Archaeology at the Interface:

Studies in Archaeology's Relationships with History, Geography, Biology and Physical Science

edited by

J. L. Bintliff and C. F. Gaffney

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5, Centremead, Osney Mead, Oxford OX2 0DQ, England.

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PREFACE

This volume contains considerably-expanded versions of a series of invited papers read at the NUARS Spring Conference at Bradford University, on March 22nd 1986.

NUARS, The Northern University Archaeologists' Research Seminar, was founded in November 1977 by John Bintliff (Bradford University) and Chris Arnold (then at Leeds University). It aimed, and still aims, to fill a perceptible void in Northern England, to create an intellectual 'clearing house' or forum for discussion of new ideas and approaches in Archaeology that operates between the yearly TAG (Theoretical Archaeology Group) meetings and international conferences. Meetings are held twice or three times a year, as a day-conference, and the venue rotates around the Northern universities. This meeting in March 1986 was the 16th day-conference, and was financed by the Students' Archaeological Society at the University of Bradford.

Hitherto, NUARS meetings have been fairly themeless, to encourage the widest range of papers and emphasize new thinking and practice rather than period or specialist interests. However this year we decided to organise an autumn and summer term 'open meeting' , but sandwich between these a deliberately thematic meeting on some topic of current concern or interest. Also, we broke with the open-meeting tradition of asking for offers of papers, and in this case made formal invitations to scholars in the North to prepare papers on the chosen theme, these scholars being selected because of their especial knowledge of particular aspects of our theme.

That theme is self-explanatory from the title of this book: ARCHAEOLOGY AT THE INTERFACE: STUDIES IN ARCHAEOLOGY'S RELATIONSHIPS WITH HISTORY, GEOGRAPHY, BIOLOGY AND PHYSICAL SCIENCE. Obviously we could not hope to cover all the disciplinary interfaces in a single day-conference, and we preferred a rapid publication to postponement while we solicited additional papers on missing interfaces. We hope you enjoy the varied diet that has resulted!

The editors would like to thank Mike Heyworth, Colin Merrony and Jean Brown for helping to produce this volume.

ARCHAEOLOGY AND ITS NEIGHBOURS IN ITS FIRST PARADIGM: 16TH - EARLY 19TH CENTURY DEVELOPMENTS

Archaeology in its nascent 16th-18th century form in Western Europe inevitably grew up as an adjunct to History, and with its definition and continuing distinctiveness being its concern with the material culture of the past. Until the mid 19th century, adherence to the literal truth of the Old Testament deprived Europe of any significant prehistory. For if, as was proclaimed by the Church from Biblical analysis, the World had been created in 4004 BC, and later all but depopulated by Noah's flood, the repeopling of the World would have brought human groups to Western Europe little earlier than the first historic descriptions of these 'barbarian' regions by the Greeks. The material culture in the first museums and to be seen by the Antiquarians in the field therefore belonged to peoples belonging properly to History (such as the Celts and Germans, whose ways are "lively described" in [illegible] etc).

Alongside this early dependence upon History, Archaeology by the 17th century had soon developed an active concern with landscape, and many of the major early archaeologists or 'Antiquaries' travelled extensively, placing field monuments into connection with each other and into 'landscapes of the past', as well as noting relevant features of the physical landscape including ancient field systems. So from the first a permanent tie with History and to a lesser extent with Geography [illegible]

ARCHAEOLOGY AND ITS NEIGHBOURS DURING THE 'VICTORIAN' REVOLUTION IN SCIENCE AND THOUGHT

[illegible] Geology [illegible] intimately [illegible] the [illegible] evolution [illegible] as well as [illegible] disciplines via its pioneer [illegible] By the later 19th century a [illegible] influence created an impressive, rapidly-advancing [illegible] the study of man's past [illegible] with the evolution of the physical landscape (Physical Geography), the evolution of the animal world including the human species (Evolutionary Zoology and Biology), and [illegible] of human social and political systems (Social Anthropology). This interdisciplinary new approach to the Earth and its [illegible] of the theoretical underpinning that had characterised the previous phase in Archaeology's development, [illegible] the primacy of Biblical and Classical sources. Instead, Uniformitarianism and empirical science were [illegible] new disciplinary approach, and the shifts in thought involved were radical enough to concern the whole of Western society, both in [illegible] man in nature and society and obviously in the way practical and theoretical science was practised. [illegible] Science, Thomas Kuhn (1962) we can justifiably refer to a revolutionary change in the predominant way of [illegible] explanation, or 'paradigm', at this time.

However, these major transformations [illegible] Archaeology was fundamentally involved, refashioned Prehistoric Archaeology. It is

ARCHAEOLOGY AT THE INTERFACE: AN HISTORICAL PERSPECTIVE

J.L.Bintliff

"Disciplines rarely evolve in isolation; there is a strong and often unsuspected parallelism in the variety of stances which they encompass" (Herbert and Johnston 1978, p.2)

The interface I wish to deal with in this paper, and that which is the dominant concern of this volume, is that connecting Archaeology with sister disciplines such as Geography, History and Social Anthropology/Sociology, as well as more remote disciplines in the Physical, Earth and Social Sciences. My particular theme, which I hope will serve as a helpful introduction to the papers that are to follow, is that of 'winds of change' flowing from these other disciplines into Archaeology, and very occasionally in the reverse direction. I shall trace these revitalizing packages of stimuli from subject to subject, and ultimately back to their source in major transformations within society as a whole. I shall focus my discussion on the following sequence of major developmental stages for our discipline:

ARCHAEOLOGY IN ITS FIRST PARADIGM: 16th-EARLY 19TH CENTURIES

ARCHAEOLOGY DURING THE VICTORIAN ERA

ARCHAEOLOGY IN THE DOLDRUMS: TILL WORLD WAR II

THE POSTWAR REVOLUTION: POSITIVISM RAMPANT

THE RESCUE PHENOMENON

IS ARCHAEOLOGY IN A 'POST-NEW ARCHAEOLOGY' ERA?

- Behaviouralism
- Structuralism
- Macro-Social Structuralism

PROSPECTS FOR PROGRESS

ARCHAEOLOGY AND ITS NEIGHBOURS IN ITS FIRST PARADIGM: 16TH - EARLY 19TH CENTURY DEVELOPMENTS

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Alongside this early dependence upon History, Archaeology by the 17th century had soon developed an active concern with landscape, and many of the major early archaeologists or 'Antiquaries' travelled extensively, placing field monuments into connection with each other and into 'landscapes of the past', as well as noting relevant features of the physical landscape including ancient field systems. So from the first a permanent tie with History and to a lesser extent with Geography was forged.

ARCHAEOLOGY AND ITS NEIGHBOURS DURING THE VICTORIAN ERA REVOLUTION IN SCIENCE AND THOUGHT

In the 19th century the rise of scientific Geology was intimately bound up with the demolition of Biblical chronology, and during the course of the century Archaeology was swept into a veritable revolution in European science and thought, in which it was profoundly influenced by the rise of Geology, Darwinian Evolution and Social Anthropology, as well as contributing very significantly to these disciplines via its pioneer research into Stone Age Man. By the latter 19th century a constant flow of interdisciplinary cooperation and influence created an impressive, rapidly-advancing research front combining the study of Man's past through material culture (Archaeology) with the evolution of the physical landscape (Geology and Physical Geography), the evolution of the animal world including the human species (Evolutionary Zoology and Biology) and the origins and development of human social and political systems (Social Anthropology). This interdisciplinary new approach to the Earth and its occupants was united in its rejection of the theoretical underpinnings that had characterised the previous phase in Archaeology's development, a paradigm dominated by the primacy of Biblical and Classical sources. Instead, Uniformitarianism and empirical science were the key features of the new disciplinary approach, and the shifts in thought involved were radical enough to concern the whole of Western society, both in the way people saw Man in nature and society and obviously in the way practical and theoretical science was practised. In the terminology of the philosopher of Science, Thomas Kuhn (1962) we can justifiably refer to a revolutionary change in the predominant way of thought and explanation, or 'paradigm', at this time.

However, these major transformations in which Archaeology was fundamentally involved, refashioned Prehistoric Archaeology. It is only in the more limited but nonetheless important area of practical methodology that other branches of Archaeology such as Classical and, later, Medieval Archaeology received major stimuli: here we can look at the mutual advances made by Geology and Archaeology in data collection and recording via stratigraphy and accurate planning. Apart from this, Historic Archaeology took little or no interest in the heightened interactions that Prehistory had achieved with Geology and Social Anthropology, and till relatively recently has remained committed to a view of Archaeology as an illustrator of historic texts. It also naturally followed the perennial concerns of mainstream History, which since the Tudor era had centred on a 'Battles and Famous Men' view of the past, in which individual and national character were at the forefront of attention. In Britain, for example, pre-1914 History was essentially 'continuous national history' presented in linear sequence: "The grand theme of the pre-1914 historians was that of the foundations of national greatness...In the schools, historical subjects seem to have been deliberately chosen for their inspirational qualities...or to register milestones of national progress" (Samuel 1984, p.7). A Board of Education circular in 1905 states: "For children in English schools the chief interest in history will rightly centre in the stirring events and in the striking qualities of the central figures of our history". European History was a conspicuous absentee from school curricula, and indeed even today History contains a powerful body of adherence to traditional disciplinary concerns: the Professor of Modern History at Cambridge, Prof.Elton, recently argued that "Long stretches of English history" ought to be the backbone of a university course, expressed wonder at the attention paid to "that curious extra-terrestrial place, the Third World", and regretted the recent invasion of History by the Social Sciences (Samuel 1984, pp.6-7).

ARCHAEOLOGY IN THE DOLDRUMS: ARCHAEOLOGICAL THEORY AND PRACTICE AND ITS NEIGHBOURS UP TILL WORLD WAR II

A general trend discernible in (Prehistoric) Archaeology and its sister disciplines during the first half of this century has been a retreat from the self-confident generalising approaches of the Victorian era. The contemporary pressures that led to this shift have been analysed elsewhere (cf. Bintliff 1984, Introduction), and include a rejection of evolutionary approaches involving value judgements about contemporary 'primitive peoples', a crisis of confidence about the validity of a Uniformitarian approach to social evolution, and a cultural and biological relativism seeking to analyse the particular qualities of distinct human cultures or forms of animal life. Social Anthropology decisively turned away from its association with Archaeology in reconstructing past social systems, and under the Structuro-Functionalist banner in Europe and Boasian regional particularism in th States, focussed on the systematic analysis of contemporary non-industrial communities as almost ahistorical organisms.

The mainsprings of this crisis of confidence clearly emanated in the nascent Social Sciences. In Geography, whereas the Physical branch continued to develop throughout this period a series of bold general models, those branches concerned with Man exhibit the same characteristics of a retreat towards the unique and towards analysis, and away from generalising explanation. Describing the period up till the 1960's in fact, it has been stated that: "it is clear that human geography in practice was centrally concerned with exceptionalism and stood in some contrast to the search for general laws in several branches of physical geography" and "a form of urban geography which continued without major conceptual change until the early 1960's...was still centrally concerned with the inanimate qualities of the city and its setting...the urban morphology trend up to this time remained descriptive and essentially idiographic" (Herbert and Johnson 1978, pp.8,11).

In History, the crisis of confidence in national destiny and Progress created a similar shift of direction to greater particularism: "after the trauma of the First World War...in history as in politics, a more introverted view prevails...historians deliberately turned attention from the history of the state to that of everyday things...the rhetoric of the 'small man' prevails" (Samuel 1984, p.8).

Abandoned by Social Science, Prehistory fled from its Victorian grand models of evolving society marching hand-in-hand with technological stages, into the same ethnocentric and particularising approach. The record of material culture was attributed to distinct peoples or races, and changes in this record were normally attributed to historic events such as migrations or the spread of ideas and innovations from the historic centres of civilisation to the non-historic peoples of prehistory. Even where Prehistory could not be 'explained' by ultimate reference to the historic record in this way, which obviously meant a 'Battles and Famous Men' interpretation, the same approach as a 'pseudo-history' was employed to make sense of prehistoric societies, the archaeological data being force-fitted into untested scenarios derived from traditional history and injected into the mute past. In the figure most characteristic for this tradition in this century, Sir Mortimer Wheeler, we see the steady advance of scientific techniques of data collection and recording shackled incongruously to a limited historic and 'pseudo-historic' mode of interpretation (Bintliff 1983). Under the surface, however, prehistorians remained faithful to the Victorian model of technological and social stages in human progress, but were constrained by the disapproval of Social Anthropology into only oblique references to such conceptions (Bintliff 1984, Introduction).

On the other hand, Gordon Childe strove to integrate the complex 'organic system' approach of Social Anthropology into his mature works, and dared not only to defend explicitly the 'Stage Schemes' for human progress but also introduce the generalising models of Eastern Bloc historical materialism into his interpretations of European and Near Eastern prehistory and early history. Significantly during his lifetime it was his reconstructions of 'what happened next', (which remained to the end coloured by Migration and Diffusion), that were widely accepted by fellow archaeologists, and his pioneering achievements in generalising theory and social archaeology were ignored.

Historic Archaeology, meanwhile, was not troubled by a divorce from Social Science, merely moving in step with the latest debates in History and with more myopic debates in period-focussed, text-orientated research; it did however continue to advance its practical skills along with Prehistory, especially in the sphere of excavation.

Archaeology as a whole did not lose touch, though, with Geography and Geology, and indeed the primacy of local culture and the unique qualities of place and people was a shared theme that brought influences into Archaeology from Human and Historical Geography. Both

Geography and Archaeology adapted the idea of a 'cultural system' from Social Anthropology and created the view of closed populations sealed within natural regions. The 'superorganic', self-regulating culture was often seen as overriding individual fates, and the influential cultural geographer Carl Sauer was to write that Human Geography was "a science that has nothing to do with individuals but only with human institutions, or cultures" (1963, p.358). Especially in Britain we can trace a strong branch of Geographic Archaeology in early to mid-century associated with Cambridge and the names of Crawford, Fox and Grahame Clark. The fortunate longevity of Professor Clark ensured that British Archaeology was well-prepared to accept, at least in the Universities, a reorientation in Man-Landscape studies towards the growth field of Ecology, from the 1960s onwards, and it was Clark's protege Eric Higgs who fronted the influential pressure group in the New Archaeology dubbed by David Clarke (1972a) the 'Ecological Paradigm'. Grahame Clark's willingness to shift from Geographical influences to influences from the Biological Sciences demonstrates another continuing link that a minority of archaeologists maintained with other disciplines actively researching into the changing environment. Normally however Archaeology's ties to the Earth and Biological Sciences were like those it had even more tenuously to the Physical Sciences, ie occasional background information was obtained as a service; the method and theory of those disciplines did not impinge significantly on those of Archaeology. Certainly no longer, as in the heady days of the mid 19th century, did Earth and Social Scientists, or Biological Scientists look to Archaeology for mutual influence and collaboration towards common goals.

In summary, Archaeology during the first half of this century has been broadly characterised (cf. Binford 1968a; Trigger 1984, p.277) as dominated by:

(1) Culture History: the delineation of races and peoples by means of their material culture, and where possible, their historic record, leading to interpretations of the past centred on specific events and national or even individual character, mainly aimed at achieving a linear narrative.

(2) Lifeways: a concern with the particularistic description of everyday life in its wonderful diversity through the past.

By the end of World War II, Archaeology existed as a strange, immature and fragmented discipline (cf. Clarke's famous dictum: "an undisciplined empirical discipline" (1968, preface)). On the one hand, its early association with Geology had spawned an increasing sophistication in the physical recovery of material culture, its recording in the field and later analysis in the laboratory. Yet the purpose of this meticulous and increasingly 'scientific' activity was highly variable, generally depending on the period specialisation of the archaeologist and especially on his perception of the relation of his finds to History. For Prehistorians, the unacceptability (except in the Communist World) of a Social Darwinism or Historical Materialist approach, both of which favoured analysis of local processes of change, had led to the largely erroneous pseudo-history of the migration of peoples and diffusion of innovations from historic centres. Historic archaeologists suffered from a disciplinary inferiority-complex towards textual historians, and this effectively stifled the development of an independent approach to archaeological data. This continues to this day amongst most Classical and Medieval archaeologists, who give the impression that their activities are less useful, less accurate and too impersonal a data-base for the reconstruction and explanation of the past compared to written sources. Syntheses such as Frere's Britannia (1978), most contributions to the synthesis Anglo-Saxon England (Wilson 1976), and the historical chapters in the synthesis The Origins of Europe (Collins 1975) exemplify this attitude.

