terracing was known and where a sizeable density of population existed. In the same way, the *gsar*, *guelaa*, *igherm* or *agadir*, which were fortified granaries, apparently corresponded to relatively egalitarian social structures and to an economy which was rather turned in on itself and isolated from trade. From these findings, it follows that it was Rome that admired the 'African hydraulicians' and not the other way round; it was not Rome that brought either terracing or irrigation methods. The question which remained pertained to the origin of this agricultural system, and its antecedents need to be sought in Maghrebian protohistory.

Twenty years later, in the *Annales* of 1978, a series of articles by Y. Thébert, M. Benabou and P. Leveau on romanization and its effects, and on the disputed contrast between the romanized plains and the indigenously-populated mountains, opened the way for a discussion of

more systematic work, either completed or in the process of being completed at the time (Benabou, 1975; Leveau, 1984). The contribution of the *Annales* school and archaeology is considerable in this type of research. The historian of antiquity is present in the field, excavating and sifting, examining specimens in all their complexity, respecting diversity in the continual debate between theorizing and the corpus of material data.

The debate on the thesis of resistance to romanization has highlighted the importance of these procedures. Y. Thébert acknowledges M. Benabou's merit in 'having introduced an underestimated mass of information into the ensemble of historical problems', and enlarged the historian's documentation by focusing great attention on archaeology. His critique was that the concept of the indigenous populations remained vague and contrived to conceal African complexity, retaining an ethnic or racial and bipolar perception of history, with Berbers and invaders, settled populations and nomads, towns and rural areas, central government and local government in continual opposition to each other. Thébert is not sure that Rome really wanted to achieve romanization, even though it was conscious that it was spreading civilization. He therefore doubts whether indigenous peoples would have had a conscious feeling of resistance to this civilization, and whether, therefore, they would have reacted antagonistically towards Romanization. He suggests that the process of development within the tribes before the Conquest and the attitude of certain élites at the time of the Conquest itself should be studied. He stresses the class nature of the process of romanization on the scale of the Mediterranean World, its heterogeneity and its adaptability to the peoples of the Mediterranean basin, where classes and not ethnic groups were the determining factors. Thébert ponders over the question whether Rome was responsible for the transition, in North Africa, from Asiatic to slave-based production methods, but believes that the latter concept cannot be applied to Africa (*AESC*, 1978, p.82, n 29). He categorically rejects the notion of a colonial society, maintaining that there was no political or economic domination by any foreign group, supported by absolute technical supremacy: there was an association of local élites who both passed on and participated in central government. This last contention may cause some surprise, as a distinctive feature of colonial societies is precisely the use of local ruling classes as intermediaries for their power, and Rome, from her conquest of Italy up to her conquest of the Mediterranean basin, did not fail to make use of this form of penetration.

M. Benabou and P. Leveau have replied to these arguments with a quite similar approach to each other. They clearly contrast the *Annaliste* method of doubts, subtleties and contradictory explanations with dogmatism in any shape or form. M. Benabou stresses military resistance (a factor which his opponent did not consider), the reality during the era of

antiquity of the concept of the Berber, and the link between a continuity of culture and religion and variations in its direction. The Roman Conquest, the numerous political forms it assumed, and the quantitative and qualitative benefits which followed cannot be denied in order to favour a view of history which accords greater importance to class contradictions within each social formation. In P. Leveau's work there is abundant reference to the valid meaning of such apposite expressions as 'Berbers, indigenous populations, North Africans'. He even believes that cultural solidarity could have transcended class phenomena. This is a particularly important observation, as it poses the problem of the underestimation of nationalism, especially during the period of decolonization, by the Marxist school. P. Leveau stresses the colonial context and the dominant/dominated relationship in Roman Africa.

In the light of this debate, issues have emerged which are fundamental to this edited volume, namely problems of methodology and the use of various approaches for which predetermined conceptual models prove inadequate, and at the same time the need to take into full account the *longue durée* and the *événement*, the structures of social formations and classes and the individual, quantitative and qualitative factors. It is therefore no surprise to see P. Leveau stressing the same concerns in *AESC* on the two subjects of romanization and agrarian structures, showing the indissoluble link between the analysis of the forms and modes of land occupation and the mechanisms of the Conquest (*AESC* 1983, 1986). They are also present in the *Annales de Géographie* of 1977, a journal which together with *L' Année sociologique* and the *Revue de synthèse historique* was, at the beginning of this century, the model for the *Annals of Social and Economic History*, as highlighted by J. Bintliff in his introduction.

For the purpose of our debate, the preceding discussion has shown us what could be learned from Leveau's 1983 article concerning the ancient town and its rural setting. Leveau's article which appeared in the *Annales* of 1986 was, once again, concerned with romanization. More precisely, it strongly maintained that the study of land occupation, geosystems and social systems was indivisible and formed one of the documentary bases of the analysis of romanization. This meant negating a fragmented version of history and perhaps putting the historical school back on its feet, by placing historical action in centre stage, and was an implicit or explicit criticism of the 'new history' at the very moment when, as J. Bintliff states, it was clearly blossoming, especially within the English-speaking world (Dosse 1987, 1988).

A fundamental characteristic of the *Annales* school has been to state the necessity of inter and transdisciplinarity and to draw attention to structures and systems, discounting the way in which traditional historical methods had relied upon the role of heroes and kings, that is, to turn the spotlight away from 'a biographical and psychological approach' and towards the 'coarse peasant masses'. This was in agreement with the

pioneer approaches of L. Febvre and M. Bloch, and it is no coincidence that a Festschrift devoted to Fernand Braudel was entitled *Espace et Histoire, hommage à Fernand Braudel.*

In his internal challenge to the *Annales* school in the form of self-criticism, what was P. Leveau's contribution to the history of romanization, as briefly outlined above? It reminded us that Despois' presentation of the role of the environment in the form of a contrast between the plains and the mountains, and settled and nomadic existence, was no longer applicable. The political form (i.e. Roman occupation), cannot be seen as being identical to its geographical counterpart (i.e. land occupation). Rome colonized some mountainous areas and neglected some plains. Current studies of land occupation on the Italian peninsula only serve to support these assertions, for the mountainous regions and their farms were not privileged areas of occupation for the Samnites or any similar pre-Roman people, high-altitude farms were not small forts, and the mountains remained an entirely Roman preoccupation, according to the *gromatici* texts of the late Empire (Vallat 1983, 1987).

As for nomadism, Leveau asserted that it was impossible to accept the image of a Roman *limes* (military frontier) solely intended to contain tribes 'compelled by nature to mobility' and committed to a jealous defence of their independence. With regard to the concept of bedouinization, Leveau recalled the anthropological approach to settled and nomadic lifestyles, which does not see them as succeeding each other chronologically. Hence, the picture of the famished tribes of the early Roman Empire seeking to force the *limes* was fading, and contact between the Roman world and its margins was coming more sharply into focus. This altered the total perspective on topography, studies on the occupation of land and, consequently, on the continuity of Berber civilization, an idea which had acted as a vehicle for a view favouring a reduction in the importance of the impact of the Roman Conquest.

Such a new approach, which led to a recognition of certain unavoidable political realities, within a school which professes to take an extremely anti-determinist stance, was just as difficult to absorb as the movement which was to make F. Braudel, himself the structuralist of the wide open spaces of the Mediterranean and little inclined to factual debate, the eulogist of *L' Identité de la France.* Was this merely a drifting off course, as Y. Thébert and F. Dosse more or less feared, or was it a marked neo-positivist trend, as condemned by G.G. Diliguenskij?

The combination of the geographer and the historian occupies a critical position in this scenario: P. Leveau was closely linked to geography and his researches on ecosystems are not concessions to some vague fashion. Contrary to certain trends within the British, or even the French, schools of archaeology, his research tends to support the fact that, around the fringes of the Mediterranean, the environment was particularly fragile, the countryside was extremely 'mobile' and human intervention had effects which were just as significant as those of

morphogenetic evolution. This resulted in micro-evolutions rather than noticeable changes in climatic conditions, erosion of mountain-sides rather than great hydrological changes, gradual changes in vegetation rather than desertification. These were trends which lasted over a long period and were intensified by events or brief cycles, some of which were military and some, at certain times, agricultural, but almost invariably linked to human action.

The complexity of this approach to the history of romanization, the study of the processes of settlement, nomadism and land occupation, and indeed of systems of land occupation, is unquestionably a result of the methodology of the *Annales* school. Its influence on the *Unesco Libyan Project* and G. Barker's research in Italy, also seems to me to be indisputable. The necessity for interdisciplinary research into a variety of areas, the understanding of the internal workings of changing social groupings on the fringes of the dominant Roman system are the most innovative contributions to what can rightly be described, with some admiration, as the English *Annaliste* and structural school of history and archaeology (Vallat, 1990). It should probably be admitted that archaeology occupies a noticeably more important place than history within this school. None the less, I believe that the French school's thoughts on societies of orders and classes should remain dominant in the sphere of higher synthesis, providing a suitable vehicle for the ultimate explication of the combined work on land occupation and romanization mentioned above.

III Economy, society, institutions and politics: full stops and commas

Implementing the wishes of Rostovtzeff, Finley, Frederiksen or De Martino, i.e. to write an economic history of the Roman era based, in the main, upon archaeology, may now seem all the more feasible since, for the last ten years at least, the accumulated data, and the procedures by which they are processed, have greatly improved, allowing increasingly for a quantitative and qualitative approach. One is struck by the amount of attention focused upon the trade and production of ceramics, palynology and osteology, to name but a few examples. In line with the Braudelian model and the hopes of the *Annales* school, studies of material civilization, the *instrumentum domesticum* (craft production) and nutritional and behavioural habits are opening up whole new areas to the historian of antiquity. It would not be irrelevant to point out that the *Annales* was the journal in which historians with a particular interest in archaeological findings have taken up issues such as the relationship between politics, economy and society, the true nature of Roman society (a society of orders, or of classes?), and consequently the autonomy (or non-autonomy) of the legal, institutional, economic and social spheres.

In 1977 this was how these questions were presented to the readers of

the *Annales*. We are asked to consider the Roman township, inegalitarian with regard to property ownership among groups and individuals, both in fact and in law, but intrinsically egalitarian by virtue of the contrast between equal citizens and non-citizens.

What was the economic basis of this rigid social stratification? C. Nicolet analyses a long period during which the prices of essential commodities varied little or not at all, and then examines a short period of great fluctuation in the prices of luxury, semi-luxury and artisanal goods. As well as revealing great disparities in income and fortune right at the very heart of the ruling groups, Nicolet's findings in particular suggest the almost completely erroneous nature of distinctions between senators and equestrians on the grounds that they possessed two different types of fortune (landed and non-landed but pre-capitalist respectively).

The statement that land is the essence of all kinds of patrimony, focuses our attention on the link we have chosen to establish in this chapter between the agrarian question, romanization, and the society of orders or of classes (Nicolet 1977, Andreau 1977). That the *Annales* school should display interest in all three of these subjects testifies to the fact that although different, the areas of inquiry are nevertheless indissolubly linked. Whenever progress is made on one of these topics a reappraisal of the other two must always follow.

C. Nicolet notes the existence of a landowning class living off its property income without seeking to dismantle public authority, a situation clearly distinguishing the social formations of Ancient Rome from those of the feudal Middle Ages. In this system, war and conquest are held to have given rise to a colonial state, which, devoid of any economic rationale, proceeded to expropriate the conquered people of all their land. Concentration of land developed mainly to the detriment of the public domain, as land was a function of the position of those taking possession of it within the structures of power.

However, during this long period there were a number of trends due to the development of consumer markets, and to the attractive profits which could be earned from specialized, income-based crop-growing. Quantum shifts in the size of properties seem to arise more as a result of power and violence than from economic evolution proper. Here we find the basis for an analysis of economic and social life in Rome largely opposed to that stressing 'the dominant role of the relations of production over the productive forces and the labour process . . . in the structural determination of social classes' (Poulantzas 1974, p. 24): a colonial society, the power of hierarchy and of laws, and the essential influence of power in the creation of wealth. Whilst contradictions within the ruling group may exist, this in no way signifies that the occasional conflict between tax-collector and magistrate or Roman and romanized élites is in fact a class conflict.

In the few pages that he devotes to an analysis of C. Nicolet's book *Le*

métier de citoyen, J. Andreau (1977) illustrates the method the *Annales* school adopts for Roman history, in which emphasis is laid on the fringes of the system, details, anomalies, a refusal to give one source precedence over another, and the approval of what Braudel called *bricolage*. He stresses Nicolet's sociological and structural vision of Roman economy and society, mixing what is the same with what is different, dominant forces and continuities, breaks and trends, what was stable and what was unstable.

Three years later, the *Annales* instigated a debate which opened up that of 1977, and significantly brought together the protagonists of the two themes whose importance we have tried to stress in this chapter; namely those of romanization, and a society of orders and/or classes (Nicolet, Thébert, Andreau, *AESC*, 1980). Y. Thébert stresses what he considers to be a British notion, which he suggests is also held by the primitivist C. Nicolet: a conception of the ancient economy in which the 'deep, underlying shifts are overlooked' and 'the dominance of agriculture leaves only limited space for other sectors'. He contrasts this with the model held by the Italian school, which is more sensitive to the specific contribution of archaeology and regards the ancient economy as being resolutely 'modern' in many sectors, presenting intensified development of the craft and commercial sectors, running parallel to a massive orientation of agriculture towards income-based commercialized activity.

If we confine our attention to the interest shown in archaeology by one or the other school, which is all we have done in our present discussion, then it is clear that the conflict of views is exaggerated. Indeed, C. Nicolet bases his argument largely on F. Coarelli's study of investment by *aediles*, P. Manacorda's analysis of the relationship between landownership and the wine trade, E. Gabba's and M. Frederiksen's work on the system of working the land, the development of the *occupatio* (occupation of land without full rights) and the *ager publicus*, and the research synthesized in J.D. Arms' and E.C. Kopff's work on trade and commerce.

So it now becomes clear that the debate remains open-ended and we are still a long way from finding a balance between Marxism and Weberism. At one point it was possible to believe that the evidence provided by material culture was totally value-free, but sadly, even the most subtle of Harris' matrices appears to be bereft of all objectivity (Harris, 1979). In this sense, as Nicolet reminds us, not only is any synthesis quite premature, but moreover, since the classification of the findings of archaeology and historical study into institutions, human behaviour and *mentalité*, banking and commercial networks (to name but a few) are not based on comparable methods, it would be difficult and risky to attempt one. In the debate between Thébert and Nicolet, is there anything to shape the perspective I had hoped to give towards our

collective undertaking in this volume, enabling us to assess the contribution of the *Annales* school to the study of Roman history?

A number of important points do emerge:

(1) From an economic standpoint, the senatorial and equestrian orders are indistinguishable: 'The senators were no more landed than the equestrians'. For topics related to the occupation of land, conquest, and the impact of a market economy upon the development of indigenous social formations, a statement such as this has many consequences. Which order played a decisive role in each of these processes? Can an equestrian order which resorted to a certain type of utilization of conquered land (cattle-rearing, Sicilian corn) be differentiated from a senatorial order choosing to put its land investments to other uses (slave-run villas, specialized crops)? Were there really, if not consciously, two strategies of conquest, one military and the other economic? Do certain kinds of behaviour tend to be linked to one order rather than to another, or should we be talking more in terms of strategies peculiar to particular families, or even individuals? On the other hand, is there a global definition of the dominant and/or ruling class in its many and sometimes contradictory forms?

When examining the question of Sestius amphorae, C. Nicolet remains extremely sceptical about the interpretation of the different amphora stamps, and even more sceptical of the recognition of a 'landowners—commercial agriculture—long-distance commercialization' model. This is not a standard model applicable to all large property-owners, and probably even less to all senators. Can one state at the same time, however, that the senatorial and equestrian were 'two orders' and not 'two classes', and then say that 'the economic and social infra-structure of both orders or of both groups' set them further and further apart after the third century BC (Nicolet *AESC* 1980, p. 878)? Is this not admitting that there was structural determination of two class fractions within the dominant class itself?

(2) In order to bring about behavioural changes, laws will not suffice and can lead to such contradictions within the apparatus of the state that their effects become negligible. Was this not the case with the *Lex Claudia* of 218 BC, which was supposed to exclude the equestrian order from the Senate by forbidding members of the senatorial order to practise certain commercial activities? Had such a law not fallen into disuse by the end of the Republic? Surely it was the immoral amassing of wealth that was being condemned, rather than the amassing of wealth *per se*. When Petronius is seen to mock his hero's rapid acquisition of wealth and his subsequent equally rapid ruin, is this not a clear reaction to the application of the *Lex Claudia*? Whilst a member of the landed gentry—a senator or an equestrian—is sup-

posed, in this tale, like Pliny, to have got into debt so as to go into business, a freed slave like Trimalchio sells his inherited land, invests all his money in the one speculative venture, loses, and then wins everything back (Veyne, 1961). This is a case of what we should call structural determination by the political sphere, according to the legal position of each order relative to the apparatus of the state, and not relative to the production process.

In the debate with Nicolet and Thébert, Andreau returns to some of these problems. He stresses that in Thébert's theory, agriculture is too neatly separated from other spheres of the economy, and that the social groups which then arise are too isolated, 'relegating agriculture to a certain degree of archaism, whilst the other sectors are stamped with the seal of modernity'. However, the commercial or financial sphere is not specific to any particular order in the Roman world, so it is not even necessary to claim, as some have done, that senators went about these activities 'in secret'. The inevitable conclusion is that there appears to be no valid distinction to be made between two classes, one depending on land income for its livelihood, and the other on mercantile and financial capital.

However, Thébert's analysis distinguishes within the ruling class between groupings or class fractions with diverse, and sometimes divergent interests. This type of analysis is questioned from P. Veyne to C. Nicolet and J. Andreau on the basis of their preference for a rather unitary portrayal of the ruling oligarchy. The latter do not, of course, refute the definition of social classes in itself, but they do appear to feel that research into class determination, positions relative to the centre of state apparatus, and the processes of social reproduction is as yet insufficiently established for them to agree to use such conceptual tools to draw 'hasty conclusions'.

It should, perhaps, be modestly conceded that it is difficult to comprehend such aspects, given our present stage of documentation. This is despite the progress made both by archaeology, in the understanding of the structures of landownership, mass production and the circulation of goods, and by epigraphy and numismatics in the perception of social mobility, the formation of municipal 'bourgeoisies' (M. Cébeillac (Ed.), Paris 1983), and the position of the monetary economy within the Roman economy (Crawford, 1985).

To help broaden our knowledge of these areas, and to deepen the debate on behaviour, the *Annales* conducts inquiries, as it did with two series of articles in 1987. The articles are, first, those concerning the study of agrarian structures in Italy and South-West Gaul (Vallat; Fiches *AESC*, 1987), and second, those devoted to family life, matrimonial strategy, and access to the Senate (Corbier, Benabou, and Jacques, *AESC*, 1987). As was the case when Thébert participated in both the romanization and society of orders/classes debates, here

too Benabou is active in both questions. This demonstrates the reality of the links between the different types of perspective required by an approach to Roman society or economy. In fact, by allotting more and more space to debates relating to ancient history and even papers on material culture, features of everyday life, commerce and urbanization, the *Annales* was giving the study of ancient societies an importance to which hitherto only sporadic attention had been paid. What the *Annales* was stating was that economic and social history cannot just be divided up into neat compartments, and that we cannot say that certain concepts are applicable solely to contemporary, medieval or modern history any more than we can say that full stops and commas by themselves provide an adequate account of economies, societies and civilizations.

This brief examination of some of the ways in which the *Annales* school has contributed to research into Roman history, and of the all-embracing interest it has shown in material structures, ideology, and politics, with particular focus on archaeology, should not disguise the differences of opinion and sometimes even opposing views held by the researchers who write in this prestigious journal. Recently F. Dosse in *L' Histoire en miettes* (Paris 1987) clearly outlined the problem of these differences and divergences of opinion within the Ecole, fearing that the fashionable 'new history' trend might produce a new version of history itself. These differences of opinion and lack of coherence stem largely from the fact that it was other historians who coined the term 'school' to define something which has never tried to define itself in such a way, except perhaps in so far as it recognizes the vague feeling that it favours a certain methodology. This does not emerge in any formal way, but it is worked into the phrases that occur over and over again in all the articles that we have been examining: to determine to what extent things are 'necessary' and 'contingent' in history, to refuse to 'spread certitudes', to try to bring together permanent vectors and intermediate oscillations with the shock of actual events. Perhaps the most difficult thing to grasp is that cycles of varying frequency intersect at different points, that all-embracing systems, in the same way as cellular or fragmentary systems, do not evolve at the same rate, and in particular that their respective and partly interdependent evolutions also enjoy a considerable degree of autonomy.

It is as a result of these concepts that the *Annales* school studies social formations with regard, both to their specificity, and their interdependence. It is a specificity which expresses autonomy, the laws peculiar to each formation, internal kinetics rather than inertia. The interdependence highlights relationships and the acceleration of processes due to external agents—a kind of induced cybernetics. The favoured areas of application of these concepts are land occupation, appropriation and

expropriation, romanization and the conquering/indigenous society pairing, ancient society and economy, and definitions linked to the notion of order and class. Did not C. Lévi-Strauss say (*AESC*, 1960) how much the methods of conjectural history had been enhanced by stratigraphic excavations, palynological analysis and interdisciplinary collaboration between archaeologists, ethnologists, sociologists and prehistorians? By emphasizing the need to identify and establish inventories of specific types, to analyse constituent parts, and to establish correlations before being able to discourse upon the totality of the social domain, did he not prepare the way for theories on *amphorae*, ceramics, farms and *villae*?