THE POSTWAR REVOLUTION: POSITIVISM RAMPANT

The Second World War, and the immediate postwar decades of restructuring and redeveloping of Western society, had a profound effect on most academic disciplines. First to respond were naturally practitioners of the hard sciences (and spin-offs were to include radiocarbon dating), followed closely, especially after the War, by Social Science, then with increasing belatedness by Geography, History and Archaeology. This development has been traced for Human Geography by Herbert and Johnson (1978):

"The growth of industrial capitalism was characterised by, and advanced because of, the widespread cooption of academic-intellectual effort into the economic system. Not surprisingly, it was the physical scientists who were initially involved in the movement away from the 'ivory tower', for it was they, particularly in the applied science offshoots, who provided many of the technological and technical breakthroughs necessary for the increases in the surplus value of labour which fuelled the growth of capital. Major contributions also came with the development of the techniques of global warfare in the twentieth century and the growth in size and influence of the military-industrial complex, with which many scientists are deeply involved".

"The important public role of physical scientists was observed by those who wished to emulate them in terms of methodology, and making a contribution to public policy became a part of the scientific ethos to which, increasingly, social as well as physical scientists were attached...it was economists and social psychologists who first achieved a recognition of relevance..to be followed by sociologists and social administrators. Emulation of this apparent eminence thus became a desideratum of human geographers, too, and 'policy implications' increasingly formed the basis of the conclusions of positivist research reports...It was the regionalism paradigm...which provided the first major area within which human geographers could practise their expertise in the service of governments. This avenue was opened by the development, after the Second World War, of physical planning programmes" (op. cit. pp.25-26).

"After the Second World War, a number of changes occurred in the geographical discipline, several of them reaching their apogee in the United States between 1955 and 1960 and in Great Britain a few years later...(The) origins of these are many and varied. Important among them was probably the war itself, the organisation of which involved geographers working in teams with members of other disciplines...and thereby becoming aware of the current methods, interests, and issues beyond the narrow horizons of the few other subjects - anthropology, geology, history - with which they had maintained some contact. This was indeed a period of major change in many social science disciplines. All of this work had the following characteristics:

1. It was nomothetic (ie law seeking) rather than idiographic, (ie descriptive), focussing on the general trends and patterns and interpreting specifics within an explicit theoretical matrix.

2. It used numerical methods to analyse its data and so was scientifically 'respectable'.
3. It apparently had predictive power and so could be used in the development of public policy" (op.cit. pp.7-8).

A similar analysis has been proferred by Grano (1981, pp.31-32), who identifies a new atmosphere spreading to the Social Sciences, which was "based on probability and used statistical quantification as its main method. The background to this was the change that was taking place in the relations between society and science. Science was seen as the best way to achieve economic growth. This resulted in geography disparaging its own object of study and instead laying emphasis on quantitative methods...This period of 'social physics' was also non-humanistic: man was seen only in terms of statistical distribution and region as a topological surface."

These trends in Geography peaked during the 1960s, and in Britain culminated in major textbooks of this 'New Geography' such as Models in Geography (Chorley and Haggett 1967) and Explanation in Geography (Harvey 1969):
"Throughout the 1960s the methodology of those investigations was both extended and sharpened. The aim was to be scientific - as physics and chemistry are - and to provide quantifiable theories and laws...These theories had a basis, often perhaps only implicit, in mechanistic assumptions of human decision-making, the concept of economic or perfectly rational man who made decisions on the basis of complete knowledge and omniscience and who was translated by geographers into spatial man...whose choice of locations was based upon the minimization of movement costs. Later, there were models adapted from Parsonian sociology, which accepted a view of society as composed of individuals allocated to particular places within the economic and social order from which, by dint of personal effort, they might escape to a higher level. The members of these various groups then compete for territory, with the resulting spatial order representing a consensus acceptance of a certain pattern. These derivative sources, and the ways in which they were translated into geographical models and theories, ensured an essentially positivist and functional suite of postures" (Herbert and Johnson 1978, pp.15-16). The positivist label attached to this whole movement reflects its lack of concern for human values and experience and a reductionism to the gross movement of data that is empirically observable and quantifiable.

History was to respond in very similar ways to the wind of change blowing from Social Science and public involvement, and this development and what was to follow it have been fruitfully analysed by Tilly (1984):
"in the 1960's, many historians felt that historical theory and practice alike were undergoing great changes. Some felt the changes threatened the proper performance of the historian's function...Others felt that history finally stood on the threshold of Science."
"Let us pay particular attention to the historical endeavours which in the 1960's began to display the stigmata of social science: self-conscious explication of concepts and models; deliberate comparison of individuals, groups, places, or events (often many of them) placed within a common framework; and fixation on reliable forms of measurement, frequently involving numerical treatment of evidence. Economic history, archaeology, demographic history, urban history, plus some kinds of political, labour, agricultural, and family history qualify" (op.cit. pp.363-365).

A key figure was Lawrence Stone, whose research was "designed to apply statistical methods of analysis to data of varying quality, in order to test some subjective impressions and traditional assumptions about English social structure and social mobility in the Early Modern and Modern periods" (Stone and Stone 1972, p.56). Stone also aimed to "combine the humane skill in historical reconstruction through meticulous concentration on the significant detail and the particular example, with the statistical and theoretical preoccupations of the social scientists...It could help reconcile history to sociology and psychology" (Stone 1972, p.134).

Even later than the 'New History' in Europe was to be the inception and diffusion of the 'New Archaeology'. Its origins were likewise both direct, from changes in society, and indirect, by the emulation of transformations that had already occurred in the Social Sciences, Geography and History:
"By the 1950's, a growing number of archaeologists were smarting from the charge that their discipline was descriptive rather than theoretical in orientation" (Trigger 1984, p.277).

After some kite-flying demanding new approaches in the late 50's (eg Willey and Phillips 1958), the 1960's witnessed a gradual crystallization in the United States of the elements necessary for a new approach to Archaeology. The various strands were skilfully brought together with forceful polemic by the acknowledged leader of the movement, Lewis Binford, in the 1968 volume New Perspectives in Archaeology. Acceptance of the virtues of the New Archaeology spread rapidly throughout America during the 1970's and informs almost all major public and University archaeology at the present day.

The effect of the 'wind of change' in Europe was to be a very different affair. The initial battle standard was raised by the lone figure of David Clarke in his remarkable and still infamous volume, also of 1968, Analytical Archaeology. By 1972 he was able to assemble a second, equally influential volume, Models In Archaeology. The contributors formed a rather shaky 'School', and the title was a direct homage to its obvious inspiration of the mid-1960's, Chorley and Haggett's Models in Geography. Through the 1970's the New Archaeology made slow progress through the British universities, greatly aided by a second front opened up with the emergence of Colin Renfrew as a leading propagandist for the primarily American version (eg Renfrew 1972). However for reasons which we shall investigate shortly, New Archaeology by the 1980's had succeeded merely in influencing most university prehistorians into new approaches; Classical and Medieval Archaeology remained virtually unscathed, and the professional archaeologists and amateurs were paying little or no attention to the phenomenon. Britain was actually the most receptive to the 'wind of change'. Also by the 1980's Scandinavian prehistorians and those in Holland were largely won over, major inroads were being recorded in Italy, but France and Germany remained virtually insulated from the contagion.

In line with those trends already cited for parallel programmes of change in sister disciplines, Archaeology's 'New' format was characterised by:

1. A desire to push the discipline out of its traditional, literary mode into one typified by quantification and statistical testing.
2. The rejection of particularist concerns, whether focussing on discrete events or individuals, in favour of generalising explanations

focussed on trends, societies and systems (cf. Figure 1).
3. A search for law-like propositions about human culture in the past, such as might be found useful by the Social Sciences and thereby bring Archaeology back into the fold of mainstream studies of Man.
4. A deliberate policy of demolishing disciplinary boundaries, bringing in by the cartload new 'models' and practical techniques developed in other disciplines.

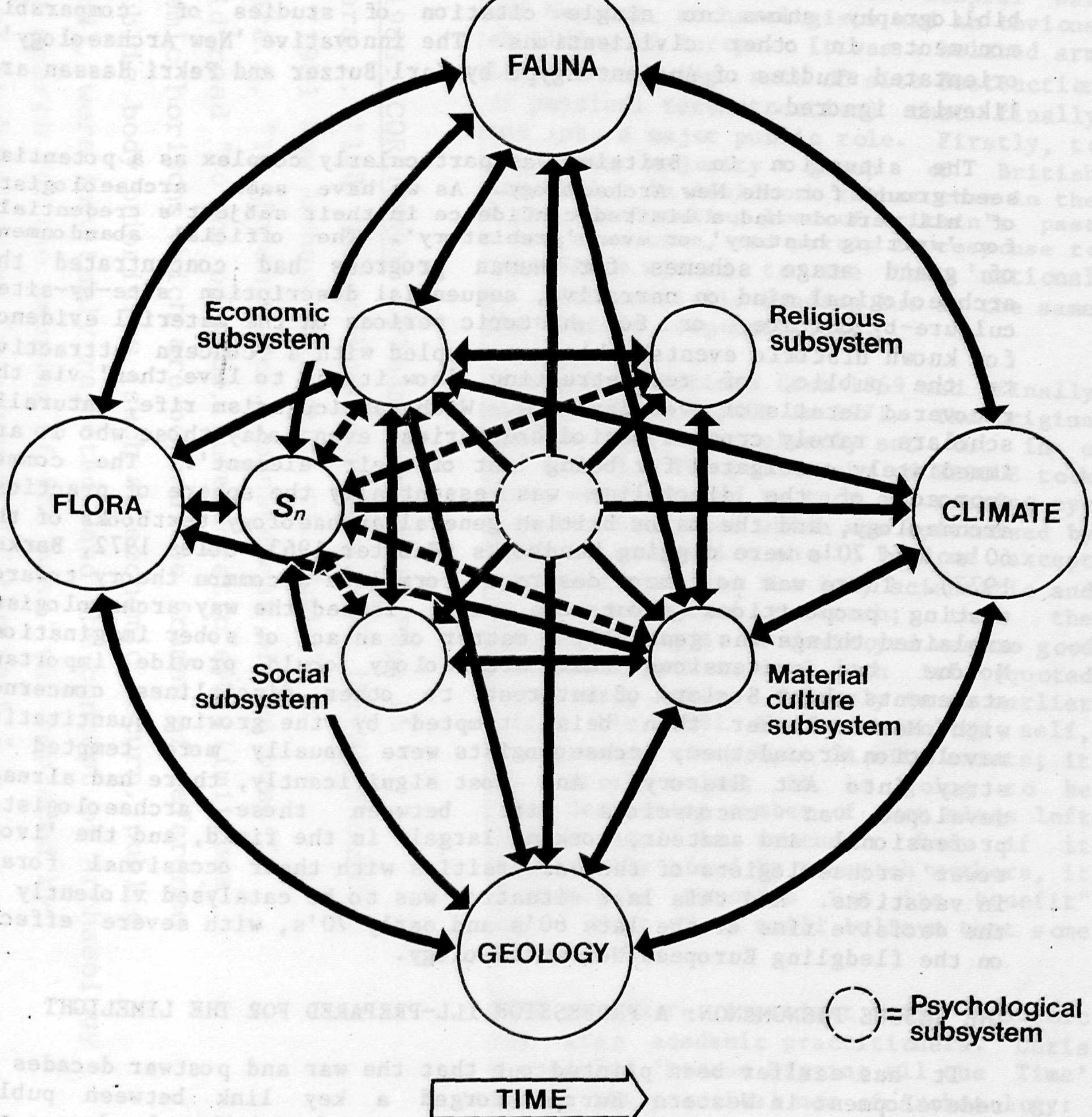
The success of New Archaeology in the States need detain us little in this discussion. American Archaeology being formally integrated with Social Science under the rubric of Anthropology had made its exposure to the wind of change in the Social Sciences quite inevitable. Significantly, the crucial decade when New Archaeology established itself in the States was a time when state funding for Archaeology leapt into a multi-million dollar investment. The massive demand for trained personnel was fed by graduates freshly-indoctrinated by the New Archaeology in its most virulent form. The provision by law that public and private developers provide in their budgets for all necessary archaeological investigations thereby brought under threat has created a source of funds hitherto undreamt of and the opportunity to test and refine the full range of theories dreamed up in academia by the gurus of the new movement.

In Europe many factors conspired against any radical change in the discipline of Archaeology. Historical archaeologists were little affected by the New History, which naturally was concentrated on the post-Medieval periods where data was most reliable and easiest to quantify. Their interests in developments in Geography and Social Science were traditionally minimal. As recently as 1981 Professor Philip Rahtz in his inaugural lecture at York University set out for the first time a programme for a future 'New Medieval Archaeology' in this fashion:
"I will also indicate the extent to which medieval archaeology still retains its original role as the 'handmaid of history', working wholly within a framework provided by written sources, - and how it may begin to establish itself as a more autonomous study. This latter aim is part of a more general trend in archaeology in its aspiration to be a science" (op.cit. p.3).
"There is, as in many subjects, a wind of change, the effect of which on medieval archaeology we cannot yet estimate. This is the impact of the modern theoretical or 'new' archaeology, developed primarily in prehistoric studies, and only now beginning to penetrate into those of later periods" (op.cit. p.6).

For Classical Archaeology, likewise, the influence of the New Archaeology has been slight and highly localised. Surprisingly this is almost as true of American Classical archaeologists as European, but the reason lies in a traditional background in Classics and History on both sides of the Atlantic. As late as 1980 Colin Renfrew was to throw out a challenge for American Classicists to bestir themselves and find out what was happening in other branches of Archaeology, where fresh horizons of method and theory had been revealed by the New Archaeology movement:
"There is therefore a brilliant opportunity for anyone who can command the data and the scholarship of the Great Tradition while employing the problem-orientation and the research methods of current anthropological archaeology" (Renfrew 1980, p.297).

The challenge has been thought necessary to repeat by a 'New

Figure 1



"A static and schematic model of the dynamic equilibrium between the subsystem networks of a single sociocultural system and its total environment system. S represents the summation of the effects of the alien sociocultural systems connected to S by cultural 'coactions' (dashed lines) and to the environment by 'interactions' (solid lines). To set the model in motion all the components must oscillate randomly along intercorrelated trending trajectories."

(**David Clarke**, Analytical archaeology 2nd Ed (1979), Figure 23)

Classical Archaeologist' in 1985 (Snodgrass 1985). But there are many specialist areas of traditional Archaeology where the absence of theoretical awareness is truly staggering. The revised edition of I.E.S.Edwards' standard study of the pyramids of Egypt (1985), for example, treats these monuments as all but disconnected from the wider context of socio-economic development in Egypt, whilst the lengthy bibliography shows no single citation of studies of comparable monuments in other civilisations. The innovative 'New Archaeology'-orientated studies of Ancient Egypt by Karl Butzer and Fekri Hassan are likewise ignored.

The situation in Britain was particularly complex as a potential seed-ground for the New Archaeology. As we have seen, archaeologists of all periods had a limited confidence in their subject's credentials for 'writing history' or even 'prehistory'. The official abandonment of grand stage schemes for human progress had concentrated the archaeological mind on narrative, sequential description site-by-site, culture-by-culture, or for historic periods on the material evidence for known historic events; this was coupled with a concern attractive to the public, of reconstructing 'how it was to live then' via the recovered details of everyday life. With particularism rife, naturally scholars rarely crossed period boundaries; even today those who do are immediately castigated for being 'out of their element'. The common approach of the discipline was essentially the sphere of practical archaeology, and the major British general archaeology textbooks of the 60's and 70's were digging handbooks (Webster 1963, Coles 1972, Barker 1977). There was no common desire to formulate a common theory towards testing propositions about the past, indeed the way archaeologists explained things was generally a matter of an act of sober imagination. No-one had pretensions that Archaeology could provide important statements about Society of interest to other disciplines concerned with Man. Rather than being tempted by the growing quantitative revolution around them, archaeologists were usually more tempted to stray into Art History. And most significantly, there had already developed an unconscious rift between those archaeologists, professional and amateur, working largely in the field, and the 'ivory tower' archaeologists of the universities with their occasional forays in vacations. And this last situation was to be catalysed violently at the decisive time of the late 60's and early 70's, with severe effects on the fledgling European New Archaeology.

THE RESCUE PHENOMENON: A PROFESSION ILL-PREPARED FOR THE LIMELIGHT

It has earlier been pointed out that the war and postwar decades of redevelopment in Western Europe forged a key link between public service and those academic subjects concerned with Man, Landscape and Society. With massive opportunities arising from expanding employment niches, these subjects responded with a new format stressing their explanatory and even predictive power, at the same time evincing their responsible attitude by a rapid shift towards quantification and testing of propositions. Furthermore, to cope with State demands, generalising analysis overrode particularist, the individual yielded to the population. Management strategies seemed more appropriately met by treating units of data as structured into systems whose whole was larger than its parts. Systems of culture were envisaged as changing not by the behavioural variability of individual people but by gross shifts in major component subsystems. Terms such as mutual feedback, multiplier effect, system destabilisation were the 'buzz' words, and the whole approach has rightly been characterised as mechanistic and de-humanising.

Whereas Social Science, Geography and Modern History were drawn inexorably into new formats dominated by such considerations, Archaeology's public profile remained far longer out of the bureaucratic eye; nor was it immediately clear that after helpful war service in Air Photography and Mapping, archaeologists had an obvious role to play in the postwar rebuilding of Europe. It was a belated arm of the State and the rapidly increasing rate of site destruction (linked to that very process of physical reconstruction) that finally dragged a reluctant discipline into a major public role. Firstly, to take Britain as our example, that majority group of British archaeologists whose main interests lay in fieldwork rather than in the library, were all too aware of the accelerating loss of Britain's past in both town and country; at the same time, and partly in response to pressure from that community, the State wished to see the 'national heritage' organised properly, with realistic funding but at the same time a clear limit placed on the State's responsibility.

The fieldworkers' pressure group had meetings from 1969 and finally established a national committee called RESCUE in 1971. The origins and purpose of the RESCUE phenomenon were later clearly set out in a volume edited by Philip Rahtz (1974). The philosophy of RESCUE took issue with the academic viewpoint, which seemed to turn an uncaring eye on the increasing pace of site destruction, and which was epitomised by Collingwood's pre-war dicta: "that no excavation should be done except to answer a specific problem; that this was the only scientific and scholarly way of approaching archaeological research; that the incidental fact that a site was going to be destroyed was no good reason for stepping out of the academic path to deal with it" (quoted in Rahtz, op.cit. p.56). In contrast, Rahtz - (clearly in an earlier and different manifestation from his 1981 New Archaeology self, cf.infra.) - defended a quite contrasted viewpoint: "A site exists; it contains evidence of potentially historic value; it is going to be destroyed; there are a limited and decreasing number of such sites left in this country; surely the evidence should be rescued. Even if it does not answer the problems currently posed by research workers, it will be needed by posterity and should be recorded for its benefit" (p.56). And as a sop to theory - "Most of us still believe that some research work should continue" (p.58).

Other leading figures in RESCUE were more impatient with academic research concerns, and clearly also academic practitioners. Chris Musson, in a remarkable chapter entitled 'Rescue Digging all the Time' conjures up a new world dominated by the practical men of Archaeology:
"there is no reason why six archaeologists should not dig a whole hillfort or a Roman camp, so long as they are allowed to spend a year or more over it" (1974, p.83) - Yes, but what was being sacrificed for speed?
"the full-time excavator can overtake the professor in no more than three years' continuous digging" (p.86) - Maybe in wielding a pickaxe, but what about the research progress?
"a growth in the number of professional diggers - specialists in excavation - should not be seen as a surprising or even a dangerous departure...In the simplest terms the contrast is perhaps between the 'thinker' and the 'doer', without implying that the thinker never acts, nor that the doer never applies his mental faculties. Indeed one of the greatest pleasures of excavation is its demand for the exercise of both mind and body in an intensely practical task...The full-time

digger enjoys the exercise of his practical skills, just as the thinker draws satisfaction from his mental agilities...For the right kind of person full-time rescue work can be infinitely more satisfying than a misconceived academic career, with its more ephemeral objectives...Unfortunately the vast majority of students, when considering a career in archaeology, see university teaching or research as their ultimate objective. Perhaps this is in ignorance of the alternatives" (Musson, op.cit. pp.87-88).

Graham Webster further encourages the creation of a non-academic, non-research, and non-theory community of archaeologists in Britain in a chapter that ironically demonstrates a total lack of awareness of the wind of change emanating from the United States since the 1960s; his chapter is entitled: 'Training the New Archaeologist' - "In the long term there is a need for the provision of permanent training centres, permanently staffed and where there are facilities for training in all branches of practical archaeology...These centres could be enterprises shared by the Government (DOE), the new regional authorities, and the universities. In addition to permanent staff to carry out teaching and research, it should be possible by secondment for members of university departments and local-authority museums to play an effective role...This field training, as distinct from academic training, is a primary necessity" (Webster 1974, pp.237-238).

The suggested syllabus for Webster's training centre is conspicuously lacking in any role for interpreting past societies, concentrating rather on the techniques of data recovery, description and the reconstruction of the changing physical format of the site.

Significantly, the recommended reading for this major book on Rescue Archaeology of 1974 focusses on traditional digging handbooks 10 and 20 years out of date, and an article dismissing the relevance of New Archaeology by Hogarth (1972).

British Archaeology had responded to the growing destruction of our heritage; the State had recognised its responsibility and wished also to systematize its financial and legal commitment to the national heritage. The result was a massive input of funds into the kind of practical archaeology promulgated by the RESCUE pressure group. In this atmosphere it is hardly surprising that the excitement generated in some university archaeology departments by the messianic doctrines of Binford and Clarke, calling for a central focussing on matters of philosophy, theory, for an end to literary, humanistic archaeology and a move towards a hard science subject ruled by statistics and computers, and for the pulling of Archaeology into the trendy world of Social Science - all this was little more than esoteric, ritual mumbo-jumbo in the ivory towers for the 'diggers' who now had the bit between their teeth!

To the outside, bureaucratic eye, limitless academic horizons which are beyond the frames of reference of public body priorities (such as economic return, public demand), have inevitably replaced the concept of Archaeology as 'a search for the meaning of the past' with that of Archaeology as 'management of the past'. In Britain, as embodied in the creation of the quango English Heritage, such an interpretation predicates the necessary personnel to protect and analyse 'objects' of public interest (museum artefacts, standing monuments), devoid of the intent to further illuminate the human past. Beyond this area of responsibility for the physical heritage, public demand may increasingly be satisfied by the traditional goals such as the reconstruction of lifeways for public titillation (eg the Mary Rose, the York Archaeological Trust's 'sniffing through the past' train ride).

Despite the generally pervasive paradigm of New Archaeology in the States, there have also been pressures there to remove academic and theoretical considerations from non-university professional archaeology or 'Public Archaeology'. Thus Lipe argued in 1974 (p.214): "If our field is to last beyond a few more decades, we need to shift to resource conservation as a primary model". As King has pointed out, this can lead to entirely negative feedback: "if the consulting archaeologist says the thing is 'archaeological', it is avoided. This being the case, it is a short step to the conclusion that we need not even describe the phenomenon; we need merely specify that it is archaeological" (King 1983, p.147). Unfortunately: "The argument against a responsibility to do research is that the legal and business contexts in which public archaeology is done have nothing to do with research...With the partial exception of the National Park Service, no federal agency that funds public archaeology has archaeological research as its mission. To the agency or industry supporting an archaeological project, if not to the laws that require that support, the outcome and indeed the existence of the research is truly 'incidental'" (King op.cit. pp.152,155).

IS ARCHAEOLOGY IN A 'POST-NEW ARCHAEOLOGY' ERA?

Having identified in Europe an embattled minority of primarily university archaeologists closely committed to a New Archaeology approach, directly comparable to the new postwar formats in sister disciplines, it has to be admitted that that battle seems to have been lost, for not only do we see most of the territory still in enemy hands, ie the traditionalists, but formerly loyal figures are deserting New Archaeology strongholds and proclaiming that New Archaeology as a movement is a phenomenon of the past. John Barrett, for example, has written (1983, p.189): "The New Archaeology is now some twenty years old, and although it cannot be dismissed simply to seek a new fad, ideas are being developed which stand in sharp contrast to those developed in the 60's and 70's". Bruce Trigger opens an article significantly entitled 'Archaeology at the Crossroads: What's New?', with the statement: "Is archaeology in serious trouble or does it stand on the threshold of brilliant new accomplishments?...There is growing uncertainty about the theoretical propositions relating to human behaviour that have guided the interpretation of archaeological data for the past twenty-five years" (1984, p.275). Ian Hodder (1982), explicitly rejecting the assumptions of the New Archaeology, and attempting to float a new school of theory, has sought reconciliation with the aspirations and priorities of traditional archaeologists such as Daniel and Piggott. Even Professor Colin Renfrew, in his inaugural lecture at Cambridge (Renfrew 1982), appeared to at least one reviewer to be seeking a path back to traditional, humanistic approaches: "perhaps the first example of the 'post new archaeology' phase" (Selkirk 1983, p.68). In a survey of recent publications in American Archaeology, Dunnell (1984) singles out: "a loss of faith in the dominant strategy of Americanist archaeology, the so-called new archaeology" as "the most important occurrence in the last decade" (p.490).

But to understand this apparently perplexing turn of events in academic Archaeology we need only turn to trends in our sister disciplines, and ultimately to shifting attitudes in contemporary society, where we shall find that Archaeology once again has been exposed, this time with great effectiveness as far as academic archaeologists are concerned, to the dominant wind of change.

In Geography we are informed: "in part a reaction against some of the early excesses of quantification and models based upon the assumption of economic man...there has been since the late 1960's a general reaction against positivism as a conceptual position and the examination of alternative philosophical stances within human geography...Phenomenology and the behavioural emphasis have provided one reaction against positivism, others have included the call for a conflict rather than a consensus view of society...which owes its major stimulus to...Karl Marx, whose influence has permeated geography much later than other disciplines...Finally...there has been a much more persistent questioning of the ways in which geographical research should be made relevant and for whom it should be relevant. Should geographers be content to concentrate their efforts on the spatial outcomes of social problems or should they more profitably examine the societal structures and allocative systems which produce the problems in the first instance?" (Herbert and Johnson 1978, p.9,10).

"During the last decade, geography has witnessed the re-emergence of a critical, reflective attitude towards explanation and understanding. In contrast to the legacy of a positivist philosophy of science, epitomized in geography by an emphasis on quantitative analysis and model building as fundamental to the development of spatial science, a desire for greater philosophical articulation has typified the various attempts by human geographers to appropriate for their discipline a suitable philosophy of the explanation of human behaviour" (Harrison and Livingstone 1982, p.1).

"Attempts have...been made to reverse the scientific method's process of isolation of the object of study and return it to its real local and temporal context and situation. In this approach, the object of study is a combination of empirical data from nature and human life in the outside observed real environment...The result is a psychological view of the environment...The new humanistic geography and radical geography, which have been united...under the name of 'social humanism', have developed the perceptual-cognitive approach further to a stage where the scholar is identified with his object of study, individuals' or social groups' perceptual-cognized environment. The application of geography is in that case directed...to the future potential environment as expressed in values" (Grano 1981, p.33).

It will be worthwhile briefly examining these different new directions in turn:

Behaviourism

Within the Social Sciences a strong reaction began to develop in the States from the mid-1960's towards the implicit positivism of the quantitative theorists; the reaction reflected the advance of Phenomenology as an alternative. In Psychology, the work of Lewin (1938) is often taken as the initiation of the phenomenological approach. "In a phenomenological perspective, the existence of an objective reality is denied and each individual is recognized as having a living world of experience within which decisions are made and which are reflected in, for example, man's model of nature and his tastes in landscape...As geographers have employed the phenomenological perspective, different cultural groups have been socialized to appreciate different aspects and components of the natural world, in whose image they create their own worlds, and the study of physical space can be pursued only through knowledge of these perceptions, just as the study of behaviour in social space can only be comprehended in the light of people's ideas about society" (Herbert and Johnson 1978, p.16).

Macro-Social Structuralism

"By the early 1970s, both the quantitative-theoretical and the behavioural approaches were coming under attack - on new grounds. This development, termed 'radical' and 'structural' by some (Robson 1976), with origins in the civil rights and anti-Vietnam movements in the United States in the late 1960's" stressed

1) The quantitative-theoretical methodology is grounded in a model of 'market capitalism' - but a major trend in recent society has been towards monopolies.

2) Consensus is inadequate for capitalist societies - conflict is equally important in its analysis.

3) Quantitative methodology is able at best "only to describe patterns, and its predictive powers for planning and policy purposes produce solutions which will continue the current situation...Given that most of the social and economic problems of the world are fundamentally a consequence of inequalities in power at all spatial scales...their solution will only come about through recognition of this fact, followed by action to remove the inequalities and not merely to patch up the associated ills"

4) "The behavioural approach...sees the individual as an independent decision-maker and not one whose actions are very much constrained by the institutional nature of his society, be it capitalist or socialist, primitive or advanced. Behaviouralism in its extreme form would take us back to the idiographic, exceptionalist stances which have proved to be inhibiting to theory formation in human geography in the past"

"The thrust of this critique is often clearly political" and interestingly, a striking convert and propagandist was formerly a leader of the New Geography, David Harvey (Herbert and Johnson 1978, pp.17-18).

Structuralism, as it was passing rapidly through the Humanities, was essentially concerned to argue a purer point of view, that: "all mature experience and knowledge possess a universal, necessary structure, and that this structure is derived, not from the empirical properties of the 'external' world of objects towards which experience is directed and about which knowledge is claimed, but from the manner in which human thinkers impose order on their own 'internal' world of perception and thought" (Toulmin 1972, quoted in Harrison and Livingstone 1982, p.3).

Structuralism may then be used either as a philosophical underpinning to Behaviourism, stressing the individual perception of the world, or in the 'radical' and often Marxist interpretation where Man is seen as dominated by 'structures' around him eg social formations, the capitalist Mode of Production and other group social forces. In any case, Geography has proceeded to pursue these separate lines of analysis very fruitfully, whether emphasizing the individual's perception of the world around him, or that of the specific community rooted in time, place and culture.

In History, the same reaction has occurred against the New History of the 1960's (Tilly 1984). A doyen of the New History, Stone, by 1979 had lost his zeal for the new ways, and wrote welcoming a "revival of narrative". He argued that events had come back into style, as the techniques and determinism that had captured the historians of the 1960's began to lose their appeal: "Many historians now believe that the culture of the group, and even the will of the individual, are potentially at least as important causal agents of growth as the impersonal forces of material output and demographic growth" (Stone 1979, p.9). The New History floated on "heady optimism...buttressed by the belief that material conditions such as changes in the relationship between population and food supply, changes in the means of production and class conflict, were the driving forces in history. Many regarded intellectual, cultural, religious, psychological, legal, even political, developments as mere epiphenomena" (Stone op.cit. p.7). Casting particular scorn on quantitative studies, he goes on to attack those who "specialize in the assembling of vast quantities of data by teams of assistants, the use of the electronic computer to process it all, and the application of highly sophisticated mathematical procedures to the results obtained...in general the sophistication of the methodology has tended to exceed the reliability of the data, while the usefulness of the results seems - up to a point - to be in inverse correlation to the mathematical complexity of the methodology and the grandiose scale of data-collection" (Stone op.cit. pp.11,13). Tilly (1984) shows how in Urban History, the 1960's was dominated by such typical trends as modelling, large data sets, multiple comparisons, quantitative analysis, but this then yields during the 1970's to studies that tried to "enrich their pallid collective biographies with colourings of individual experience" (p.376). He further points out that one of the major reactions to the alleged excesses of social-scientific history was 'retrospective ethnography', a "self-conscious turn to anthropology as a guide to historical reconstruction...The idea is to recreate crucial situations of the past as a thoughtful participant-observer would have experienced them" (Tilly op.cit. p.380).

It may be noted here that Marxist collective models are equally castigated with New History mechanistic models as straying from individual perceptive history.