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5 Archaeology, the *longue durée* and the limits of the Roman Empire

Rick Jones

Introduction

The Roman Empire united under a single political authority a vast area of Europe, Asia Minor and North Africa, today arranged in more than thirty sovereign states, divided among capitalist and communist, Greek and Turk, Israeli and Arab. From the Atlantic to Mesopotamia and the Sahara to the North Sea, the Empire also meant some degree of cultural homogeneity for at least four centuries. One way of trying to understand the dynamics of this phenomenon is to examine the manner of Rome's expansion through military conquest and the assimilation of new provinces. Crucial to that is the question of why the expansion of the Empire stopped where and when it did. It has direct relevance to the more familiar issue of the decline and fall of the Empire. The problem of the limits of the Roman Empire therefore provides a useful case study for the application of the three planes of historical time put forward by Fernand Braudel. It allows the interplay of fundamental structures, middle level systems and immediate events to be examined in the context of a major crux in European history, and one in which the Mediterranean that was so important for Braudel himself played an important part.

The issue of the frontiers of the Roman Empire can be crystallized into an even more specific form, a tale of two forts. Newstead and Binchester are both frontier forts in the north of Britain where I have conducted excavations (Figures 1–3). Both were founded about AD 80 as large cavalry forts, about four hectares (ten acres) or more in size. Both were

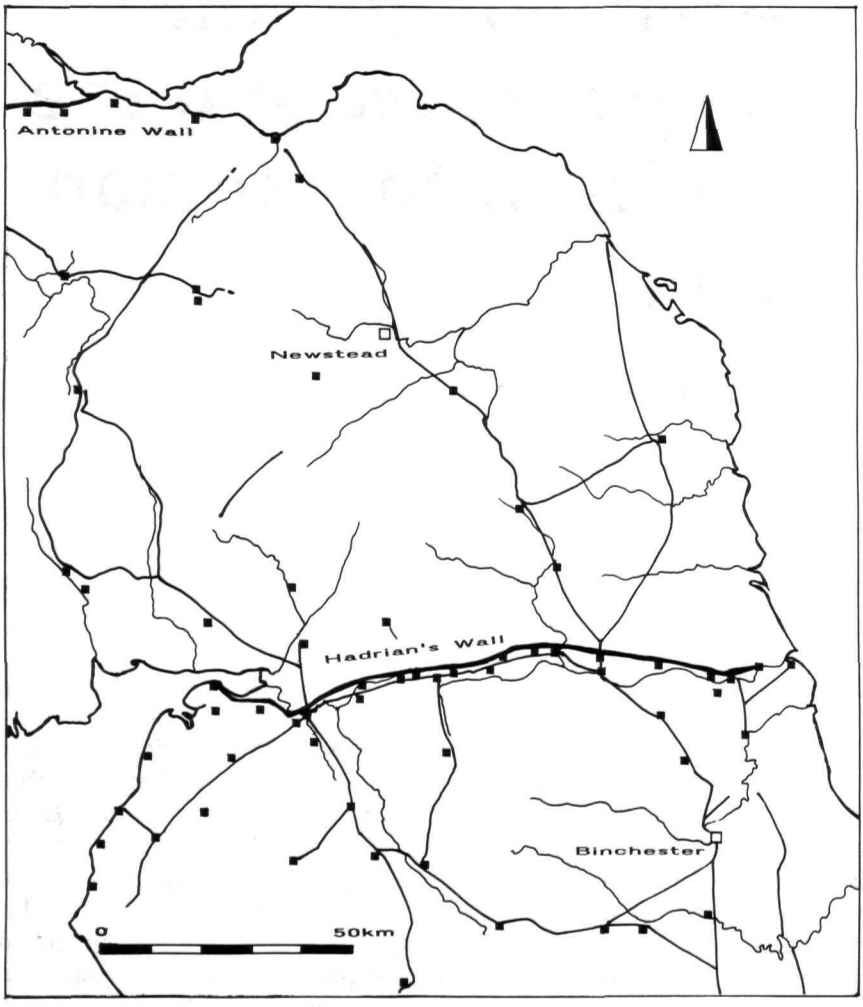


Figure 5.1 The Roman frontier zone in Britain, showing Roman roads and major Roman forts.

situated in good arable land at important river crossings and road junctions. Each lay thirty to forty miles in the rear of linear frontier barriers built in the second century AD: Binchester behind Hadrian's Wall, Newstead behind the Antonine Wall. Like all Roman forts, both sites also formed part of the overall Roman army system, with its discipline, ceremonial and pay, and institutionalized mechanisms for providing staples such as cereals and iron. The sites differ however in their occupation history. Binchester continued as a major centre for some 350 years to the end of the Roman period, developing into a large nucleated settlement, whereas Newstead was effectively abandoned by

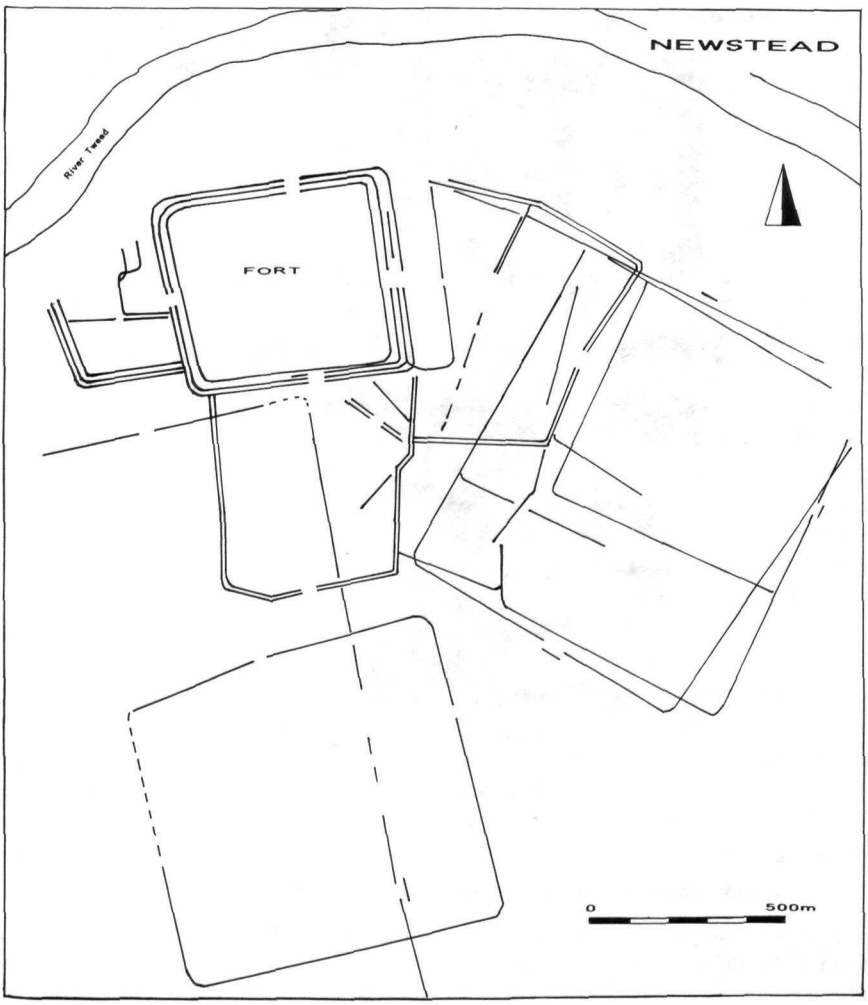


Figure 5.2 The complex of Roman military sites at Newstead, based upon evidence from air photographs and excavation. The permanent fort is surrounded by ditched annexes and at least five temporary camps.

the end of the second century, after having been actually occupied probably for no more than sixty years (Binchester: Ferris and Jones 1980, 1990; Newstead: Curle 1911; Richmond 1952).

The contrast between the development of the two sites arose simply from the fact that from the late second century Newstead lay outside the Roman province of Britain. The northern limit of the Roman Empire in Britain came to rest effectively at Hadrian's Wall, leaving one site within the province and one outside. Any explanation for the end to Roman expansion has to deal with the histories of Newstead and Binchester.

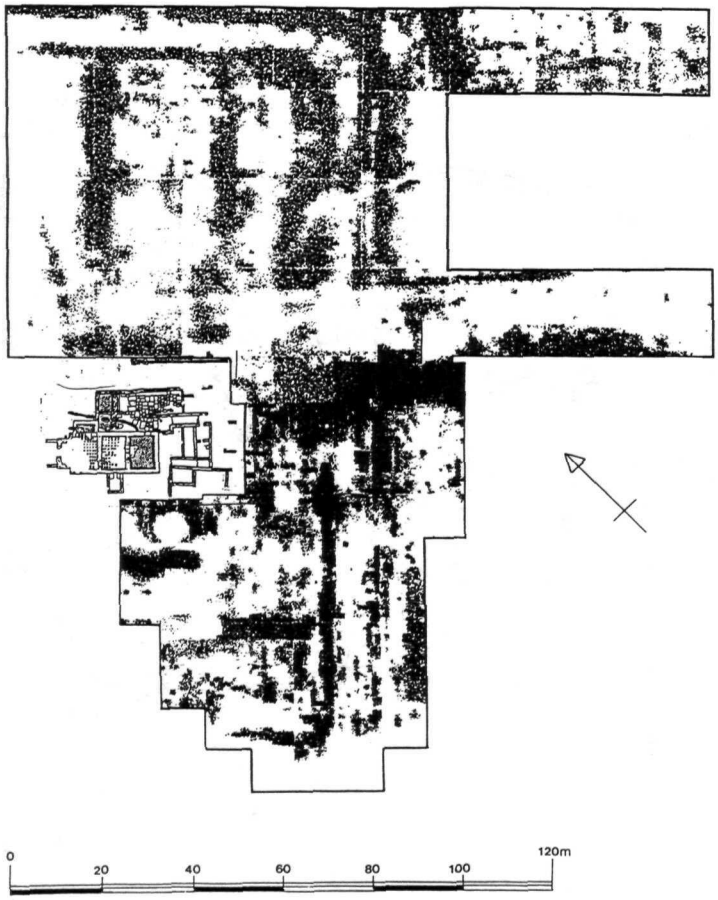


Figure 5.3 The plan of Binchester Roman fort revealed by resistivity survey. The black dots represent areas of high resistance, here mostly internal streets, as well as the walls of the fort defences and the internal buildings. The curved corners of the fort are seen at the top and bottom of the plan. (Survey by Mark Gillings and Steve Dockrill.)

To understand the problem it is necessary to understand the interrelationships of the decisions and actions of Roman emperors and generals, the political, social and economic structures operating in the frontier zones, and the background of the natural environment. Thus Braudel's formulation of the three planes of historical time can be applied to this question, as an example of *histoire problème*, illuminating wider issues from a specific case. The twin threads to this paper are therefore the usefulness of Braudel's three planes for explaining what happened on the Roman frontier and the nature of archaeology's contribution to the problem as conceived within the Braudelian framework.