The parallel is so obvious that I cannot resist at this point introducing the contemporary and identical debate in Modern Architecture. That very postwar boom whose consequences we have been following in academic thought began most directly with city replanning, and we are all by now familiar with the great impetus this gave to the Modern Movement in Architecture, the landscape of tower blocks, urban glasshouse skyscrapers, drawing board fantasies where the individual and the local community were lost to sight and to each other. The same philosophy of mechanistic gross modelling, simple law-like solutions, and the celebration of technological wizardry, permeate these all too real townscapes. And just as inevitably has come the reaction: the literal demolition of much of this landscape, the halt called to the implantation of additional glass monsters in historic contexts, and the thorough questioning of the philosophical and moral failings of the associated Modernist paradigm.

For many architects, financial considerations have led to a minimal attempt to reintroduce traditional points of cultural identification into their new blocks: this 'post-Modern Classicism' as seen flashily in Johnsons's AT and T Building in New York, is seen by many as in no respect different from the 'suppressive corporation of High-Tech': "It is a long time since architects believed that they could revolutionise society through their buildings. But to assume that architecture has no effect at all on social affairs is just as absurd. The advocates of both High-Tech and Post-Modern Classicism postulate that their architectures are politically value-free. In fact such approaches, with their celebration of corporatism and the status quo make very specific political statements" (Darley and Davey 1983, pp.23,25).

Amongst various contrasting alternatives that are now being built as rejections of Modernism, there is an obvious Revivalist Movement, as a result of which much new buiding on a private and to a lesser extent public basis is a direct imitation of historic styles familiar to the public and on a more manageable perceptive scale. More original is a school attempting to design new buildings that nonetheless respond to the criticisms of Modernism: Romantic Pragmatism. "Romantic Pragmatism...celebrates the primacy of the individual and particular and, pragmatically, it responds to the exigencies of brief and site without introducing (unlike Classicism, pure, Neo, or Post-Modern) an irrelevant geometric discipline between programme and product...It suggests multifarious ways in which people who use buildings can identify with locality...The aim is an architecture which recognizes and enhances the life of the individual and group as well as the organisation" (Darley and Davey 1983, p.23).

And so, at last, to Archaeology. John Barrett's critique will raise some of the key issues, which will now appear quite familiar: "The New Archaeology embraced the ideas of science because, I suspect, most people regard scientific knowledge as the only credible kind of knowledge; this view of science is beginning to be challenged. The embrace with science was two-fold. Firstly, theories were established which were aimed at achieving a law-like status, in other words to become...applicable in all times and at all places. Many of these so called laws were very trivial and this aim has been slowly eroded; none the less certain high levels of generalisation are still aimed for. These generalisations concern the working and organisation of <u>systems</u>, such as states, urban centres or hunter-gatherer communities".

"Secondly, there have been developments in methodology, and here there have been very real advances, even if the overall aim of hypothesis testing has defied clear definition. In all this the idea of tracing specific historical developments has been put to one side...systems change, apparently, through such mysterious means as 'maladaption', 'positive feedback' and 'multiplier effects'. In all this people appear to act out the roles in which they have been cast...Let us make a quite essential distinction here, namely that between <u>behaviour</u> and <u>action</u>. I will characterise the former as humans responding to the requirements of the system to maintain that system: defining laws of human behaviour is the main concern of the New Archaeologist. Action...looks at the way individuals and groups actively construct and manipulate a social order. Action is about people at <u>specific</u> times and in <u>specific</u> social contexts, and long term change is the result of those conscious and unconscious actions. If we are interested in past human action then, I believe, we write history. To my mind this distinction between behaviour and action (with its broad moral and political implications) lies at the heart of the current debate in theoretical archaeology" (Barrett 1983, p.189). And to this we may add that even the founder of New Archaeology, Lewis

Binford, could be said to have fled from his (1968a) search for 'general laws' of human behaviour, and moved down to 'archaeological problems for archaeologists' in his (1977) Middle Range Theory. "His distinction between middle-range theory, which supplies archaeologists with behavioral information, and general theories, that seek to explain cultural change...is of great practical importance because it distinguishes theoretical problems that are of particular interest only to archaeologists from those which are of general interest to the social sciences" (Trigger 1984, p.276).

Colin Renfrew, after a long espousal of characteristic New Archaeology approaches such as Systems Theory and mathematical modelling, has clearly shifted his ground to acknowledge the need for 'An Archaeology of the Mind' (Renfrew 1982) or 'Cognitive Archaeology'. It is apparently the states of mind of human participants that must now be allowed for alongside the usual New Archaeology large-scale, impersonal variables of the culture system: "now we are beginning to see very much more clearly that in the process of development of new, more complex social formations, including early states and civilizations, there are important cognitive factors accompanying the demographic, economic and social changes, without which these can hardly be explained" (op.cit. p.25). Both Renfrew (1983) and Wagstaff(1983) have also pointed to the common ground of Archaeology and Geography in post-positivistic soul-searching, tentatively indicating the potential for future collaboration on themes more recently favoured in Geography such as behaviourist/cognitive approaches and 'idealist'/socially-committed approaches.

Easily the most thorough-going critique of New Archaeology has come from Ian Hodder, in setting out an alternative paradigm of 'Symbolic and Structural Archaeology' (Hodder 1982a). He challenges the Systems Approach, reintroduces the individual, finds quantification totally inadequate for explanation, and focusses instead on the structures of the individual and community mind - considerably adapting Structuralist thought to this purpose. The future aim of Archaeology should be more 'historic', ie rediscovering the unique attitudes of mind and belief that belonged to particular cultures at particular epochs:

"functionalism refers to the use of an organic analogy and to the viewpoint that an adequate explanation of a past society involves reference to a system, equilibrium and adaptation...processual and systems archaeology is almost by definition a functionalist archaeology" (p.2).

"The evolutionary perspective has employed adaptive relationships at different levels of complexity, but it has not encouraged an examination of the particular historic context...The uniqueness of cultures and historical sequences must be recognised...Another limitation of the functionalist perspective of the New Archaeology is the relationship between the individual and society...Individual human beings become little more than the means to achieve the needs of society...In fact..individuals are not simply instruments in some orchestrated game and it is difficult to see how subsystems and roles can have 'goals' of their own" (pp.3-5).

"Levels of probabilility and statistical evidence of correlation are no substitute for an understanding of causal links and of the relevant context for human action. The use of mathematical and statistical formulae which provide good fits to archaeological data leads to little understanding of the past" (p.5).

"Material culture can be examined as a structured set of differences. This structural symbolising behaviour has functional utility, and it must be understood in those terms. But it also has a logic of its own which is not chiefly observable as pattern or style. The structure must be interpreted as having existed partly independent of the observable data, having generated and produced those data" (p.7).

Matching the 'Macro-Social Structuralism' of Geography, there has been a significant development in archaeological theory towards explanations emphasizing exploitation and class struggle in the prehistory and history of pre-Industrial communities (cf.Gilman 1981, Chapman 1979, Bintliff 1982, 1984 pp.26,173-174). This trend almost certainly reflects a direct influence from anti-Establishment tendencies in contemporary society on archaeologists rather than influence from related academic areas such as Geography, and may be considered as in part a response to the linked phenomena of economic recession and a wave of (government-supported) anti-liberal, anti-socialist activity in the Western world:

"it could be said that the rise of New Archaeology correlated with the 'anti-authoritarianism' of '68, but this correlation can hardly be ascribed any direct causal significance. The new Archaeology never took up the other aspect of the '68 movement: the social, and political critique."

"It is symptomatic that while major parts of the progressive elements within social and humanistic sciences discussed Marx-inspired theories, then at the same time 'progressive' archaeologists discussed mechanical evolutionary theories, inspired by Service and Sahlins. This is perhaps one of the best indicators of archaeology's academic isolation nowadays. A foundation in historical materialism would have implied a useful evaluation of the institutional and ideological content of archaeology, and its context" (Mahler, Paludan-Muller and Hansen, 1984, p.217).

In summary, current trends throughout the Humanities represent a rejection of what we might call postwar Modernism. The assumptions under challenge include:

1) Study of, and manipulation of, human communities in the present or past are best done at the aggregate, impersonal level, because
2) Forces at work in the past, and to be played with in the present, are most potent at the community level and are those most amenable to statistical treatment eg demography, modes of production, economic performance
3) Since data about the past is fragmentary, the only explanatory factors we can control are those operative on the large scale, as the Logical Positivist philosophy requires empirical testing for serious attempts at interpretation and causation.

If one can risk a sociological interpretion, this trend has occurred at a time of acute alienation of the public from government and public bureaucracy, with the latter encouraging a focus on self-promotion; at the same time economic depression and the activities of anti-Establishment pressure groups in areas such as ecology, feminism, ethnic minorities have reinforced feelings towards local community and the diversity of subgroups in society. The priority of the individual and the subculture, of creating one's own future, seems reflected in contemporary 'post-Modernist' approaches.

PROSPECTS FOR PROGRESS

It is more than timely to confirm that the earlier experience of our sister disciplines in such a post-Modern crisis can offer the prospect of ways out of our current difficulties and crisis of confidence.

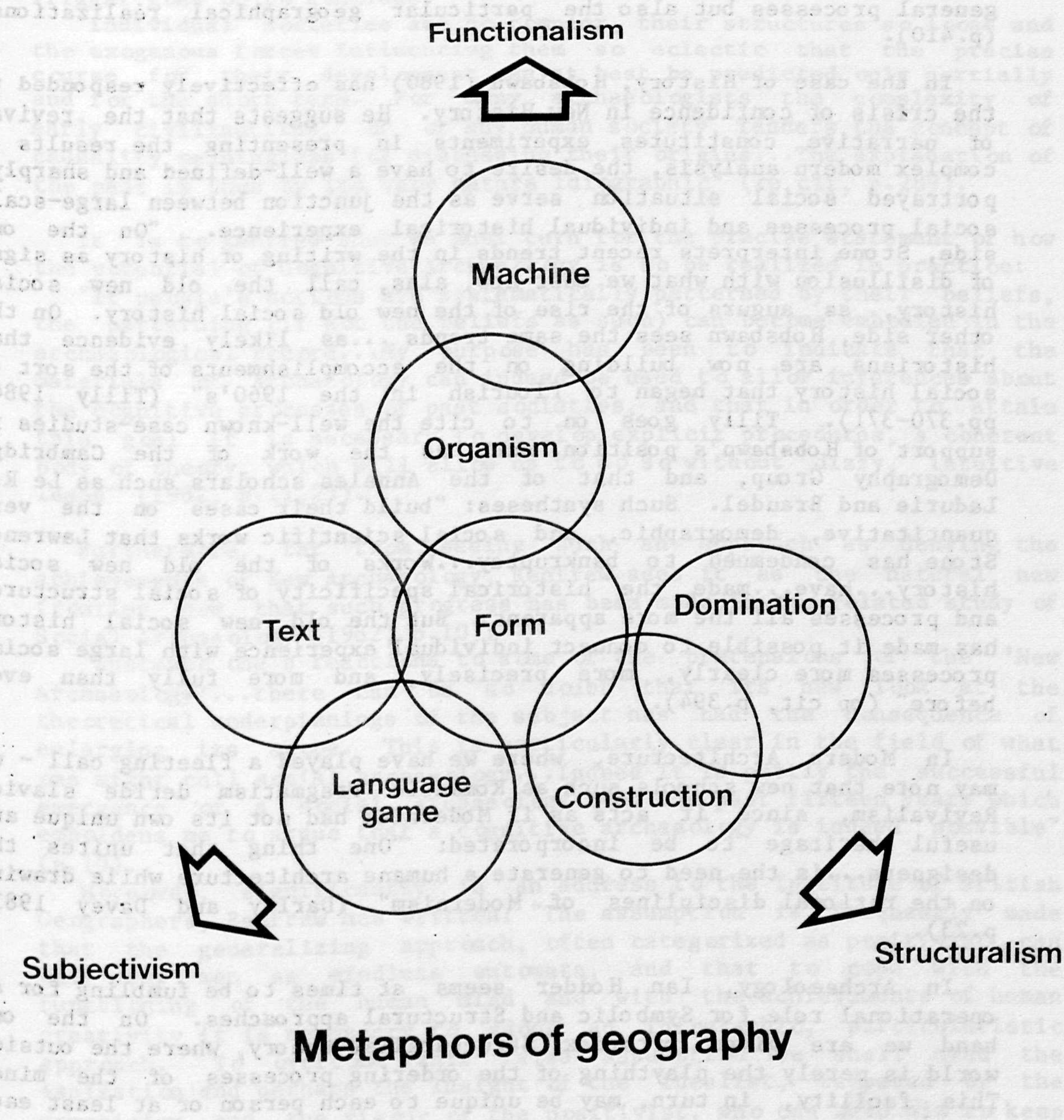
In Geography, for example, the Behaviourist appeal to the power of individual perception soon had to come to terms with collective realities and the unavoidable advantages of New Geography in methods in analysis: "the urban studies which adopted the behavioural approach neither focussed on the activities of particular individuals nor adopted the more discursive, literary style which was typical of geographical phenomenology. The concerns of urban geography - where people shop, work, live, or move to, etc - involved millions of decisions, many of them made daily, which even when sampled required the quantitative arsenal of the earlier methodology in order to find spatial order, the morphological laws, and the laws of association. Thus Gould's mental map studies are based on components and regression analysis and on an inferred spatial order" (Herbert and Johnston 1978, p.17). In fact, the recent wind of change has merely broadened the range of models and operational conditions of geographers: "Urban geographers are more sophisticated in the quantitative methodologies which they employ; a basic statistical competence is almost universal, but at the same time a healthy, informed, and mature scepticism of the proper role of quantification is now evident" (op cit, p.20). Interestingly, it is suggested that this broadening of Geography's perspectives, especially as regards the Social Sciences, may lead to the end of disciplinary boundaries and the corresponding time-lags for intellectual waves of innovation:
"As Harvey (1973) and others now argue, disciplinary boundaries within the social sciences are largely obfuscatory. Society must be studies as a whole, and its use of space, which is the main focus of geographical endeavour, cannot be studied independently or be the source of independent theory."

"On a broader front, urban geographers have also become more aware of the existence of alternative methodologies and philosophies and have shown a willingness to delve into the literature of the evolution of thought in the social sciences. New concepts rarely have an internal relevance only, and the waves of innovations which have repercussions throughout the literature are now more easily identified and incorporated into urban geography. It is unlikely that time lags of the scale which preceded the arrival of innovations from the earlier part of this century will be replicated. There is also a greater acceptance of the fact that an investigation of the human condition cannot be ideologically neutral" (op cit, p.20).

In general, these authors envisage a fruitful collaboration between the generalising, model-building and numerate approach and the most recent concern with the individual and collective perceptions (cf Figure 2): "One of the problems with the behavioural perspective has been its reductionism. Having returned to the individual as the unit, how then does the geographer return to his appropriate level of generalisation? The aggregate is the appropriate scale, generalisations are the aims; behavioural input allows these generalisations to be reached from a better qualitative base...One possibility, already exploited by urban geographers, is that of using the aggregate statistics as a framework from which to develop behavioural approaches" (op cit, p.24).

Harrison and Livingstone comment: "Under the panoply of 'humanism' the various attempts to reinstate Man as a knowing subject in encounter with an object constitute a confused melange of subjective

Figure 2 (From Harrison and Livingstone 1982, fig.1-4)



geographies...little more than introspective esotericism, lacking a cohesive framework for substantive research" (1982, p.2). In fact, synthesis to the aggregate level is possible, because "understanding is never purely individual but communal" (op cit, p.30).

Johnston (1980) presents a noteworthy compromise to bring the real potential of the new approaches into a framework of progress in the discipline of Geography: "The case presented here, therefore, is that human geography is the study of particular realizations of general processes, and that it needs to develop an arsenal of methodological procedures which allows it to explain those particualr realizations...it seems didactically necessary to explain not only the general processes but also the particular geographical realizations" (p.410).

In the case of History, Hobsbawn (1980) has effectively responded to the crisis of confidence in New History. He suggests that the revival of narrative constitutes experiments in presenting the results of complex modern analysis, the desire to have a well-defined and sharply-portrayed social situation serve as the junction between large-scale social processes and individual historical experience. "On the one side, Stone interprets recent trends in the writing of history as signs of disillusion with what we must now, alas, call the old new social history, as augurs of the rise of the new old social history. On the other side, Hobsbawn sees the same trends ...as likely evidence that historians are now building on the accomplishments of the sort of social history that began to flourish in the 1960's" (Tilly 1984, pp.370-371). Tilly goes on to cite the well-known case-studies in support of Hobsbawn's position, such as the work of the Cambridge Demography Group, and that of the Annales scholars such as Le Roy Ladurie and Braudel. Such syntheses: "build their cases on the very quantitative, demographic, and social scientific works that Lawrence Stone has condemned to bankruptcy...Works of the old new social history...have...made the historical specificity of social structures and processes all the more apparent. But the old new social history has made it possible to connect individual experience with large social processes more clearly, more precisely, and more fully than ever before" (op cit, p.394).

In Modern Architecture, where we have played a fleeting call - we may note that new schools such as Romantic Pragmatism deride slavish Revivalism, since it acts as if Modernism had not its own unique and useful heritage to be incorporated: "One thing that unites the designers...is the need to generate a humane architecture while drawing on the rational disciplines of Modernism" (Darley and Davey 1983, p.23).

In Archaeology, Ian Hodder seems at times to be fumbling for an operational role for Symbolic and Structural approaches. On the one hand we are given orthodox Structuralist theory, where the outside world is merely the plaything of the ordering processes of the mind. This facility, in turn, may be unique to each person or at least each culture, in the pattern it creates. How then can one hope to infer or read such vital organising principles? In effect, we must read between the lines of Structuralist Archaeology and pick up certain notable contradictions to predict a solution. Ethnographic parallels are the clues in Hodder's book, a practice of course entirely dependent on a trust in general principles found in widely differing cultures. We are already moving towards collective rather than unique, individual patterns. Thus Hodder (1982a) has to admit for his edited volume: "the approaches developed by the majority of the contributors of this volume are not structuralist" (p.9), and indeed: "The chapters by Tilley and Shanks, Shennan and Hodder...use generalisations from ethnographic and anthropological studies to link Neolithic megalithic ritual into other aspects of archaeological evidence" (p.11). Although Hodder also toys with the possibility of unreadable, or at least historically unique patterns, it is left to other observers such as Trigger (1984, pp.282,292) to suggest that on that route we find ourselves back where we started from, with Hawkes warning archaeologists off the mysterious and unattackable worlds of social and belief systems in the past. And Trigger goes on to conclude:

"individual societies are so complex, their structures so loose and the exogenous forces influencing them so eclectic that the precise course for their development can at best be predicted only partially and for the short term. For many archaeologists the complexity of early civilisations, or of any human society, renders the concept of causality meaningless for discussing their origins...The explanation of the past is thus by its very nature idiographic" (op cit, p.289).

It is to Renfrew that we must turn for the precise statement of how the potential of Cognitive Archaeology is to be realized in practice:

"if people's actions are systematically patterned by their beliefs, the patterning (if not the beliefs as such) can become embodied in the archaeological record...My purpose has been to indicate that the materials of archaeology can indeed be used to allow inferences about the cognitive processes of past societies, and that in order to attain this goal it is necessary to develop explicit procedures, a coherent body of theory, which will allow us to do so without dizzy, intuitive leaps" (1982, p.11,23).

Furthermore, far from seeing such an approach as denying the achievements of New Archaeology, Renfrew sees it as the natural new frontier now that such progress has been made in the related study of Social Archaeology (1982, p.10):

"Whatever one's reactions to some of the pretensions of the 'New Archaeology'...there can be no doubt that its new look at the theoretical underpinnings of the subject has had the consequence of enlarging its scope. This is particularly clear in the field of what one might call social archaeology...Indeed it is partly the successful emergence of a social archaeology over the past fifteen years which emboldens me to argue that a cognitive archaeology is indeed possible" (p.10).

Moreover, significantly in an address to the Institute of British Geographers, Renfrew has written: "The assumption is frequently made that the generalizing approach, often categorized as positivist, can only view men as mindless automata, and that to cope with the functioning of the human mind and with the achievements of human creativity it is necessary to adopt an idiographic, particularistic approach. To overcome this false assumption we shall need the aspiration and perhaps the insight of the idealist, tempered by the scepticism and the rigour of the positivist, who can help him to keep his feet on the ground"

"...geography, like processual archaeology, has in recent years often overlooked the importance of human thought and action, and of that uniquely human ability to create and use symbols. The current trend in both subjects to seek ways of dealing rigorously with matters that sometimes seem largely subjective is thus of the greatest interest, and probably fundamental to their effective development" (Renfrew 1981, pp.275-276).

This brief survey of Archaeology and related subjects has, I hope, not only revealed a way forward that is far from revivalist, but also exposed for discussion some highly intriguing opportunities for the future of our discipline.

1) It is hardly possible to consider, despite the hopes of traditionalists, that Archaeology will reject such central emphases of the New Archaeology as: the testing of propositions, problem-orientation in fieldwork, quantitative methods wherever possible, the search for generalisations about our past - in order to go back to imaginative intuition, inadequate data bases, descriptions of endless unique events and fieldwork as an end in itself or to provide more data for 'what happened next' or 'how it was to live then' scenarios of ever increasing detail.

2) Cognitive Archaeology is here to stay, but nothing will be achieved by it unless it adopts the explicit, scientific procedures of the New Archaeology. Nor will it offer much of interest unless it identifies collective, rather than individually unique, structural and symbolic representations. Probably it will also fail to contribute to our understanding of the past in any major way unless it merges the cultural specific to general and recurrent aspects or processes in human life in the past (and here the essential role of ethnoarchaeology suggests an implicit reality to this request).

3) A maturer discipline combining research on the specific with that of the general, or inner experience and perception with external 'realities', is possible, as recent developments in Geography and History can show.

4) The isolation of Archaeology from other disciplines has meant infrequent and sluggish flows of innovations into the discipline, and a minimal role for Archaeology's achievements to feed back out of our subject. As is being suggested for Geography, it should be encouraged that Archaeologists integrate their concern with Man, Society and Environment into a general Social Science perspective which would increasingly ignore disciplinary boundaries. The effects of such a shift of emphasis, beginning merely with Archaeologists regularly reading key journals in sister disciplines, would be momentous on the rate of evolution of Archaeology as a discipline.

BIBLIOGRAPHY

Barker, P.A., 1977 - Techniques of Archaeological Excavation. Batsford, London.

Barrett, J.C., 1983 - "Goodbye, New Archaeology?" Current Archaeology No.89, Vol.7(6), pp.188-189.

Binford, L.R., 1968a - "Archaeological perspectives", pp.5-32 in S.R. and L.R.Binford (Eds) New Perspectives in Archaeology. Aldine Press, Chicago.

Binford, L.R., 1977 - "General Introduction", pp.1-10 in L.R.Binford (Ed) For Theory Building in Archaeology. Academic Press, New York.

Binford, S.R. and L.R. (Eds), 1968 - New Perspectives in Archaeology. Aldine Press, Chicago.

Bintliff,J.L., 1982 - "Settlement patterns, land tenure and social structure: a diachronic model", pp.106-111 in C.Renfrew and S.Shennan (Eds) Ranking, Resource and Exchange. Cambridge University Press, Cambridge.

Bintliff,J.L., 1983 - "A matter of debate", Popular Archaeology June 1983, pp.44-45.

Bintliff,J.L. (Ed), 1984 - European Social Evolution: Archaeological Perspectives. University of Bradford, Bradford.

Chapman,R.W., 1979 - Comments to "The Social anthropology of a Neolithic cemetery in the Netherlands", by P.Van de Velde, Current Anthropology Vol.20, pp.35-58.

Chorley,R.J. and Haggett,P. (Eds), 1967 - Models in Geography. Methuen and Co., London.

Clarke,D.L., 1968 - Analytical Archaeology. Methuen and Co., London.

Clarke,D.L. (Ed), 1972 - Models in Archaeology. Methuen and Co., London.

Clarke,D.L., 1972a - "Models and paradigms in archaeology", pp.1-60 in D.L.Clarke (Ed) Models in Archaeology. Methuen and Co., London.

Coles,J., 1972 - Field Archaeology in Britain. Methuen and Co., London.

Collins,D. (Ed), 1975 - The Origins of Europe. Allen and Unwin, London.

Darley,G. and Davey,P., 1983 - "Sense and Sensibility" The Architectural Review Vol.174, No.1039, pp.22-25.

Dunnell,R.C., 1984 - "The Americanist Literature for 1983: A Year of Contrasts and Challenges" American Journal of Archaeology Vol.88, pp.489-513.

Edwards,I.E.S., 1985 - The Pyramids of Egypt. Revised edition. Penguin Books, London.

Frere,S., 1978 - Britannia. A History of Roman Britain. Revised Edition. Routledge and Kegan Paul, London.

Gilman,A., 1981 - "The development of social stratification in Bronze Age Europe" Current Anthropology Vol.22, pp.1-23.

Grano,O., 1981 - "External Influence and Internal Change", pp.17-36 in D.R.Stoddart (Ed) Geography, Ideology and Social Concern. Blackwells, Oxford.

Harrison,R.T. and Livingstone,D.N., 1982 - "Understanding in Geography: Structuring the Subjective", pp.1-39 in D.T.Herbert and R.J.Johnston (Eds) Geography and the Urban Environment. Progress in Research and Applications, Vol.5. John Wiley, Chichester/New York.

Harvey,D., 1969 - Explanation in Geography. Edward Arnold, London.

Harvey,D., 1973 - Social Justice and the City. Edward Arnold, London.

Herbert,D.T. and Johnston,R.J., 1978 - "Geography and the Urban Environment", pp.1-33 in Herbert and Johnston (Eds) Geography and the Urban Environment. Progress in Research and Applications, Vol.1. John Wiley, Chichester/New York.

Hobsbawm,E.J., 1980 - "The Revival of Narrative: Some Comments" Past and Present No.86, pp.3-8.

Hodder,I. (Ed), 1982 - Symbolic and Structural Archaeology. Cambridge University Press, Cambridge.

Hodder,I., 1982a - "Theoretical Archaeology: A reactionary view", pp.1-16 in I.Hodder (Ed) Symbolic and Structural Archaeology. Cambridge University Press, Cambridge.

Hogarth,A.C., 1972 - "Common-Sense in Archaeology" Antiquity Vol.46, pp.301-304.

Johnston,R.J., 1980 - "On the nature of explanation in human geography" Trans.Inst.Br.Geogr. N.S. Vol.5, pp.402-412.

King,T.F., 1983 - "Professional Responsibility in Public Archaeology" Ann.Rev.Anthrop. Vol.12, pp.143-164.

Kuhn,T.S., 1962 - The Structure of Scientific Revolutions. University of Chicago Press, Chicago.

Lewin,K., 1938 - The Conceptual Representation and Measurement of Psychological Forces. Duke University Press, Durham, N.Carolina.

Lipe,W.D., 1974 - "A conservation model for American Archaeology" Kiva Vol.39, pp.213-245.

Mahler,D.L., Paludan-Muller,C. and Hansen,S.S., 1984 - "Danish Archaeology in the 1980's - beyond theoretical poverty?" Journal of Danish Archaeology Vol.3, pp.215-217.

Musson,C., 1974 - "Rescue Digging all the Time", pp.79-89 in P.A.Rahtz (Ed) Rescue Archaeology. Penguin Books, London.

Rahtz,P.A., 1974 - "Rescue digging past and present", pp.53-72 in P.A.Rahtz (Ed) Rescue Archaeology. Penguin Books, London.

Rahtz,P.A., 1981 - The New Medieval Archaeology. Inaugural lecture. University of York.

Renfrew,A.C., 1972 - The Emergence of Civilisation: the Cyclades and the Aegean in the Third Millennium BC. Methuen and Co., London.

Renfrew,A.C., 1980 - "The Great Tradition versus the Great Divide: Archaeology as Anthropology" American Journal of Archaeology Vol.84, pp.287-298.

Renfrew,A.C., 1981 - "Space, time and man" Trans.Inst.Br.Geogr. N.S. Vol.6, pp.258-278.

Renfrew,A.C., 1982 - Towards an Archaeology of Mind. Inaugural lecture. Cambridge University Press, Cambridge.

Renfrew, A.C., 1983 - "Geography, Archaeology and Environment. I. Archaeology" The Geographical Journal Vol.149, pp.316-323.

Robson, B.T., 1976 - Editorial introduction Trans., Inst.Br.Geogr. N.S. Vol.1, p.1.

Samuel, R., 1984 - "Forum" History Today, May 1984, pp.6-9.

Sauer, C.O., 1963 - Land and Life. A Selection from the Writings of Carl Ortwin Sauer, edited by J.Leighly. University of California Press, Berkeley and Los Angeles.

Selkirk, A., 1983 - Editorial Current Archaeology 86, Vol.7/3, p.68.

Snodgrass, A.M., 1985 - "The New Archaeology and the Classical Archaeologist" American Journal of Archaeology Vol.89, pp.31-37.

Stone, L., 1972 - "Prosopography", pp.107-140 in F.Gilbert and S.R.Graubard (Eds) Historical Studies Today, W.W.Norton, New York.

Stone, L., 1979 - "The Revival of Narrative: Reflections on a New Old History" Past and Present No.85, pp.3-24.

Stone, L. and J.C.F., 1972 - "Country Houses and Their Owners in Hertfordshire, 1540-1879", pp.56-123 in W.O.Aydelotte et al (Eds) The Dimensions of Quantitative Research in History. Princeton University Press, Princetown.

Tilly, C., 1984 - "The Old New Social History and the New Old Social History" Review Vol.VII/3, pp.363-406.

Toulmin, S., 1972 - Human Understanding, Vol.1. Oxford University Press, Oxford.

Trigger, B.G., 1984 - "Archaeology at the Crossroads: What's New?" Ann.Rev.Anthrop. Vol.13, pp.275-300.

Wagstaff, J.M., 1983 - "Geography, Archaeology and Environment. II. A Human Geographer's View" The Geographical Journal Vol.149, pp.323-325.

Webster, G., 1963 - Practical Archaeology. A. and C.Black, London.

Webster, G., 1974 - "Training the New Archaeologist", pp.235-240 in P.A.Rahtz (Ed) Rescue Archaeology. Penguin Books, London.

Willey, G.R. and Phillips, P., 1958 - Method and Theory in American Archaeology. University of Chicago Press, Chicago.

Wilson, D.M. (Ed), 1976 - The Archaeology of Anglo-Saxon England. Methuen and Co., London.

ARCHAEOLOGY AND HISTORY: THE SHADES OF CONFRONTATION AND COOPERATION

C.J.Arnold

> "It is in my view a cultural disaster of the English-speaking world that...we have no single word for academic research in all fields - a disaster because it tends to create division where none should exist."
> (Harvey 1983, p.74)

The division between archaeology and history (in the narrow sense) came about largely as a result of the study of written evidence being an older discipline and of archaeology originating within the prehistoric field (hence the confusion between 'archaeology' and 'prehistory')(Clarke 1978, pp.10-11). Classical historians' interest in the physical remains has a long ancestry, but the formation of a society specialising in the archaeology of post-Roman centuries, The Society for Medieval Archaeology, did not occur until 1956. While the age of the various disciplines may go some way toward explaining the divisions that exist, it does not explain why the integration of archaeological and written evidence occurs so rarely.

Archaeology and history, by whatever means they are defined, are concerned with the past dimension of anthropology. Past societies can only be studied through man's material remains, artefacts and the wide variety of means by which man has altered the earth's surface by relocating and reconstituting materials from it. In more specific terms it may include artefacts of various materials, coins, inscriptions and documents, human and animal bone, excrement, plant and insect remains, structures surviving in various forms including houses, storage facilities, military installations, administrative offices (including libraries and record offices), religious structures, places for rubbish disposal, graves, industrial activity. We may note in passing the varying degrees of protection, statutory or non-statutory, afforded to this wide variety of material remains. The data are studied by archaeologists who divide themselves into a variety of specialisations with specialist collaboration with other disciplines. There are some areas which, for historic reasons, have been parcelled off, especially numismatics, epigraphy and written sources. This peculiar division is found running through much of the literature concerned with historic periods, which serves to emphasise the deep-rooted belief that written records are distinct from other artefacts:-

> "As we are dealing with an historical period, it would obviously be ridiculous to disregard the documentary evidence entirely, and it is brought into the discussion as a necessary background, but the emphasis throughout will fall on the material evidence [my underlining] for the period." (Clarke 1984, p.9)

This view has been taken to extremes by some writers, not necessarily recent ones, who, for instance, have stated the belief that 'Archaeological data are historical documents in their own right, not illustrations to written texts' (quoted by Pyddoke 1964, p.16).