Events

The study of Roman frontiers has been largely obsessed with the identification of events. The mentions that survive in the historical sources of campaigns by particular commanders have led to a continuing fascination for locating the military bases used in specific years—the syndrome of 'Hadrian slept here'. What happened on Roman frontiers clearly was influenced by a series of personal decisions: those of the local commander on the ground, those of a general's campaign strategy, and the overall aim of imperial policy. What happened was also affected by the actual outcome of specific unpredictable events, notably battles. These factors cannot be ignored, and sometimes we have substantial information on them in the historical sources. They have produced the strong emphasis in the study of Roman frontiers on military history and the implications of frontiers for the history of the emperors themselves. That emphasis must be questioned on two grounds: whether the framework of the narrative of events offers an adequate and meaningful approach to the phenomenon of frontiers, and whether our archaeological data can provide the sources to reconstruct such a narrative.

In the cases of Newstead and Binchester our use of historically attested events to explain the pattern of occupation is very restricted. The foundation and abandonment of forts were no doubt military decisions made in response to particular policies and events. It seems reasonable to think that neither Newstead nor Binchester were established as Roman forts until the region as a whole had been taken over by the Roman army. The military occupation of northern Britain is conventionally attributed to the Roman governor Agricola, in campaigns beginning in AD 79 (Hanson 1987). However, the link with the historical record has to come from the archaeological evidence actually found at the particular site. The archaeological evidence recovered at both sites, mainly pottery, seems to be consistent with the conventional date, but it remains an assumption to say that Binchester was founded in 79. The actual evidence directly from the site cannot distinguish at best between, say, 75 and 85 or some date in between: more likely is a grosser bracket of a quarter of a century. The assumption of foundation in 79 may be a reasonable one, but I feel more comfortable in saying that the site was first occupied in the late first/early second centuries.

Similarly, much has been written about the date of the Roman withdrawal from Scotland in the late second century, in which the date of the end of occupation of Newstead is seen as an important element (cf. Gillam 1974, Breeze 1982, Hanson and Maxwell 1983). The precise date has importance for the history of imperial frontier policy: under which emperor was the Empire reduced? Yet it is highly questionable whether the answer can ever be found in the pots and buildings we dig up as excavators. The second half of the second century offers a number

of titbits of historical evidence about military activity. Unfortunately there are too many to fit the accepted view of the second century phases of the Antonine Wall and Hadrian's Wall. Therefore those trying to work within that framework have to reject at least one of the historically attested happenings as not having resulted in a major constructional phase or withdrawal (cf. Gillam 1974, Breeze 1982). The problem of making sense of this evidence is essentially an historical one (cf. Mann 1988).

Most work on Roman frontiers has been conceived from the Roman imperial point of view, seeking details of Roman army life from archaeological remains to support the information derived from papyri and inscriptions. The tone of discussion in fifteen Roman Frontier Studies Congresses over forty years is set by an interesting new detail of a fort gate here or new light on a particular campaign there. There has been a remarkable complacency about the ability to pursue campaigns and military history through archaeology, despite the warnings of one of the leading post-war British authorities:

> A weakness of present-day protohistoric archaeology is its tendency to make inferences about military and political events from archaeological evidence, and also to use hypothetical interpretations of written evidence as a basis for dating structures and artefacts. (Gillam 1974, p. 1)

Gillam in the same paper went on brilliantly to expose the way in which those hypothetical interpretations of very fragmentary written sources had been used to define the archaeological record on the northern British frontier. He himself then attempted to produce what he regarded as an improved synthesis of the same mixed historical and archaeological evidence. He failed to think through the implications of his own trenchant statement: archaeological evidence is not suited to the reconstruction of specific events. The use of written and archaeological evidence together requires the most rigorous source criticism of what each kind of evidence can be used to do.

Yet Hartley's (1972) use of the makers' stamps on Samian Ware was received with relief by all those who had previously wrestled inconclusively with the written evidence, and has since provided the orthodoxy for interpreting the period (cf. Frere 1987, pp. 126–53). These potters' stamps are not normal ceramic evidence in that they allow a much more specific attribution of pots to producer than the usual stylistic or compositional methods. They do nevertheless share with the mass of pottery the property that they are not directly dated themselves, only by association with coins or with structures or deposits that form an archaeological horizon that can be dated from an historical event.

We have come back round the circle to Gillam's warnings quoted above! The dating of Samian stamps must still be susceptible to revision in detail, making any attempt to link assemblages with individual years

at best conditional. Given the very questionable assumption that the historical evidence records all the main events of the period, the ceramic material may just possibly permit us to choose the most likely association between an historical event and an archaeological phase, within limits of confidence which ought to be clearly stated. However, for me the intricacies of such exercises are of limited interest. Even in terms of the history of imperial policy, they are of dubious use compared to issues such as whether to have a frontier at all or to pursue an active policy of total conquest (Mann 1974). Studies of the archaeological data are more productively directed towards goals which they are more suited to achieve.

The frontiers of the Roman Empire thus present in a particularly acute form some fundamental problems of working in prehistory, how to relate together written and archaeological sources and how to assess the importance of the specific event. Despite the serious technical objections that individual historical events are at best very difficult to identify in the archaeological record which is our most abundant source of information for Roman frontiers, the more important historical question concerns whether anyway the event is the crucial factor in trying to understand our specific problem of the different histories of Newstead and Binchester. Even if we postulate a military defeat for Rome as the key reason for the withdrawal from Scotland and abandonment of Newstead, a more significant question remains, why Roman forces did not try to regain the territory lost. That requires consideration of broader issues.

Middle-level systems

Here we can start to discuss social, political, military, or economic formations at what has become our most familiar level of analysis. Several interpretations of the dynamics behind Roman frontiers have been put forward in recent years, including imperial grand strategy, natural and social ecology, and core–periphery relationships. In a more comprehensive synthesis which sets Roman military organization at the centre of consideration of the Roman state, Michael Mann defines societies as made up of 'multiple overlapping and intersecting socio-spatial networks of power' (Mann 1986, p. 1). His networks of power are ideological, economic, military and political, corresponding to the middle level of Braudel's scheme, the *conjonctures*. How far can such conceptions be mobilized to explain Newstead and Binchester?

In looking at the north of Britain in the Roman period we have to explain both the success and failure of the Roman Empire: both how it held Binchester and how it lost Newstead. In that sense grand strategy has little to offer since the strategic difference between Hadrian's Wall and the Antonine Wall is negligible. They are variations on the same theme (Luttwak 1976, Mann 1974).

It is more productive to consider what differences can be observed between the regions in question. In this the ecological and core–periphery approaches offer most illumination, incorporating to some extent the concepts of Mann's networks. The idea of the Empire being forced to stop expanding because of limitations of carrying capacity in some form has obvious attractions. It has been expressed cogently by Groenman-van Waateringe (1980, 1983), stressing factors both of social and political organization and of agricultural production. These approaches are closely linked to a centre–periphery model defining a developed imperial centre interacting with an undeveloped periphery, and at the same time development spreading into the peripheral areas (cf. Lattimore 1962, Bloemers 1988). Specific aspects of such relationships have been investigated in terms of patterns of religious and kinship organization (Roymans 1988), changes in social organization in regions beyond the frontier itself (Hedeager 1988, MacInnes 1984), regional settlement systems (Willems 1986), and the impact on economic structures of the arrival of the Roman army (Breeze 1984, Jones forthcoming).

Studies at this level not only operate at scales which permit the recognition of regional differences, they also deal directly with the parameters within which specific events took place and specific decisions were made. Moreover, they can be carried out effectively on the archaeological data. At this level, hypotheses can be constructed and sensibly tested, so that patterns defined in the archaeological record can be related with reasonable confidence to settlement, production and exchange systems.

Structures of the *longue durée*

Clearly Braudel's long-term structures such as topography and communications, land quality and climate provide the environment within which the social and economic systems discussed above actually operated. Braudel's own masterwork on the Mediterranean in the sixteenth century obviously shares with the Roman Empire the same central area, despite the difficulties he found in accommodating the Roman Empire into his framework of interpretation (as noted by Anthony Snodgrass in chapter 3 of this volume; Braudel 1972–3). Much of what Braudel had to say about the character of the Mediterranean holds true for the earlier period. He was able to argue for a Greater Mediterranean extending as far as Moscow: central Scotland and the Antonine Wall need be no more implausible. Yet there is very little difference to be seen in terms of climate, topography and subsistence base between northern England and southern Scotland. The structures of the *longue durée* are too broad, if taken in isolation, to help much in the specific problem posed of why the Roman Empire's limits included Binchester but not Newstead.

However the interrelations of these levels of analysis may more fruitfully be pursued, and can best be appreciated by putting forward a brief outline of an explanation to that problem.

The northern frontier in Britain

The limits of conquest of the Roman Empire were in the end set by a balance between will and resources: the strength of will to continue expanding and the resources needed to perform the task against the obstacles in the way at the time. Success could thus only be achieved where the combination of will and resources was strong enough to overcome opposing will and resources. In measuring will and resources, it is necessary to return to the kind of categories set out by Michael Mann: the ideological/political, the economic and the military.

Traditional Roman frontier studies have provided much information about the nature of Roman military organization in northern Britain as a whole, but less about the kinds of military force employed by their native opponents. There seems to have been no obvious simple military reason why the Roman army could not have returned successfully into Scotland: to some extent they did exactly that under Septimius Severus in the first decade of the third century. However, the occupation was brief and was never attempted again. Yet there is little reason to believe that in purely fighting terms the Roman army had lost its earlier advantages over its opponents. If the Roman army was still able to win battles, the problems have to be sought in the Empire's capacity to hold and integrate new peoples in new territory.

In Scotland in the first and early second centuries, Rome's main strategic difficulty was the lack of a social and economic infrastructure adequate to sustain the army and take responsibility for itself. The pattern of trading contacts with the Roman system prior to conquest had long been established. Caesar in Gaul had taken his armies far beyond the areas of close connection, leading to some slowness in the development of northern Gaul compared with the centre (cf. Roymans 1988). I have argued elsewhere that the conquering Roman army's supply system operated in three stages. At first the army relied upon supplies from outside, taking what it could get locally, especially basic foodstuffs. As surplus production in frontier areas expanded to meet the new demands, the need to move basic commodities over long distances diminished, but the need for specialist goods like weapons and tools remained and had to be met by production organized by the army itself. Only when the production system grew enough to be able to supply all the goods, was the army able to draw on an integrated 'Roman-style' economic system. In northern England that stage does not seem to have been reached before the mid-second century at the earliest (cf. Jones forthcoming).

Rome's rapid campaigns into the north of Scotland in the late first century AD cut deep into areas where no earlier trade connections can be shown through imported Roman goods. The conquest sprang off from an area in the north of England that was still moving from the first to second stage of frontier economic development. Supply lines therefore had to be stretched over very long routes to the south of England and continental Europe. Moreover opponents had not been 'softened-up' by trading contacts with the Roman world. The parallel advances of military conquest and cultural contact were distorted by pushing the military far ahead of the cultural.

The withdrawal to the Tyne–Solway line can therefore be seen as bringing the extent of military control back to match better the background of social and economic development. The choice of the Tyne–Solway line itself for the Hadrianic frontier was a simple matter of topography. If there was to be a linear barrier somewhere in that part of the country, the Tyne–Solway was the only practicable place. To the north the only other possibility was the Clyde–Forth used for the Antonine Wall. Linear barriers across the Lake District or Galloway and the Cheviots would not have been at all feasible. Topography thus played a key part in the actual choice of where specifically a linear barrier should be—and perhaps even the topography of the narrow neck to Britain was an encouragement to the idea of the barrier. It is more interesting to ask why the Romans accepted this limit to their power of conquest. As already noted above, defeat in a particular battle is not an adequate explanation in itself, since the normal response to that was to return with increased strength.