One result of the separation between archaeology and history (and also art history) is a tendency for archaeologists, to varying degrees, almost to ignore the results of studying inscriptions, coins and written records, except when they provide a means of obtaining calendrical dates; this is presumably a hangover in attitude from the pre-scientific dating era. It is a way of thinking which persists despite the fact that all artefacts (including documents) adhere to the same patterns; they consist of materials which are altered, shaped, reconstituted, embellished, inscribed, decorated and in varying ways transmit information. All, despite what we are sometimes led to believe, are bedevilled by differential survival and a variety of distorting factors considered by archaeologists under the heading of formation processes. Nothing is done to any particular artefact type which differs, except in detail, from the practices of research which are carried out on the others; there is always a concern with material, origin, technology, content, authenticity, context, function and meaning. Despite this there remains an ingrained belief that the problems faced by archaeologists in using the data they have chosen to concentrate on are different to those faced by the student of written records. Speaking of archaeology Sawyer has suggested (1983, p.46) that "All too often they [archaeologists' hypotheses] owe more to the assumptions than to the evidence" as though this is a problem peculiar to archaeology and not one shared by all disciplines concerned with past material remains.

Archaeologists have failed to view all surviving remains as artefacts (or 'society' has given greater attention to certain specialisations than others) to the same extent that they do, for instance, pottery; why are archaeologists concerned with the content, function and meaning of palaeolithic cave art but not of the works of Hogarth? They rarely assess societies in terms of the use of written documents so that when operating within an historic period, in which historians automatically operate, the archaeologists must consult an historian for detail. Should historic societies not be viewed in terms of the contexts in which documents are found, the origins of the documents, their content etc? There are nearly as few archaeologists who view coins in this manner. The suggestion that all of man's material remains should be considered in the same manner is by no means novel:-

"Archaeology can trace the same process [adaption of human societies to their environments] in historic times with the additional aid of written records. Without any change of method it can follow down to the present day the working out of trends discerned already in prehistory."
(Childe 1942, p.7)

All lines of study of man's past are concerned with both what happened and why and there should be a desire to use all sources of information to that end. We should not allow ourselves to be distracted by the varying degrees to which the data relate to different aspects of the past, with varying degrees of detail being obtainable. Written records can especially enlarge on political, legal and administrative areas of human activity, often in great detail, whereas the study of the remainder of man's material remains is concerned with a wide variety, nearly all, areas of past human activity; these include the same areas as those for which documents are a source, but to varying degrees of detail and by different routes. To say that there are areas of overlap between what are traditionally archaeological concerns and what is frequently contained in primary written sources, and that there is considerable scope for collaboration, indicates a particular belief about the subjects' relationship. The implication has been that they have different goals. It is only possible to study the past using all sources, but the data must be presented in forms by which they are compatible, and this applies equally to the specialisms of all types of artefact, soil features and standing structures.

There really would not be a problem, and it may seem odd to many readers that one exists, if it were not that some historians imply that what is contained within written sources is impenetrable to archaeologists and that they allow the only 'real' reconstruction of the past. This attitude has encouraged much research based on written records which is carried out in isolation of other material remains. In the words of one historian:-

"Archaeologists should concentrate on being archaeologists and resist the temptation to draw heavily on other types of evidence, historical or linguistic."
(Sawyer 1983, p.46)

and:-

"If archaeological evidence is fitted with the supposed historical situation, the result is obviously circular."
(ibid, p.47)

The contention here is that data gleaned from written sources is but one contribution which material remains can make to the study of the past, without denying that their study, like that of any aspect of past material remains, is a specialised task. The problem as this writer sees it, often reaches ludicrous levels:-

"There is a danger in their [medieval historians] regarding archaeological evidence simply as a source of background or illustrative material used to confirm historical statements which have already

been inferred from the documents. This in Sawyer's words reduces archaeology to 'an expensive way of telling us what we know already'. In fact, of course, there are myriad aspects which archaeology can illumine from the pre-literate origins of medieval society, the many gaps in the documentary record and in particular, the interests of the non-record making classes."
(Steane 1985, p.xv)

This statement perpetuates the view that history tells us a great deal about the past and that archaeology can fill in the gaps, with some overlap. Typically, it is often claimed that far more can be achieved by the study of written sources in isolation than is actually possible. The written sources can only inform us about what is mentioned in them, but this tends to be viewed as all-embracing because historians have laid down the range of subjects concerning the past which are appropriate for study; these are determined by the sources. Thus 'history' in the narrow sense is often 'History' in the general sense as if the only obtainable past is contained within documents. The alternative viewpoint is that the historian is a specialist of a particular type of artefact which can manifest itself in a variety of forms and materials collected together under one heading by the existence of various scripts - a heading which includes written and printed documents, inscriptions, to a lesser extent coins and to an even lesser extent inscriptions on pottery. Archaeologists attempt to use all material remains for as detailed a study of past human behaviour as is possible. They rely on specialists to deal with certain categories of material. They are guilty, however, of a tendency to ignore a major class of artefact, written records. This is in part because the specialists concerned with them naively believe they are creating a meaningful past by examining them largely in isolation, and because archaeologists rarely dig them up. But, as with all specialist fields, documents have to be integrated with all other forms of data. The problems of differential survival apply to all artefacts, not only those traditionally dealt with by historians. Generations have been brainwashed into believing that because documents have for long been studied by a separate group they are somehow different to the remainder of the past material and non-material record.

Documents can be considered in general terms to determine the manner in which past societies actually used them, and in more specific terms as a source of knowledge regarding aspects of human behaviour. But if these data are to be capable of integration with the knowledge gained from all other material remains, they must be comparable and there must be a desire to see a specialist topic in a broader context. Studies of specific types of artefact or of an aspect of types of artefacts are rarely comparable at the specific level and can only be integrated with each other at a level of abstraction of the raw data. Those few archaeologists of historic periods who are concerned with such manipulation and abstraction find it difficult to integrate their knowledge with that gained from written sources because the specialists of the latter are too often concerned with the detail and there is a widespread lack of concern with the implications of the evidence for the study of man. Particularism is rife amongst archaeologists of historic periods and historians of earlier historic periods. Hence the term 'history' is often applied to a very specific form of reconstruction of the past. There has to be a desire to achieve a level of generalisation where integration becomes possible. The petrologists must pursue their detailed research, but at the end of the day it may be, for instance, the source of the raw material that needs to be known to enable a generalisation to be made regarding pottery production and distribution mechanisms in a particular period which can then form a part of a broader understanding of that society. This applies to all artefacts.

Many writers piously state that integration is essential, but few explain how they think this may be achieved and why it hasn't been achieved previously. A reason why historians often believe that their data base is somehow of greater value than archaeological data stems from a misunderstanding of the goals of archaeology and the place of written sources in that quest. Medieval archaeologists only have themselves to blame as their work is often theoretically weak and particularistic; integration of archaeological data with the written sources is generally only at the detailed and trivial level of, for instance, linking the phasing of a castle on the basis of archaeological analysis with the known historical sequence. At present it is possible to identify three common standpoints. The first states that archaeologists and historians should pursue their own research independently. The second is that the evidence should be integrated but there is uncertainty about how this may be achieved. The third assumes that one discipline, normally archaeology, can illustrate the other. This writer has sympathy with the view that "medieval archaeology could have a major role, if it ceases to be particularistic, descriptive and historically-based" (Rahtz 1983, p.13) but to say that 'Medieval Archaeology' should develop as an autonomous discipline (ibid, p.12) is shortsighted. It is often stated that medieval archaeology is a new discipline going through the necessary phase of data-gathering. One may ask when, given that the questions we ask of the data are continually evolving, sufficient data will have been gathered for the next phase to come into operation, and also what is that next phase? It is also inexcusable (even if explained by historical archaeologists' particularism) that this data-gathering should be carried out in a vacuum with so little involvement in theoretical debates. It is almost as though Roman and Medieval archaeologists believe that written records are a substitute for a sound theoretical basis, that they are immune from theoretical problems and that the need for theory disappears when a chronological narrative is achievable (for what it is worth). This stems from an over-dependence on 'historical sequence' and a failure to realise that written records are no different to other material remains and make up a part of that data-base on which the theoretical debates are, or should be, based. It is interesting that the naivity regarding the power of written records is also displayed by prehistorians who look enviously over the fence in the belief that they can test their own theories within an historic period; the documentation is supposed to provide such an improved data-base as to make this feasible. What is being overlooked is that the problems faced by prehistorians in interpreting their data are the same as those in the historic periods, including the written records (see for example: Rahtz, 1983). The written records do not necessarily provide all the answers to the questions which prehistorians find difficult or impossible to approach, except at the most trivial level. Prehistorians are as dependent on material remains as are archaeologists of the historic period (in which category we include documents), and it is from the former group that the majority of the theoretical debates flow with a minimal involvement by the latter who shun theoretical issues partly by the excuse that they're still gathering data, but also because of an over-dependence on historical frameworks and because of their particularistic approach.

There are, albeit very few, examples where writers have successfully integrated generalisations based on all classes of artefact including written sources. There are others where attempts have been made to integrate the specifics of primary and secondary written sources with the specifics of the remainder of the material world; these are always methodologically unsound and their outcome is confused unless the archaeology is squeezed into the chosen historical framework.

Part of the problem has been discussed by one student of the past, who stated the belief that the problem lay in the lack of a theoretical framework especially for early historic periods and that:-

> "The framework we chose will be founded in our philosophic response to the problem of acquiring knowledge of the human past, particularly from material evidence, and in our opinion of what governs human behaviour."
> (Dickinson 1983, p.43)

This writer would not disagree with the view that theoretical frameworks are essential and eagerly waits for others to put their money where their typewriter is. But the statement assumes that there must be one correct theoretical framework and avoids the fundamental point that very few students of historic periods make use of the 'theoretical frameworks' that already exist; even fewer would agree that they are working towards an understanding of past human behaviour. The majority use an 'historical framework' whereby our whole understanding of the past has to be fitted to the very narrow and immeasurably partial view extrapolated from one type of artefact, the written sources. This is as ludicrous as attempting to relate the remainder of material remains to a framework of the past constructed on the basis of metal analyses. Yet such an approach is frequently found. Myres has written a "history book" in which only "the more distinctive and significant features" of the archaeological evidence are considered, in "broad outline...where it has been essential to the course of an historical argument" (1986, p.xxiv); that is, only that evidence which can be seen to conform to his historical narrative, often based on a rather literal interpretation of written sources which we know to be spurious in terms of their factual content.

Too often students working on the material remains of historic periods (including written evidence) are immersed in their own 'period' and there follows from this the absence of a desire to attempt to generalise; those that find it difficult to generalise about their data, or even to see any patterning in it might do well to question the whole basis for their initial ordering of the material. When theoretical concepts are introduced to an historic period by archaeologists the response is negative and the objections particularistic. It is as though many fear to leave the details in their proper place, that one's credibility will be diminished if one ceases to argue the details of individual artefacts or types of structures and use that knowledge to take a more generalised stance.

There is no denying the need to study detail; pottery fabrics are of as much concern as the content of medieval statutes, but an exchange between the specialists and the generalists is an essential dialogue if both areas are to develop. For the purposes of integration it may be sufficient to know that a society's socio-political development was such, and power was being used in a particular way, that laws were being made by X over Y at Z, which can often be determined as the result of analysis and generalisation of and about a variety of artefact types, not only those with writing on them; this has to be appreciated and admitted by all. Historians may claim that generalisation is impossible until consensus is reached and, like the medieval archaeologist, disappear once again under the mountain of detail; generalisation is an essential part of the route to that mythical consensus. It is often the absence of generalisation by historians that frustrates the early medieval archaeologists, but those generalisations that exist can be integrated very fruitfully with archaeological data (for a practical example and detailed discussion of the problems see: Arnold forth).

Communication is essential; archaeologists would have serious problems if petrologists claimed that their results couldn't be used because the archaeologists didn't understand how they were arrived at, and that when they were used the archaeologists ignored the problems. Levels of communication between archaeologists, specialists and generalists are such that there is an equal and fruitful flow of ideas and data between them. Specialists of artefacts with writing on them should be able to achieve the same goal, of presenting their data in a form which can be understood and with the problems, assumptions and limitations clearly stated for all to use. We would not expect a ceramicist to try and write the history of the world from pottery alone, although it is possible to write the world history of ceramics from which there would be a great deal of data for general understanding of social, technological and economic systems in society. The necessary level of generalisation is more easily obtained at some periods than others, depending on our appreciation of the reliability and the particular nature of the data. Thus it is as difficult to generalise about the earliest human behaviour as it is for the earliest written records when so little of the material is found in a primary context; it is only by the application of cross-cultural and theoretical models that the earliest hominid behaviour is beginning to come into focus. The very unreliability of the data at such times is informative about society and the environment in which it lived.

Specialists must continue with their research; none need, nor has any right to, believe that their specialisation is superior to any other; each must feed their data, observed patterns and generalisations to those who seek to take a more generalised overview of past human behaviour, an overview which must incorporate all types of evidence. Historians continue to enjoy a separate existence to the other specialists of past material remains because of the continuing misconception about the place of written records in the material past. Harvey may bemoan the absence of a word which encapsulates all academic research into the past; how easy it is for an historian to overlook the meaning of the word Archaeology.

BIBLIOGRAPHY

Arnold,C.J., forth. - "Territories and Leadership: frameworks for the study of emergent polities in early Anglo-Saxon southern England" in S.Driscoll and M.Nieke (Eds) Early Historic Archaeology. Edinburgh.

Childe,G., 1942 - What happened in History, Penguin Books, Harmondsworth.

Clarke,D.L., 1978 - Analytical Archaeology. Revised Edition. Methuen, London.

Clarke,H., 1984 - The Archaeology of Medieval England. British Museum Publications, London.

Dickinson,T.M., 1983 - "Anglo-Saxon Archaeology: twenty-five years on", pp.38-43, in Hinton 1983.

Harvey,P.D.A., 1983 - "English Archaeology after the Conquest: A historian's view", pp.74-82, in Hinton 1983.

Hinton,D.A.(Ed), 1983 - 25 Years of Medieval Archaeology. University of Sheffield, Sheffield.

Myres,J.N.L., 1986 - The English Settlements. Clarendon Press, Oxford.

Pyddoke,E., 1964 - What is Archaeology?. John Baker, London.

Rahtz,P., 1983 - "New approaches to Medieval Archaeology: Part 1", pp.12-23, in Hinton 1983.

Sawyer,P.H., 1983 - "English Archaeology before the Conquest: A historian's view", pp.44-47, in Hinton 1983.

Steane, J.M., 1985 - The Archaeology of Medieval England and Wales. Croom Helm, Beckenham.

WHY SHOULD HISTORIANS TAKE ARCHAEOLOGY SERIOUSLY?

J.A.Lloyd

There are excellent reasons why historians should take archaeologists seriously. Naturally, as Finley (1971, p.174) has pointed out, the value of the material remains as a source of evidence for history will diminish in proportion to the amount and quality of written documents available, but there is surely no past era whose physical heritage is of no historical interest and importance.

For the Graeco-Roman period, with which this paper is concerned, the surviving textual documents, including inscriptions, are very numerous. But the written record is quantitatively biased towards certain places at certain times (for example, Athens in the fifth and fourth centuries B.C.) and massive gaps exist. Archaeology, with its ability to aquire vast amounts of new data, is uniquely placed to expand our knowledge, especially in relation to economic and social history. This has long been recognised by some ancient historians, although extensive discussion of the interface is comparatively recent. Humphreys (1967), Frederiksen (1970) and Finley (1971, 1985) offer some of the most stimulating and constructive analyses of the possibilities. Snodgrass (1983) gives a rare, and by a long way the best, discussion by a classical archaeologist. All these works emphasise the common aims of archaeology and history and suggest that the relationship of the two disciplines, although beset by difficulties, is ultimately rewarding.