The Tyne–Solway line apparently marked the approximate limits of the lands of the political group known in the written sources as the Brigantes, who had been clients of Rome more or less since the first campaigns in Britain in AD 43. This longer exposure to contacts with Rome nevertheless had not led during the first century to widespread changes either in the native settlement hierarchy or in the level of imported Roman goods. The appearance of one major centre at Stanwick (North Yorkshire) which appears to have been a site very similar to southern British *oppida* and to have received quantities of imports from the south in the middle of the first century cannot be taken as evidence for general urbanization (cf. Haselgrove 1984, Jones 1984). The settlement hierarchy that did develop in the north of England in the Roman period seems to have been a simple one, with little more than native farmsteads and Roman forts or towns which grew from military origins (cf. Higham 1986). The two sites in the frontier zone proper which are normally accepted as genuine towns, Carlisle and Corbridge, both had close links with the Roman army. The forts themselves were nucleated settlements, to many of which, if not quite all, some kind of civilian settlement attached itself (Sommer 1984).

However, a distinction must be made between those with a few

civilian buildings and those where a large settlement grew up. The small settlement of only a handful of houses presumably depended directly upon the soldiers of the fort, providing shops, taverns, and homes for their families. The large settlements reached a scale that exceeded those limited functions. The Binchester settlement for example spread over some 16 ha (forty acres). In places at least there were three distinct superimposed phases of stone building. Several other comparable settlements can now be recognized at other forts in north-eastern England (Jones 1984). Their size alone argues that such sites acted as central places for the areas surrounding them. Although we have very little evidence for Roman period rural settlement in north-east England, the clear implication of the forts and civilian settlements is that the overall settlement system had grown significantly more complex, and urbanized to a substantial degree.

These changes happened during the third and fourth centuries. They were matched by developments further north. In south-eastern Scotland the hill-fort of Traprain Law has produced a rich collection of Roman imports, far exceeding that from any other native site in Scotland. It seems to have become the key political centre in the region (Jobey 1976; cf. Hill 1987). Below it in its settlement hierarchy were brochs and crannogs, which were architecturally more complex than simple farmsteads and which have also produced more Roman imports (MacInnes 1984).

The third and fourth centuries thus show the growth of at least a partially urbanized settlement system within the Roman province and continuing contact into the territory beyond, where there were also changes in the settlement system that suggest more complex sociopolitical differentiation. The conditions by then had certainly become much more appropriate for renewed conquests into Scotland. That they did not happen seems to owe more to the lack of appetite for conquest now found in the imperial government, rather than to the conditions actually prevailing in the frontier zone itself.

The abandonment of Newstead and the notion of conquering Scotland thus came about because the problems of conquest exceeded the will and resources available to Rome. Part of those problems lay in the geography of northern Britain. Moving north towards the Highlands of Scotland the proportion of good farming land to poor quality uplands decreases. The Highlands also provided more places of refuge for opposing forces, at least at bandit level. Distance from the Mediterranean was hardly a significant factor here. More satisfactory explanations come from the interplay of economic and military systems with ideological factors. While the urge for conquest was strong, the resources required to hold Scotland were inadequate because of the lack of supporting development in northern England. By the time the north of England had become developed, the urge to new conquests had faded.

Conclusions

The Braudelian model of the past provides a useful guide for conceptualizing historical problems. It allows us to determine clearly at what level of generalization we are working. The level of structures of the long-term deals with factors that are almost timeless, or at least change at a very slow pace. Part of the attraction of Braudel's *The Mediterranean in the Age of Philip II* (1972–3) lies in its general application to the Mediterranean in all periods. Concern with such factors of the *longue durée* inevitably tends to emphasize those things that stay the same over long periods, like mountain ranges and rivers. If we are concerned with areas of human history where rapid change can be discerned, the utility of those structures is inevitably limited. Braudel's own view of underlying formations inevitably draws attention towards things away from people, to nature and away from culture (Clark 1985, p. 190). Stressing the factors which limit human actions inevitably tends towards a deterministic view of the past. However, for the problem of the Roman Empire determinism is not enough. The parameters of geography, climate and agrarian subsistence changed little in north-western Europe between 100 BC and 500 AD, while the Roman Empire came and went. Difficulties of communication obviously became greater the bigger the Empire grew, as was recognized by Lattimore (1962, pp. 487–8). However the main change at the level of the medium-term structures (*moyenne durée*) at this time seems to have been the marked improvement in overland communications produced by the Roman imperial system itself through the building of roads.

If we wish to examine historical problems at levels such as the dynamics of the Roman Empire, it is necessary to pay most attention to phenomena of the right scale—political, ideological, economic and military formations. If most historians would now accept that it is at least necessary to examine such formations, as they set the framework within which individuals and events operate, most archaeologists would also agree that this is the level where we have most to contribute. In periods like the Roman, we can identify a three-fold split in our use of the archaeological data, just as in Braudel's *durées*. At the simplest level, we can describe and classify the things we can dig up and observe directly, like pots, buildings, seeds and bones. Requiring a higher level of inference is the attempt to form linking patterns between the observed evidence, such as settlement hierarchies or production and exchange systems. Only by making yet higher inferences can we build interpretations of social structures, political legitimation and religious frameworks. The trends we thus define in the archaeological data can be used as the basis for meaningful discussion of the middle-level formations. Setting them at centre stage also has a special attraction for archaeologists who can rarely construct specific events from their evidence.

A clear recognition of the common ground here between those who study the human past can only be helpful, whether we call ourselves historians or archaeologists. There are growing signs of *rapprochement* between the disciplines (Young 1988, Hodder 1986). To history and archaeology can also be added anthropology as all share many common concerns, even if they take different perspectives and use different sources of data (Claessen 1983, Deetz 1988). Such open approaches must lead to special benefits in protohistoric periods such as the Roman. The kind of commonality available can be seen in the *Annaliste* aim of Braudel and his colleagues to broaden historial concerns, clearly expressed by Febvre:

> Words, signs, landscapes, titles, the layout of fields, weeds, eclipses of the moon, bridles, analysis of stones by geologists and of metal swords by chemists, in a word, anything which, belonging to man, depends on man, serves him, expresses him and signifies his presence, activity, tastes and forms of existence. (Febvre 1973, p. 34—as quoted by Clark 1985, p. 189)

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6 *Annalistes*, hermeneutics and positivists: squaring circles or dissolving problems

Christopher S. Peebles

By the way of preface, I must emphasize that I seek to dissolve methodological barriers rather than to erect them. Thus, with the exception of so-called 'behaviourists', this chapter is not to be taken as an attack on any individual or any school of thought in archaeology.[1] Instead it is an argument for the role of history and of representations in the production of prehistory.

Given that caveat, I would like to discuss prehistory as history—as text and context—and thereby outline the first tentative steps in the formation of an alliance between archaeology, history and the emergent cross-disciplinary movement that has been labelled as cognitive sciences. In taking this path I wish to suggest that both writing about the prehistoric past, and writing about the writings about the prehistoric past, are important tasks for archaeologists. In the former case we ask about the knowledge it took to live one's life in a time and place bereft of written records; in the latter case we ask about the writing of prehistories in the here and now—the knowledge we, as prehistorians, deploy on behalf of this unwritten past. In this quest, I shall argue that prehistory must be in some measure both art and science, in which the latter is embedded in the former. It can be more of one than the other, it can choose to be one rather than the other, but it cannot choose to be neither the one nor the other. That is, prehistory cannot choose a hybrid third way, because to do so leads to an uncritical relativism, solipsism, and perhaps nihilism: a trio to be avoided at all costs.

Prehistory is rapidly becoming an intellectual (and anti-intellectual) battleground. Various individuals, groups, and so-called schools—from hyper-nationalists to post-modernists—compete for the right to assert that theirs is the only legitimate interpretation of the past. Leaving aside the 'crazies', who wish to seize the past for various dubious ends (to resurrect Plumb's past, discredited in his *The Death of the Past* (1969), that is dying but, unfortunately, is not completely dead), current battlelines are drawn between two poles, the scientific and the humanistic. On the intellectual side, the clash is between what Jerome Bruner has called the two great modes of cognition, the paradigmatic or logico-scientific and the narrative (Bruner 1986). For Bruner, scientific (empirical) 'truth' is the measure of the former, and verisimilitude or 'lifelikeness' is the measure of the latter.

Given Bruner's characterization of the two modes of human cognition one might ask: Does the narrative mode have a place in the production of scientific history and prehistory? The answer, I believe, is yes. In so far as history and prehistory present temporally ordered descriptions of changes in humankind, society, and culture, then these disciplines are, by definition, involved in the production of narratives. By the same token, it is necessary that these narratives be true to a 'real' past: that they be constructed from logico-scientific operations that are both logically and empirically adequate, that the premisses entail the conclusions, and that the conclusions are true in the light of the evidence.

Historical narratives, complete with the conceptual bases for inclusion and exclusion of events and other evidence, place the narrator in a privileged position. As Danto has observed: 'The historian not only has knowledge they [the participants] lack; it is knowledge they cannot have because the determinate parts of the future are logically concealed' (Danto 1985, p. 350). That is, 'the knowledge available to him [the historian] is logically outside the order of events he describes' (Danto 1985, p. 356). Moreover, historical narratives comprise both ahistorical (atemporal and law-like) components as well as elements that are strictly historical. In this task, Danto has observed: 'There are limits assigned by historical locations in human affairs that have no counterparts in the natural world' (Danto 1989, p. 274). Consequently:

> To be human is to belong to a stage of history and to be defined in terms of the prevailing representations of that period. And the human sciences must, among other things, arrive at historical explanations of historically indexed representations. (Danto 1989, p. 273)

In another, far more destructive and profoundly anti-intellectual sense, the current battles among some archaeologists replay those fought by Enlightenment and counter-Enlightenment scholars over the legitimacy of sociology as a discipline with anything important to teach

about humankind and society. In the case of nineteenth-century sociology, Wolf Lepenies, in his book *Die Drei Kulturen*, observes:

> In essence . . . the battle lines are drawn as follows: sociology is a discipline, characterized by cold rationality, which seeks to comprehend the structures and laws of motion of modern industrial society by means of measurement and computation and in doing so only serves to alienate man more effectively from himself and from the world around him; on the opposite side there stands a literature whose intuition can see farther than the analyses of the sociologists and whose ability to address the heart of man is to be preferred to the products of a discipline that misunderstands itself as a natural science of society. (Lepenies (1985) 1988, p. 13)

As Lepenies points out, when rationality promises too much and cannot deliver, the cult of irrationality prospers. Such was the case among some sociologists and other scholars in late nineteenth and early twentieth-century Europe; such seems to be the case, at least in some quarters, in prehistory today (Shanks and Tilley 1987). Because the 'laws' of cultural development have not been forthcoming in history, anthropology, or prehistory, standards of scientific judgement have been abandoned by some in favour of locating archaeology somewhere between the arts and the sciences, but in the grasp of neither the one nor the other (Hodder 1986).

C.P. Snow, who is responsible for the modern contours of the Two Cultures debate (1959, 1964), and of which Hodder is a latter day participant, attempts to find a middle ground in the Third Culture, which Lepenies characterizes as politically expedient but methodologically unstable (Lepenies 1988, ch. 15). Snow envisions the reconciliation of the Two Cultures in a Third, in the historical dimensions of *les sciences humaines*. He observes:

> All of them are concerned with how human beings are living or have lived—and concerned not in terms of legend but of fact. I am not implying that they agree with each other, but in their approach to cardinal problems—such as the human effects of the scientific revolution, which is the fighting point of this whole affair—they display, at least, a family resemblance. (Snow 1964, p. 70)

He is convinced that the third culture will be realized, and he sees its genesis in the communication between the arts and sciences, but he does not endow it with a standard by which we can judge its products—although perhaps some measure of both truth and verisimilitude would suffice.

Jean-Claude Gardin, like Lepenies, argues that the third mode or Third Culture is by definition unstable. It is neither art nor science, and it is measured neither by truth nor by understanding, but by uncertainty. At its worst, it embraces scepticism and various brands of

relativism (Gardin 1987). Instead, for Gardin, although archaeological (and historical) constructs—compilations and explanations—may be artful, they are none the less to be judged solely by their logico-semantic structure and their correspondence with the 'real' world (Gardin 1980).

The great strength of the *Annales* school, at least as broadly construed, is in its quest to reconcile the paradigmatic (logico-scientific) within the narrative in the study of the past, without either reducing the one to the other or succumbing to unproductive and uncritical relativism and formless scepticism. Coherence, consistency, and lack of contradiction, as well as logical adequacy, are the measures that are used to judge historical concepts and constructs; these constructs, in turn, are assayed against the historical 'record' as tests of their validity and opportunities for their falsification. Again, *Annaliste* goals might be said to comprise both knowledge (for which truth claims can be made) and understanding[2] (which, at least in part, includes lifelikeness), which itself may later be cast in paradigmatic, logico-scientific terms.

If the *Annales* school can be described in broad strokes (see Bintliff, Ch. 1 above; Stoianovitch 1976), it can be characterized by an absence of dogmatism, a certain non-pathological eclecticism, a general commitment to research directed toward the solution of explicit problems, and longstanding efforts to include the methods and products of the social sciences, especially anthropology and economics, as part of historical methods. Moreover, scholars associated with the *Annales* have sought constantly to broaden the field of historical investigation, bringing in the 'people without history' (in the sense meant by Eric Wolf (1982)): the illiterate and the preliterate, the disenfranchised and the distant. These features should have led to an early and easy alliance between the *Annales* brand of history and prehistory. Unfortunately, the *Annales* seem to have made little impact on prehistory, even in France. There have been articles on the archaeology of classical antiquity in their journal, *Annales: Economies, Sociétés, Civilizations*, and one issue (1973, 1) was devoted to archaeology, but otherwise I could not find any articles specifically devoted to topics and problems of prehistory. The three volume synthesis of the *Annales* movement, *Faire de l' Histoire*, edited by Jacques Le Goff and Pierre Nora (1974), contains two articles on archaeology and prehistory. One, by Alain Schnapp (Volume II), 'L'archéologie', is a period piece on new approaches in archaeology: mathematics, computers, research methods associated with the new archaeology. The other, by André Leroi-Gourhan (Volume I), 'Les voies de l'histoire avant l'écriture', is still vital and instructive.

It is curious that Leroi-Gourhan's article was not among those chosen for translation into English (see Le Goff and Nora 1985). Many of the themes he developed in that chapter have resurfaced lately under the banner of 'contextual' or 'post-processual' archaeology. Leroi-Gourhan stressed the point that archaeological sites are not merely to be viewed as calendars for the ordering of isolated finds, but approached as 'texts'

in which the context of the material remains provides the basis for the construction of contemporary meaning:

> Si l'on considère le document préhistorique non plus comme un calendrier mais comme un texte, l'activité essentielle de la recherche n'est plus dans la réflexion interprétative sur des objects dûment récupérés dans leur ordre stratigraphique, mais dans la lecture du document que constitue la surface dévoilée par la fouille, document éphémère, amalgame de poussière, de pierres, de débris d'os dont la valeur fondamentale ne réside que dans les rapports mutuels des éléments qui le composent. (Leroi-Gourhan 1974, p. 140)

He concludes that archaeology has failed to use the scientific apparatus available to it in favour of excavating and reporting sites in the old and underproductive ways (1974, p. 148). In effect, he accuses archaeologists of asking the wrong questions and producing texts that cannot answer the right questions, even if they were asked. He argues that materials gathered in a haphazard manner and from excavations conducted on a vertical face cannot provide the contextual evidence necessary to answer fully questions of technology and economy, let alone those about prehistoric society and beliefs. Leroi-Gourhan stresses that only meticulous, open-area excavations can produce the kind of context (text) necessary to answer questions that go beyond those that can be asked of individual artifacts and assemblages of artifacts.

From the vantage point of North American prehistory, archaeologists have reduced the *Annalistes* to incantations in texts and icons in bibliographies rather than using them as substantive guides to research and exemplars for the writing of prehistories. Thus reconciliation of narrative with science, at least from the *Annales* perspective, has been rendered a sham. Again, from the perspective of North America, prehistory and anthropology, like so many of the other social sciences, have abandoned history as either method or metaphor. In fact, with few exceptions, archaeology has developed a pronounced antipathy to history. American archaeologists have led the search for the nomothetic: the invariant covering laws of human behaviour. In doing so they have eliminated—or at least neutralized—the sentient human and human representation from the prehistoric past.

A search of the *Social Sciences Citation Index* for the years 1982 to 1988 for references in North American journals of archaeology to writings by historians associated with the *Annales* school, especially works by Bloch, Braudel and Le Goff, was generally fruitless. There were few references in the major journals: Mark Leone's citation of Braudel's work in his *American Antiquity* article, 'Some opinions about recovering mind' was one of the very few. Even more surprising was the fact that the two Distinguished Lectures of the Society for American Archaeology for 1987, which were given under the general title 'History and Archaeological Theory' (Deetz 1988, Young 1988), did not make reference to a single

historian associated with the *Annales* school. Instead, exemplars were provided by the works of R.G. Collingwood, E.H. Carr, and Carl Becker. In lesser journals and monographs there have been occasional citations from the *Annales* historians—seemingly for effect rather than for inspiration.[3]

It is important to remove the stigma applied to history by some archaeologists; to eradicate the claim that it and its products are solely ideographic, particularistic, and empathic in structure and content. This assertion is certainly false, and serves only to distance archaeology from historical methods and practice. Bruce Trigger characterizes the roots of this antipathy as based on a false dichotomy

> that was introduced into American archaeology by Kluckhohn and Steward, and reinforced by the adoption of neo-evolutionism. The latter encouraged prehistoric archaeologists to believe that human behaviour and cultural change exhibited strong regularities that could be accounted for in terms of evolutionary generalizations, and that doing this constituted scientific explanation. This left history as a humanistic residual, to account for the 'unique, exotic, and non-recurrent particulars' of cultural change, all matters that neo-evolutionists judged to be of little, if any, scientific importance. (Trigger 1989, p. 373)

The patterns among these 'particulars', however, turn out to have substantial scientific and historical value. Consequently, it is important to reintroduce the concept and methods of history into prehistory. This suggestion does not imply that science and scientific methods should be eliminated from the intellectual armoury of prehistory. Instead, the scientific and the humanistic should be made to work together, towards knowledge and understanding of humankind and cultural variety. In Bruner's terms, the paradigmatic should be set within the narrative, as long as the result is the production of knowledge and not the kind of understanding gained from fiction. In brief, both scientific truth and verisimilitude are crucial to the construction of history, although the critical judgement appropriate to the former is perhaps more important than the aesthetic considerations of the latter.

Yet at this point, there is a major question: What then does separate history from fiction? Both employ narrative; both exploit verisimilitude. One answer is provided by Paul Ricoeur in his Aquinas Lecture, 'The Reality of the Historical Past': 'the historian is constrained by what once was. He owes a debt to the past, a debt of gratitude with respect to the dead, which makes him an insolvent debtor'. That is, the constructs of the historian (and the prehistorian), be they either hypothetico-deductive constructs or historical narratives—or some combination of both—must be assayed against the remains of a 'real' past—remains, which, as Collingwood (1939) pointed out more than fifty years ago, are here with us in the present. In brief, both analytic prowess and rhetorical skill are important in the writing of prehistory. Hayden White's

(1973) 'trophics of history' are every bit as important as trophic levels in human ecology.

It is perhaps ironic that the 'project' of the *Annales* school reads like an indictment of several important aspects of archaeology, both new and old. Ricoeur notes in his Zaharoff Lecture for 1978–9: 'Neutral observation, the cult of erudition, empiricism, determinism of the fortuitous, methodological individualism—these form the methodological constellation that the "Ecole des Annales" set itself the task of dismantling' (Ricoeur 1980, p. 8). They rejected the tyranny of documentary 'facts', the primacy of politics, and the straitjacket of nineteenth-century political frontiers. They likewise rejected the prophetic philosophy of history handed down by Hegel and Spengler.

As a consequence of these rejections, the *Annales* historians have provided a potent stock of methods and examples that ought to be of interest to archaeologists. The sense of problem and the breadth of historical vision are their hallmarks and provide considerable overlap with those of archaeology. Both narrative, in the sense of Paul Veyne's (1984) 'plots', and scientific arguments, in the sense given in 'serial history' by Pierre Chaunu (1968), are important to writing history. For the *Annales* scholars, taken as a group, the line between science and literature does not comprise an either-or choice, but the taking up of a position generally closer to the former than the latter (Chartier 1988, pp. 1–14). Their primary concern is with the questions and problems, answers and solutions embedded in the narrative.

Following Marc Bloch (1953), *Annales* historians have used multiple, shifting scales to measure both time and social phenomenon. The power of these methods is demonstrated by Braudel in his use of the temporal scales that encompass events, conjunctures, and the long duration in the second edition of his book *The Mediterranean and The Mediterranean World in the Age of Philip II* (1972–3) and in his historical 'experiments' in the long duration and the geographic, almost timeless expanse of Europe in his *The Identity of France* (Braudel 1988, p. 21). Implicit in these several scales is the varying role played by necessity and contingency as causal factors in stasis and change. The former is far stronger at the level of structure and the long duration; the latter is more strongly associated with the event.