Nonetheless, the question which forms the title of this paper is still asked, in one form or another, by a substantial number of undergraduates, post-graduates and teaching staff in departments of Ancient History and Classics (where many ancient historians are located) at British universities. In research, and even more in teaching, archaeological evidence is often rejected, dismissed or, most frequently of all, ignored. The idea that history should simply employ all sources of evidence, written and material, to investigate the past (Finley 1985, p.26) seems to receive more lip-service than practical encouragement.

Why should this be so? This paper offers some reasons, adopting the hypothetical viewpoint of the sceptical historian. Although this has difficulties for an archaeologist, it is no bad thing to try to understand better the arguments of one's critics. An awareness of the concerns of the historian, derived from reading and especially discussion (here I acknowledge with gratitude the kindness and patience of my colleagues at Sheffield), has led in my own case to a much more informed appreciation of what archaeology can and cannot do in pursuit of the historic past.

Ancient historians have been aware of the value of archaeological evidence for a very long time. George Grote's remarkable ten volume History of Greece, the first volume of which was published in the 1840's, makes use of topographical research and the results of excavation (not much, admittedly, but comparatively little archaeological/antiquarian work had taken place in the Greek lands by the time that Grote was writing). A generation or so later the works of the great Roman historian, the German Theodor Mommsen, show his concern for the more rigorous scrutiny of the literary sources - the main body of evidence for the study of the Graeco-Roman period - and for serious attention to be paid to other kinds of evidence, especially inscriptions and coins.

In 1926 another major figure, the Russian Mikhail Rostovtzeff, published The Social and Economic History of the Roman Empire, the first of his classic surveys of the social and economic history of the ancient world. In the preface he wrote "for a student of economic and social life [the archaeological evidence] is as important and indispensable as the written evidence".

In 1980, Keith Hopkins, now Professor of Ancient History at Cambridge University, deployed a variety of material evidence, particularly shipwreck and coin data, in developing a theory of the relationship between taxation and trade in the Roman Empire (Hopkins 1980).

The list of Ancient historians who have used and who do use material evidence to investigate topographical, political, social, religious and economic questions could be expanded. They belong, with biblical and medieval historians, to a modern tradition whose origins lie in the first half of the nineteenth century if not earlier. This may come as a suprise to some archaeologists who seem to regard history and historians as concerned wholly with political events and personalities and interested only in the archaeology of war (Troy) or famous men and women, preferably royalty (Boudicca). (It is true, however, that in textbooks for schools and junior undergraduates professional historians have predominantly placed emphasis on the political framework; and that a political narrative is usually the basis of introductory university courses in ancient history). A more acceptable definition of what historians regard as their field of enquiry might be something like 'the study and understanding of the past, for which no kind of evidence is excluded'.

But what of archaeologists? To what extent, if at all, have they been concerned that historians should take notice of their discoveries and their interpretations of them? There seems to me to be strong evidence that most archaeologists (apart from those who reject any dialogue with history) view their work as a contribution to historical enquiry (including here prehistory) in the sense just defined. The Aegean archaeologists Popham and Sackett (1980, pp.355-369) close the first volume of the Lefkandi excavation report with a section entitled "Historical Conclusions". The early medieval archaeologist Richard Hodges stated in the editorial introduction to the papers of the second (British) conference on Italian archaeology that "one aim of the conference was to blow some fresh air through the corridors of our ancient history/archaeology departments" (Barker and Hodges 1981, p.5). The request for attention is clear. John F. Cherry, discussing the emergence of the state in the prehistoric Aegean, remarked "Whether one agrees or not, it remains true that, even for the historical states of the Classical age, the question of origins is one in which the mute archaeological evidence weighs heavily. This should mean that many of the opportunities, problems and biases to be faced in studying the rise of early poleis apply with equal force to the prehistoric world and vice versa." (Cherry 1984, p.18).

In the light of this, why is it the case today that if we were to enter a library devoted to Greek and Roman history we would find that the great majority of books pay little or no attention to material evidence? Why do so few school or university courses in ancient history prescribe an archaeological element? Why are students of ancient history at our universities, with very few exceptions, not required to develop skills in archaeological method in the same way that they are required to display skills in textual evaluation? Why can the ancient historians in British universities who have acquired serious experience and expertise in modern field techniques be counted on the fingers of one hand? In other words, despite the existence of a receptive climate for well over a century, why have so few of the teaching and research resources of ancient history, at individual and institutional level, been devoted to archaeology?

From the historian's point of view, there are many reasons why the interface between the two disciplines is so narrow. They include the susceptibility of many archaeologists to what Snodgrass (1983, p.142) has called the "positivist fallacy of archaeology" - the notion that what can be seen must be important - which has led, amongst other absurdities, to a grotesque emphasis on the commercial significance of the pottery trade; the over-ambition and myopia of some of archaeology's leading spokesmen - Binford's (1962, p.220) claim that "archaeology can present a systematic and understandable picture of the total extinct [his italics] cultural system" is meat and drink to historians inclined to be suspicious of archaeology; Hopkins (1978a, p.71; 1980, p.104) has castigated the "hopelessly fragmented" nature of archaeological reports and the reluctance of archaeologists to write synthetic works. And so on. Sociological and psychological factors may also come into play. For example, I suspect that historians and archaeologists do not in general share the same attitude to manual labour.

The remainder of this paper deals, through case studies, with some of the more intellectually challenging of the historian's criticisms.

Although archaeology can in theory inform us about a huge range of past activity, the historian might argue: in practice the nature of material evidence and the typical state of its preservation make it very unlikely that its contribution to many fundamental questions about the past will ever be more than marginal. If one is interested in, say, politics, law, administration or an institution like slavery, archaeology has yet to demonstrate that it can make a significant impact upon interpretations based on the written record. This is not to deny that such matters have an archaeological dimension but it is essential to keep the amount and quality of the written and material data in perspective, both in research and perhaps even more in teaching. Cartledge (1979, p.9) sums up these reservations: "From archaeological evidence alone we may infer (relatively) much about material techniques, a considerable amount about patterns of subsistence and utilization of the environment, far less about social and political events, and least of all about mental structures, religious and other 'spiritual' ideas and beliefs".

Is this overly pessimistic? Archaeologists might point out in reply that their work can make an enormous difference too, for example, our knowledge of political institutions. In the case of the eastern Mediterranean, at numerous cities of Asia Minor and the Middle East (for instance, Priene, Hierapolis, Gerash), excavation has brought to light important evidence for urban political organisation. The evidence takes the form of inscriptions, usually carved on the seats of a theatre or theatre-like building, which name the voting tribes of the council or assembly of the city. The buildings seem to have served as the meeting places of these bodies.

The historian might object that the inscriptions are crucial to this identification and that writing is not classed as archaeological evidence, since it can be studied effectively independently of archaeological context. This has some truth. Historians have certainly been content to decipher epigraphic material in isolation of its original physical setting. In fact, in most cases they have not had to, for obvious reasons. For the most part this has not seemed to make much difference to the informative value of the text (although readings can often be difficult and mistakes have frequently been made).

In the example cited, however, the fact that the inscriptions are <u>in situ</u> in well-preserved buildings substantially augments their intrinsic value. For example, there can be much more certainty about the number of tribes and the size of the voting body than if, say, the inscribed blocks had been found sporadically re-used in later structures. This precious contextual information can only be recovered archaeologically. (And one might add, the archaeolgical contribution would be even greater if there was ever a question of restoring a re-used inscription to its original place).

Most archaeological finds are of course uninscribed and cannot be explained in such precise terms as the buildings we have just discussed. Even when repeated patterns of finds occur, alternative and equally valid interpretations of their meaning may be possible. The ambiguity of much archaeological evidence is a serious difficulty for the historian. This is not to deny that there are often grave deficiencies, of many kinds, in textual evidence, but this has long been understood (by both Grote and Mommsen, for example) and methodologies for dealing with them have been much discussed. It is also much easier to be reasonably sure of meaning, the historian would argue, when the medium of communication is language rather than mute objects.

An earlier generation of archaeologists (e.g. Piggott 1959) was acutely aware of the limited range of inferences which could legitimately be drawn from the archaeological record alone. Accordingly it set itself modest targets in terms of questions about the past which it sought to address. In the last twenty years or so, however, many archaeologists have been concerned with expanding the horizons of the discipline. The cautious approach has given much ground to an energetic, adventurous spirit which is continually setting itself new goals of explanation (for example, Clarke 1968, Renfrew 1982, Cherry 1984). Some historians and archaeologists have mixed feelings about some of the developments (for example, Finley 1971; Cartledge 1979, pp.8-9; Alcock 1984) but there is no doubt that advances in archaeological theory and method have been stimulated by the pioneering atmosphere and the more ambitious questions which have characterised the period. These questions range widely, from large and complex political issues like peer polity interaction to details of trade like the sourcing of marble by isotopic analysis. Historians have obvious interest in such matters, indeed have often been investigating similar questions for a very long time, so there is in principle (and often in practice) a warm welcome for new approaches and new evidence. What often gives cause for concern, however, is the amount of confidence placed in what historians regard as inherently problematic evidece. The circumspection of the earlier generation is admired (Finley 1985, p.25) and there is a strong feeling that archaeologists should work harder to understand the weaknesses as well as the strengths of their discipline. The three examples which follow develop aspects of these issues and are all taken from publications which have appeared within the last ten years.

The first is a report of the excavation of a site called Tell el-Maskhuta in the Delta region of Egypt (Holladay et al 1982). In his account of the site's development (it was a place of modest importance) the principal author presents a table (Table 2) which lists possible correlations between documented external and internal events and site features. Thus the capture of Jerusalem in 597 BC is linked with two floorings for Building L.2002; the rebellion against Darius of 487-6 BC with the deliberate blocking of Well H.4002. All things, perhaps, are possible, but as Bailey (1985, p.55) remarked in his review of this report "Buildings are destroyed and rebuilt for many reasons, not all of them to do with invasion and response". Bailey added that this is a fine example of text-hindered archaeology, using a phrase that is much in vogue these days. But I wonder whether this phrase accurately describes the problem. 'Text-hindered' archaeology would seem to imply that written information can in some way impede the interpretation of material evidence. However, since archaeological finds and patterns of finds very rarely, if ever, explain themselves, surely the archaeologist ought to welcome the possibility that ancient documentary testimony might shed light on whatever is being studied? This would not, of course, take away the responsibility to evaluate that information rigorously, as any good historian would. In the Egyptian example, Table 2 overstretches the evidence, both written and material. This is just bad archaeology and bad history.

All archaeologists who carry out research into historic periods, however, will recognise the seductiveness of the written explanation or the attested event. Many, including myself, have been lured into faulty interpretation because of it, and Bailey is quite right to condemn the uncritical "constant striving to equate archaeological features with historical events" which is still widespread today. Only very, very rarely can physical remains be linked to a moment in history with a high degree of certainty, at least for the Graeco-Roman world. When it happens (Pompeii is one of the clearest examples) the advantages are very considerable, particularly for dating. But in the vast majority of cases there are strong technical objections to the marriage of material remains and recorded events and the temptation to unite them should be resisted.

My second example is taken from one of archaeology's recent growth areas, the identification and interpretation of animal bones. The American Steven Dyson has championed the alternative perspective which archaeology has brought to research into historic periods. "Since most of our literary texts look at society from the top down, it is important to look at it literally and figuratively from the ground" (Dyson 1979, p.12). What then more basic, more in touch with the lives of ordinary people than their food debris? And progress in the sphere of faunal studies has been tremendous in recent years. Bones once thrown away are now collected in ever greater numbers and with ever greater care. The finds from classical period sites are very numerous - 4,000 from the small 5th century AD Schola Praeconum deposit in Rome, 14,000 from Hellenistic and Roman levels at Berenice (Benghazi) in Libya, nearly 400,000 from the late Iron Age town of Manching in Bavaria (up to 1971). From these finds hypotheses about diet, about the organisation of agricultural production and about meat marketing can be made. This is of considerable interest to the ancient historian, not only, as Dyson suggests, for the prospect of insights into the lives of ordinary people, but because the textual data for fundamental aspects of ancient society like agricultural production, which was the resource base of the wealthy and the powerful as well as the peasant, are scanty.

A special study which has been justifiably hailed as of pioneering importance in this field is Mark Maltby's monograph on the animal bones from Roman and Medieval Exeter (Maltby 1979). In his analysis of the 18,000 fragments from the Roman town Maltby found, for example, that cattle figured very prominently in the sample, a piece of dietary evidence which, as far as I am aware, is not recoverable from any other source. There is much else of value in this report, but I wish to turn directly to what seems to me to be an intriguing and important problem of interpretation.

At Roman Exeter, how did meat reach the consumer? From the faunal remains it was deduced that "Cattle were brought to the city on the hoof for slaughter" ....."centres for slaughter" existed, and "organised butchery" was practised as part of "a systematic policy for the marketing of beef" and the "large scale organised marketing of cattle in the early Roman period" (Maltby 1979, pp.38-40). The institutional framework which seems to be posited here - producer to wholesale market to slaughterhouse to retail market to customer's home - is analogous to that which operates in our own society. And, as in our own, the laws that govern the civilian supply systems seem to be largely the laws of the marketplace.

But is this the only explanation which would fit the evidence? Since the bones are not intrinsically unambiguous in their meaning, there is at least a <u>prima facie</u> case for considering other, possibly non-commercial mechanisms of supply and distribution. (The legitimacy of ascribing modern economic concepts like 'systematic marketing policy' to the inhabitants of Roman Exeter is another issue). Might all the animals represented (and this is not many over a period of several hundred years) have been owned by the civic authorities, slaughtered and cut up by one or two part-time butchers and the meat distributed free of charge? Other hypotheses are possible and equally likely.

Without other data there is no way of resolving this difficulty. Here the historian's knowledge of conditions of Roman society can help. There is no written evidence directly relating to Exeter, as Maltby knew, but elsewhere in Roman Britain (for instance, at Bath (Salway 1981, p.687)) and much more abundantly in the Mediterranean Graeco-Roman world, which is comparatively well documented, there is a great deal of information which relates to the problem of the food supply of towns.

In the first place, the written record confirms the existence of cattle markets and butcher's shops. These certainly existed in antiquity and no-one would dispute that a supply and demand factor was part of the process which ended with the deposition of bones in the archaeological record. However, the written record suggests that other forces must also be considered.

In classical antiquity a wealth of literary and epigraphic data reveals that religious festivals were of exceptional importance to urban (and rural) communities. In the largest (and admittedly exceptional) cities there might be well over 100 days allotted annually to public festivals alone (Cartledge 1985b, p.99: Friedlander 1908, pp.11-12); at smaller places they might still be numerous (for example, Myconos (Sokolowski 1969: no.96)). It was normal practice on these occasions to sacrifice at least one animal, of which part would be reserved for the deity, part given to the priest and the rest consumed by the celebrants. Sometimes the number of beasts could be very large. On the second day of the festival of the Eleusinian Mysteries, for example, each initiate was required to sacrifice a suckling pig (Parke 1977, p.63). Since there might be hundreds, even thousands, of initiates in any one year the impact on the faunal record is likely to have been substantial.

Another urban institution which is well attested is the sponsored feast. Literature and many Hellenistic and Roman inscriptions, from a wide range of towns, small to large, record the provision by wealthy citizens of banquets for the community or sections of it (see Hands (1968) for a set of translated documents from Africa, Spain, Italy, Greece and Asia Minor). At Athens in the second century AD the millionaire Tiberius Claudius Atticus provided funds for 100 oxen "to be sacrificed to the goddess on a single day on many occasions and in providing a sacrificial feast for the whole people of Athens" (Hands 1968, D80), though generosity on this scale is rare. More typical, perhaps, were the public feasts held in the country town of Corfinium in central Italy (Hands 1968, D40) or at Aegialeon on the island of Amorgos, where beef and pork were set before the community (Hands 1968, D5). In the latter case, incidentally, it seems that joints of meat were given to the young men of the city to take away after the celebration. There is evidence strongly suggestive of similar practice elsewhere (Sokolowski 1969, no.96) and it would be interesting to know how widespread it was.