Patricia Galloway is one of the very few North American prehistorians to have incorporated the methods of the *Annales* in her research. In doing so, she has drawn a number of cogent parallels between *Annales* methods in history and those of archaeological research. She notes that ecological and technological analyses in prehistory measure time over the *longue durée*, whereas social and economic approaches look to the *conjoncture* as an appropriate temporal framework. Furthermore, she observes that the facts constructed by *Annales* historians are very much like the facts constructed by prehistorians:

> an *Annales* fact is by and large a tabular fact of some kind, the kind of fact that is gathered in vast quantity in censuses, ledgers, baptismal records, and customs declarations, very much the same kind of fact that archaeologists collect in potsherd counts, records of burial layouts, and distribution of sites over a landscape. (Galloway 1990, p. 4)

Galloway's points can be illustrated and amplified with the aid of a brief summary of the current state of research in what is demonstrably a single, late prehistoric (AD 1050–1550) culture (polity) in Alabama, USA. It is an exercise, following Childe (1956), in pre-*history* or *pre*-history, depending on the perspective.

Excavations at the major site, Moundville, began in 1845, were continuous from 1929 to 1941, and have been intermittent from 1948 to the present. Survey and excavation at several other sites of the Moundville phase has been undertaken almost every summer since 1970. The records and collections produced by this research have been conserved, from 1845 to the present, with great care, and are available for use today (see Peebles (1987) and Scarry (1981) for a reasonably complete bibliography on 150 years of research at Moundville).

Moundville, and sites of the Moundville phase, are set in a 500 sq.km portion of the Black Warrior River Valley near Tuscaloosa, Alabama. For purposes of analysis, this portion of the valley can be treated as an 'island ecosystem', an oasis of fertile soils and floodplain forests set in the midst of extensive and unproductive pine barrens. For purposes of definition, the Moundville site and phase can be called 'Mississippian'. The population grew corn (*Zea mays*) and depended on this staple for the majority of their calories, built truncated earthen platform mounds around a plaza, made the right sorts of shell tempered pottery, participated in widespread exchange of manufactured goods and exotic raw materials, had a burial sociology that is consistent with a well-defined hierarchy of social statuses and offices, and through the distribution of 'foreign exchange' among specific parts of the settlement and with particular burials give evidence of what has been called a 'prestige goods' economy.

The Moundville phase grew (Figure 6.1) from settlements of hunter-gatherer horticulturists (West Jefferson phase, AD 900–1050). The earliest Moundville phase groups (Moundville I, AD 1050–1200) were fully agricultural, and maintained single temple mounds as foci for regional populations scattered among farmsteads and hamlets. In the later Moundville phases (Moundville II and III, AD 1200–1550) the system grew into a complex system of hamlets, larger villages and single mound centres, in which the site of Moundville was the demographic, economic and symbolic centre. This system began to decline in the later Moundville III period, leaving a series of equidistantly spaced settlements in place of the former hierarchy (Moundville IV, Alabama River phase, AD 1550–1700).

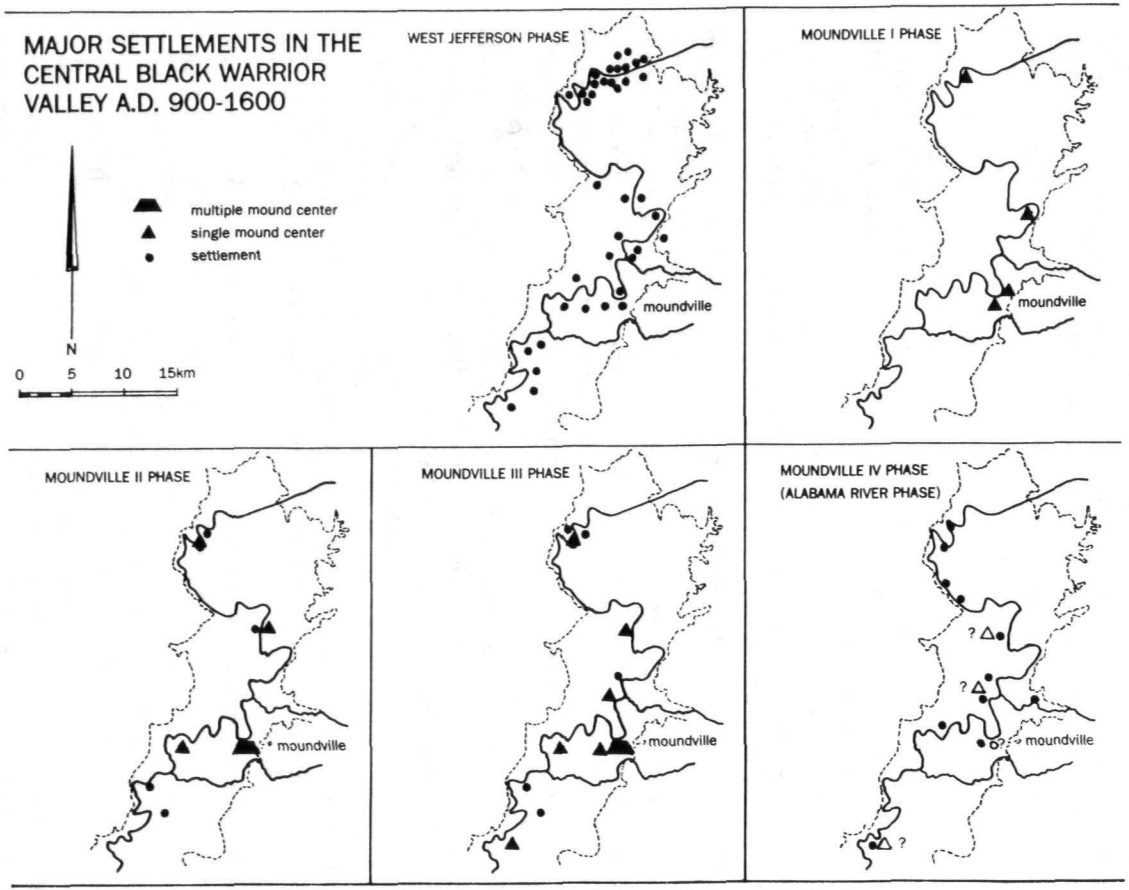


Figure 6.1 The growth of Moundville phase settlements in the Black Warrior Valley, Alabama from AD 900–1600.

The Moundville site itself (Figure 6.2) began as a small West Jefferson phase settlement. At some time after AD 1000 a single mound was erected, making Moundville the southernmost of four such regional centres. Four mounds were in place by Moundville II, establishing the bilateral symmetry of the site and raising it from *primus inter pares* to primate among the regional centres. Moundville reached its greatest size during Moundville III, at which point it comprised twenty large mounds which defined a 40 ha plaza; there were, in addition, social spaces devoted to public buildings, workshops, élite residences, and mortuaries. Soon thereafter, in the earliest part of Moundville IV, the site was abandoned.

The extant data from Moundville and from several other sites of the Moundville phase will support the construction of economic and demographic 'series', in the sense given this term by the *Annales* (see Peebles 1987 and Steponaitis 1989, for the specifics and for additional citations). Variety in the subsistence economy, the flow of imports and changes in the demographic profile (see also Powell 1988) can be measured with a reasonable degree of accuracy (Figure 6.3).

This is not the place to redescribe analyses that are already published. In brief, the Moundville subsistence economy becomes less varied over time: the phenotypic diversity in the corn crop declines by one-half, and the variety of animals in the diet, after a brief increase in diversity, declines slightly. The aggregate of imported goods (ceramics and their contents, shell and copper) reach their peak early in the Moundville sequence, at about the time the site begins its major period of growth. Imports decline thereafter and shrink to insignificance late in Moundville III. That is, imports seem to provide 'leading economic indicators' of the later collapse of Moundville. If, in fact, Moundville is an example of a 'prestige goods economy' then a decline in the volume of essential social currency presages the dissolution of the social hierarchy.

Maximum population size at Moundville was reached in later Moundville II or early Moundville III. Steponaitis has suggested that Moundville served as a necropolis for the whole of the Moundville phase population, and this marked increase represents the growth of population of the Black Warrior Valley as a whole and not just an increase in the population resident at Moundville. His position is now undergoing critical evaluation. The health of this population, from Moundville I through Moundville III was generally good among both élites and commoners; life expectancy at birth was 28 years and at age 20 was 15 additional years (Powell 1988, p. 184). Thereafter, although the population level in the Black Warrior Valley seems to hold steady for several generations, in the Moundville IV (Alabama River Phase), more than one-half of this population bears signs of malnutrition, and over one-third, many of them adolescents, died before reaching adulthood. With the collapse of valley-wide organization, the population descended into a kind of Hobbesian equality that had profound consequences. As if

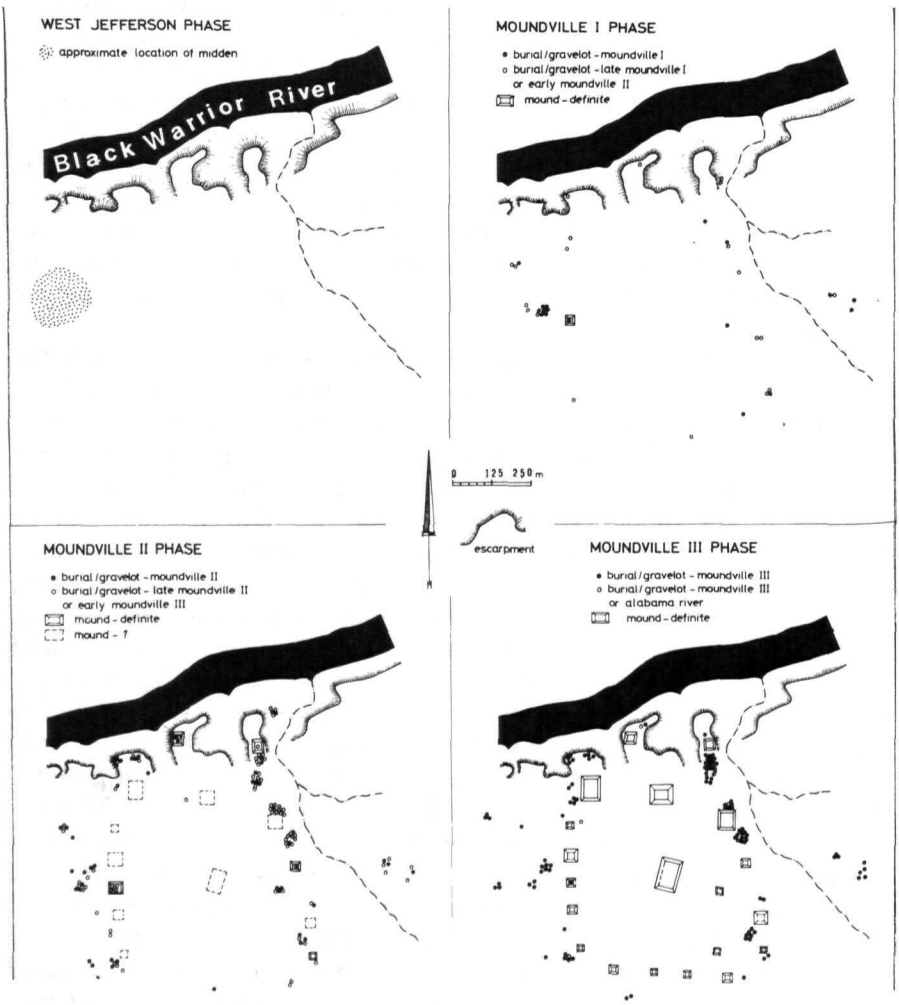


Figure 6.2 The growth of the Moundville site from agricultural village to major ceremonial center, AD 900–1550.

to pile injury upon injury, the effects of the first European explorers would be felt within a very few generations after the collapse of Moundville.

The example of a prehistory constructed from the Moundville materials may be limited, but it is indicative of a history written using just some of the components of the *Annales* style from an archaeological text.

If space permitted, there would be many more central *Annaliste* themes whose relevance to interpreting the rich Moundville database has been or could be explored: e.g. the interaction of structural constraints and possibilities with contingent events in the production of the historical trajectory of Moundville, or the identification of specific *modes*

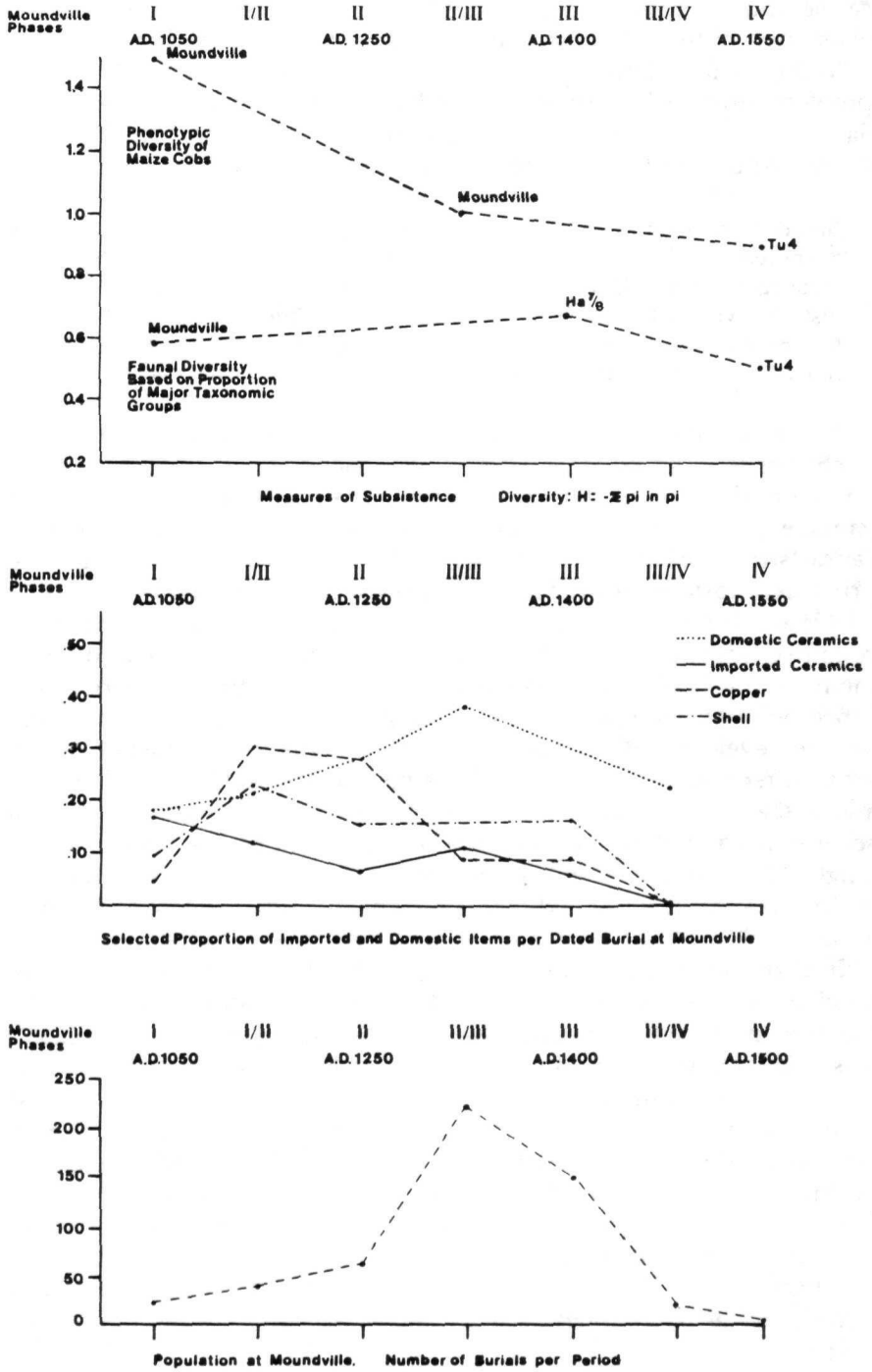


Figure 6.3 Selected economic, ecologic and demographic measures for Moundville and the Moundville phase, AD 1050–1550.

de vie and *mentalités* within the Mississippian phenomenon and the evaluation of their contribution to the observed cultural dynamics.

There is far more to the *Annales*, moreover, than serial history, problem-directed history, eclectic methods, and *mentalités*. There is, as Samuel Kinser has noted, a profound and deep-seated metaphysic, expressed best in the work of Braudel:

> Braudel's concept of the temporalities making up history is metaphysically anchored, attributable to the very nature of the world. And his three metasigns—Time, Space, Man—stand like sentinels at the edges of Braudel's historical vision; they are the absolutes guaranteeing the rest . . .. The timing that Braudel used is a humanly anchored vision, the product of a historian's reflection about the ideal tempo for human affairs. (Kinser 1981, p. 99)

It is, again, a human history, in which the unintended consequences of change far outnumber those that were planned and could have been known to the individuals who participated directly. It is only from the vantage points of a past's future that these consequences and their causal structure can be delineated and perhaps explained. Hence the privileged position of the historian and the prehistorian.

It is at this point, with the repeated emphasis on humankind, that the cognitive sciences can be invoked. As Howard Gardner has defined them, in his book *The Mind's New Science*, the cognitive sciences are concerned with 'the nature of knowledge, its components, its sources, and its development' (Gardner 1987, p. 6). Thus for Gardner—and for most other cognitive scientists—it is both a natural, biological science and a science of representation and intention—a human and cultural science, if you will. It has interests in brain—and mind—and in knowledge. Moreover, it is a historical science in which each generation builds upon the genetic and representational legacy of preceding generations.

In both history and in anthropology—the past and the present—the touchstone of representation, intention and adaptation is knowledge: the human capacity for knowledge and the content of knowledge itself. It is axiomatic that human cultural abilities have evolved; it likewise is clear that current cognition is dependent on perception and ratiocination—on biological apparatus and mental constructs. What is not (perhaps) so clear is that these capacities and their content have a co-evolutionary history. Mind–brain and culture were, at least in part, the selective environments for each other's evolution. To the extent that the cognitive sciences involve themselves in the study of the production, development and deployment of knowledge, then some of the future of archaeology as paradigmatic science and as narrative is bound up in this study.

In geological time, prehistorians and human paleontologists seek the evolutionary pathways of both mind and brain; in somewhat more

recent spans we are interested in archaeological materials and their contexts in the terms laid down four decades ago by V. Gordon Childe: 'as concrete expressions and embodiments of human thoughts and ideas—in a word [as embodiments] of knowledge' (Childe 1956, p. 1). Childe is not offering either an idealist's quest or plea for what has derisively been called 'paleopsychology'. He is arguing for a variant of what Danto has termed 'representational materialism'. Therein:

> We have the concept of knowledge because we need there to be not simply representations but true ones, and not only true ones but those that are true because of the way the world causes us to have those representations in the first place. (Danto 1989, p. 272)

In effect, there is both a human ecology and a representational ecology, both of which are important parts of the human career, and both of which are instantiated to varying degrees in the archaeological text.

To the extent that prehistory can include human intention, representations, and knowledge as important aspects of its conceptual framework, it will become a more exciting and relevant discipline. To this end, the only requirements are some form of basic realism (see Lakoff 1987) and a commitment to critical rationality (in the sense meant by Popper and his students). As Stephen Toulmin argues, the metaphysical focus must shift from the certainty of Descartes and Newton to the critical scepticism of Erasmus and Montaigne, from the cosmos to the polis in Cosmopolis (Toulmin 1989). To the extent that prehistory clings to the tenets of behaviourism and positivism, in which logical empiricism sets the goals of invariance and necessity for the scientific study of all human phenomena, it will be relegated to the same musty warehouses that store the last of the Skinner Boxes, abandoned by most psychology departments more than thirty years ago. To the extent that it continues to ignore history, it deprives itself of its anchor in the present. To the extent that it chooses relativism and solipsism and abandons a standard of judgement for its products, it risks becoming totally irrelevant.

Acknowledgements

I thank John Bintliff for the invitation to participate in TAG '87 and for his infinite patience when decanal duties kept getting in the way of my revisions to the original text. I thank Christopher Chippindale and Michael Herzfeld for reading and commenting on various versions of this text, Patricia Galloway and Vincas Steponaitis for allowing me to use their unpublished works, and L. Halles for collegial support throughout.

Notes

1. Several weeks before the TAG meetings at which this paper was read, I included several of the ideas in this chapter in a presentation at the American Anthropological Association to anthropologists who teach in community (two-year) colleges. They had invited me to talk about the 'limits' of archaeological knowledge and discourse in 1987. After I had finished, a very well-known social anthropologist, a famous cultural materialist, spent his time rebuilding the frontiers I had attempted to erase—presumably to make the world safe once again for his brand of behaviourism. He rose to my prose like a shark to a very ripe piece of meat that had been cast upon the waters. He then took out his 'transitive tar brush', and said with a predatory grin: 'Professor Peebles obviously agrees with Professor A, who agrees with Professor B, etc., etc., . . . who is an adherent of Professor Z. Now we all know that Professor Z is an idiot'. Hence, the unspoken conclusion that Professor Peebles must also be an idiot and what he says must be rubbish. As I sat there, I thought, but did not say: 'Just because Professor 'X' bears a remarkable resemblance to YZ—a completely psychotic American comedian—does not mean that we ought not to take him seriously.'
2. I am indebted to Arthur C. Danto (1989) for this particular distinction between knowledge and understanding.
3. One scholar, from my part of the country I regret to acknowledge, even went as far as to say:

> Southern anthropologists should take comfort and encouragement from the fact that Bloch and Febvre first launched their program from the University of Strasbourg, an institution that was no more in the mainstream of academia than most southern universities today. (Hudson, *Southern Anthropological Society Proceedings*, Vol. 20, p. 22, 1987)

It seems a bit odd to compare his university, which was sued recently and successfully for a conspiracy to graduate illiterate athletes, with one, founded in the sixteenth century, that graduated Goethe and Albert Schweitzer, and that included Pasteur among its faculty. As Carole Fink points out in her recent biography of Marc Bloch (Fink 1989), prior to the First World War, the University of Strasbourg had the largest university library in the world. It continued to be a major university after the war, perhaps the second or third in France, and Bloch, while there, had as colleagues such luminaries as Charles Blondel, the psychologist, and Maurice Halbwachs, a sociologist with an international reputation.

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