There would seem to be important implications here for the interpretation of faunal remains. Religious, social and political values - piety, display, ambition, prestige, tradition - may have played a significant part in the distribution of meat within the Greaco-Roman city, more significant, perhaps, than marketplace economics. Without the written evidence, however, how could the archaeologist be certain that such factors should be taken into account?

Graeme Barker, whose elegant discussions of faunal data have done so much to make historians aware of the potential of this evidence (see, for example, Cartledge (1985a, p.114)), has remarked that "in due course this kind of archaeology will make a very considerable contribution to our understanding of the economic life of the ancient city" (Barker 1982, p.91). This does not seem over-optimistic, as long as we recognise that the economy of the ancient world was embedded within its value-systems (Finley 1973) and proceeded accordingly. We owe this conceptual framework principally to the written evidence. Archaeology by itself could never reveal the interplay of political, religious, social and economic factors which influenced the production and distribution of food in antiquity (cf. Finley (1985, pp.24-5) on the legal and economic structures of local pottery production revealed by papyri from Oxyrhynchus in Egypt).

The final example concerns one of the largest archaeological projects in Europe in the last 35 years. A splendid synthetic account of the results has been available for some years (Potter 1979). I refer to the South Eturia survey, a massive volunteer programme of fieldwalking just north of Rome which in 20 years of work saved a huge volume of evidence threatened by the city's expansion. About 100km2 were surveyed and some 2000 sites recorded. In The Changing Landscape of South Etruria Potter (1979) provides a series of site distribution maps which show the development of the ancient settlement pattern. The later Republican and early Imperial period maps indicate a remarkable density of settlement, with numerous towns and road-stations and hundreds of farms, from smallholdings to great estate centres, all held together by a well-developed public and private road network.

Although recent, more intensive surveys suggest that at certain times parts of the classical world were even more thickly settled, these data had considerable impact at the time of their discovery and still impress. They appear to contrast sharply with the view based on the surviving literature that life in the ancient world was overwhelmingly city-based (Finley 1977, p.305) and evoke strikingly a sense of the organisation and activity of the countryside near Rome. This apart, though, has this mass of information anything more than antiquarian interest? Is there anything here of serious value to the mainstream historian?

At first glance, there would seem to be. A leitmotif of late Republican history is the fortunes of the peasant proprietor, who was the backbone of the Roman state and its armies. During the last centuries BC the yeoman was removed from the land by a combination of factors, not least the capital-intensive villa farming system which relied heavily on slave labour. This contributed to destabilisation of traditional values which led ultimately to the collapse of the Republic and its replacement by the Roman Imperial system, whose longevity is well known (see Hopkins 1978b for discussion of the process).

The South Etruria evidence appears to undermine one of the main props of this theory, the celebrated story of the aspiring politician Tiberius Gracchus' journey through Etruria in the 130's BC. Plutarch, a Greek biographer writing in the late first or early second century AD, tells us that Tiberius' impressions of this journey were recorded in a pamphlet written by his younger brother Gaius. According to Plutarch's version of this, Tiberius saw a largely deserted landscape, peopled only by the slave herdsmen who had replaced peasant farmers (Plutarch, Tiberius Gracchus 8) and he was so affected by this experience that land reforms became the burning issue of his later career; and indeed problems of the land remained very prominent in the history of Italy for the next hundred years.

Potter (1979, p.125) remarks that there is little in the archaeological record to support Plutarch's tale. Rather, the coutryside is thickly settled, mainly with smallholdings. Is this a clear case then of the material evidence undermining the written word and forcing a reappraisal of long held views? As Potter acknowledges, there are powerful objections. Apart from the question of location, there is a dating difficulty. The later Republican sites can be dated no more closely than to a third to first century BC bracket. All, therefore, might be of the third century BC, or the second, or the first. If not, what proportion should be allocated to each century? What if a major change in the settlement pattern had taken place in the second half of the second century? It makes a big difference to the interpretation of these data. Also, the material remains do not tell us how the land was owned - are we dealing with independant farms, tenant farms, small slave farms? In our present state of knowledge we cannot tell. Again, it makes a big difference.

Has this evidence then any value for historical enquiry? I would say that it does, providing that appropriate questions are asked of it. These should be questions which do not overstrain the evidence. For technical reasons the South Etruria survey data cannot inform us reliably about the nature of agricultural exploitation there in the 130's BC. However, along with other survey evidence from Italy, they can be used, as Garnsey (1979) does, to answer an equally important but different kind of question, "Where did the Italian peasant live?", for which the literary evidence is negligible. Garnsey's approach maximises the strengths of archaeological data - quantity, location, regional coverage, long time sequence - and minimises the weaknesses - imprecise dating, juridical definition - to produce plausible and to the historian very interesting results.

"All too frequently a line still divides archaeological from historical study" (Salmon 1984, Preface); "The present situation is that with a few rare exceptions archaeologists and historians are distinct groups with too little knowledge of each other's interests and problems" (Humphreys 1967, p.375). In twenty years little seems to have changed. Some of the reasons why a substantial sector of historical opinion (not the authors quoted above) remains unconvinced of the value of archaeology have been discussed. Perhaps the most important of these is the perception that for large parts of ancient history archaeology is a minor source of evidence and will remain so. In those areas of antiquity where it has an equal or the major role to play (Greek colonisation, trade, agriculture, settlement patterns and many more) the problem of ambiguity looms large. As the examples have shown, more work needs to be done on defining the limits of archaeological inferences. There is a feeling that a discipline which claims to have lost its innocence should be more prepared to admit, as historians sometimes have to, that 'we just don't know' and even 'we cannot know, without other evidence'.

This has been in the main a deliberately one-sided view of the interface between archaeology and history. It would be equally possible to ask the question 'why should archaeologists take historians seriously?' and I hope that someone, perhaps an historian, will do this. There would be much to discuss. For example, historians too have been reluctant to write synoptic works. To the best of my knowledge no-one has collected and analysed the copious and very detailed data available on animal sacrifice. The best discussion of stock-raising which Maltby was able to find was White's Roman Farming (1970), which does not set out to deal with consumption patterns. And there is still no book on Greek farming, even though it is widely recognised that agriculture accounted for at least four-fifths of the productive activity of the classical world.

I will end on a more optimistic note. Today more historians than ever before, especially the younger generation, are receptive to and often knowledgeable about archaeology. More historians are getting involved in joint fieldwork projects. The opportunity to break down more of the remaining barriers is there and more co-operation will be to the advantage of all. The illustrations which I have used in this paper point clearly to achievements as well as problems. For the archaeologist the challenge of working within an historical framework, where material evidence can often be controlled by, or at least compared with, other, frequently more powerful strands of information, is an exciting one. We should invite more historians to work with us, just as more historians are asking us to work with them.

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I have learnt a great deal about archaeology and history from many long and stimulating discussions with friends and colleagues in the Department of Ancient History and Classical Archaeology at Sheffield University, in particular John Drinkwater, David Kennedy, Philip Freeman, James Roy and above all R.I. Winton, who also made available to me the text of a brilliantly provocative paper on Archaeology and History which he delivered at a staff seminar in Sheffield in 1984. I should stress that the Sheffield department places considerable emphasis on archaeology in its teaching at all levels and in research.

BIBLIOGRAPHY

Alcock,L., 1984 - Review of Renfrew (1982) in The Antiquaries Journal, Vol.LXIV Part I, pp.112-113.

Bailey,D.M., 1985 - Review of Holladay et al (1982) in Journal of Egyptian Archaeology, Vol.71, pp.54-55.

Barker,G., 1982 - "The Animal Bones" in D.Whitehouse, G.Barker, R.Reece and D.Reese, "The Schola Praeconum I", Papers of the British School at Rome, Vol.L, pp.53-101.

Barker,G. and Hodges,R. (Eds), 1981 - Archaeology and Italian Society. Papers in Italian Archaeology II. British Archaeological Reports 102. Oxford.

Binford,L., 1962 - "Archaeology as Anthropology" American Antiquity, Vol.28(2), pp.217-225.

Cartledge,P., 1979 - Sparta and Lakonia. Routledge, London.

Cartledge,P., 1985a - Review of Graeme Barker, John Lloyd and Joyce Reynolds (Eds), Cyrenaica in Antiquity in Libyan Studies, Vol.16, pp.113-115.

Cartledge,P., 1985b - "The Greek Religious Festivals" in P.E.Easterling and J.V.Muir, Greek Religion and Society. Cambridge University Press, Cambridge.

Cherry,J.F., 1984 - "The Emergence of the State in the Prehistoric Aegean" Proceedings of the Cambridge Philological Society, Vol.210, pp.18-48.

Clarke,D., 1968 - Analytical Archaeology. Methuen, London.

Dyson,S.L., 1979 - "A classical archaeologist's response to the New Archaeology", paper presented to the 1979 meeting of the Archaeological Institute of America.

Finley,M.I., 1971 - "Archaeology and History" Daedalus, Vol.100, pp.168-186.

Finley,M.I., 1973 - The Ancient Economy. Chatto and Windus, London.

Finley,M.I., 1977 - "The Ancient City" Comparative Studies in Society and History, pp.305-327.

Finley,M.I., 1985 - Ancient Times: Evidence and Models. Chatto and Windus, London.

Fredriksen,M.W., 1970 - "The Contribution of Archaeology to the Agrarian Problem in the Gracchan Period", Dialoghi di Archaeologia, Vol.I, pp.330-57.

Friedlander,L., 1908 - Roman Life and Manners. London.

Garnsey,P.D.A., 1979 - "Where did Italian Peasants Live?" Proceedings of the Cambridge Philological Society, Vol.205, pp.1-25.

Hands,A.R., 1968 - Charities and Social Aid in Greece and Rome. London.

Holladay,J.S. et al, 1982 - Cities of the Delta. Malibu.

Hopkins,K., 1978a - "Economic Growth and Towns in Classical Antiquity", pp.35-77, in P.Abrams and E.A.Wrigley (Eds) Towns in Societies. Cambridge University Press, Cambridge.

Hopkins,K., 1978b - Conquerors and Slaves. Cambridge University Press, Cambridge. Hopkins,K., 1980 - "Taxes and Trade in the Roman Empire" Journal of Roman Studies, Vol.LXX, pp.101-125.

Humphreys,S., 1967 - "Archaeology amd the Social and Economic History of Classical Greece" La Parola del Passato, Vol.116, pp.374-400.

Maltby,M., 1979 - Faunal Studies on Urban Sites. The Animal Bones from Exeter 1971-1975. Department of Prehistory and Archaeology. Sheffield.

Parke,H.W., 1977 - Festivals of the Athenians. London.

Piggott,S., 1959 - Approach to Archaeology. Black, London.

Plutarch - Tiberius Gracchus translated by B. Perrin in Plutarch's Lives, Vol.X, Loeb Classical Library, London, 1921.

Popham,M.R. and Sackett,L.H., (Eds) 1980 - Lefkandi I. The Iron Age: Text. Thames and Hudson, London.

Potter,T.W., 1979 - The Changing Landscape of South Etruria. Elek, London.

Renfrew,A.C., 1982 - Towards an Archaeology of Mind. Cambridge University Press, Cambridge.

Salmon,J.B., 1984 - Wealthy Corinth. Oxford University Press, Oxford.

Salway,P., 1981 - Roman Britain. Clarendon, Oxford.

Snodgrass,A.M., 1983 - "Archaeology" in M. Crawford (Ed) Sources for Ancient History. Cambridge University Press, Cambridge.

Sokolowski,F., 1969 - Lois sacrees des cites grecques. Paris.

White,K.D., 1970 - Roman Farming. Thames and Hudson, London.

ARCHAEOLOGISTS IN ACADEME: AN INSTITUTIONAL CONFINEMENT?

J.G.Lewthwaite

INTRODUCTION

In this necessarily brief and superficial essay, I wish to reflect on the contemporary crisis in archaeology, a crisis all the more profoundly disturbing because of the cold and complacently silent mask it wears, at least when compared with the quantities of heat and light generated by the turbulent theoretical polemics which followed the publications of Clarke and the Binfords in the annus mirabilis of 1968; as its participants have gained weight and lost hair, succumbing to the relative material comfort of middle-aged, middle-class professional suburbanity (the video recorder to tape 'Spitting Images' while in the pub, the golden labrador contentedly nestling among the Sainsbury's groceries and the children's safety harness in the back of the family Volvo estate...) all the last traces of the vigorous soaping and scrubbing which once promised to open the disciplinary pores appear to have evaporated, leaving only a disagreeable bathmark of opaque sub-sociological jargon and some deep gurgling sounds from the politicised opportunists in the academic plughole. Archaeology's fall from grace through tasting of the fruit of the tree of knowledge has yet to find its Milton, although we have all grown weary of the skein of articles entitled 'Paradigms lost'; a traumatic loss of innocence (Clarke 1973) has initiated, not the eagerly sought 'meaningful and ongoing open-ended relationship' with caring disciplines but a string of sordid little affairs with every hustling paradigm in sight, a very library of one-book stands.

At the heart of this disillusion lies the failure of the emulation of Science (as then understood) to deliver the goods; it is instructive in this respect to contemplate the slow transformation of Kent Flannery, one of the discipline's most honest, incisive and articulate spokesmen, from the optimistic arch-proponent of systems theory, albeit already alert to the proliferation of 'Mickey Mouse Laws' (Flannery 1973) through his most celebrated role as the shrewd and witty observer of the human-zoo aspect of the archaeological microcosm (Flannery 1976) into the conference-weary Juvenal, driven by outrage at the cynical opportunism of the theoretical hotshots spawned by the New Archaeology into a reawakened nostalgia for the values symbolised by the oldtimer's trowel (Flannery 1982). While emotionally satisfying, this is no lasting solution. To begin with, I would like to point out that the alternative of a realignment with History was never seriously considered by the 'generation of '68', particularly but not exclusively the Americans among them, which I could explain as much in terms of the fundamentally ahistorical and (to neologise) 'ageographical' quality of transatlantic culture as through the more obvious effects of a professional formation in departments of anthropology. In Britain the situation is different: the historical consciousness is greater, the departmental affiliation (particularly in the 'provinces') very often with Classics and Ancient History, the influence of archaeologists such as Gordon Childe, R.G.Collingwood and Stuart Piggott profound and persistent.

SCIENTIFIC STATUS

Science rules, O.K.?

If there has been one fetish or ju-ju of which the New Archaeologists have been particularly enthralled, it is surely that of the Scientific Method, that miraculous mode of establishing Theory of progressively greater power, scope and elegance on a bedrock of empirical reality. True believers are endowed with wondrous powers: if not an invulnerability to bullets or the power to cast out demons, at least of attaining that Scientific Status which can be guaranteed to inspire a superstitious reverence among the uninitiated and a proper place in the hierarchy of this poor world.

Such is not, however, the view of sociologists or historians of science, who are at least tolerably familiar with what scientists do and have done, nor even of a growing band of philosophers of science, who believe they know what scientists ought to do; in retrospect, it is perhaps unfortunate that the New Archaeologists placed their faith in the latter rather than the former, although, to be fair, the exponential growth of such research has occurred only very recently.

Thus, in a reflection on the supposed seventeenth-century Scientific Revolution, Fores (1984, p.227) observes that "There is no more a single and identifiable 'scientific method', which has been used regularly by scientists to generate science-as-knowledge, than there is a single, identifiable, articulable and successful 'method of wheelwrighting'...There is a range of particular methods of working to choose from at the time of working...In each case the appropriate test is applied to the product of working effort." Her microsociological study of the day-to-day practices of a science laboratory has drawn Knorr (1979) to conclude that: "The mechanisms ruling the progress of research are more adequately described as successful 'tinkering' than as hypothesis-testing or cumulative verification. Epistemologically, a constructivist model of the scientific mode of operation suggests itself...A satisfactory mode of operation in which successes are achieved entirely through experimenting with locally existing opportunities invokes the image of tinkering...Theories are the cocoons left behind when practice is abstracted from the conduct of enquiry." Finally, Laudan (1982) has argued that while philosophers of science were once agreed that the essence of science lay in its capacity to generate consensus (compared with the interminable, subjective squabbling elsewhere), the irruption of Messrs Kuhn and Feyerabend has generated a concern with the capacity of science to be controversial; incidentally, neither generalisation necessarily involves much correspondence with an objective 'reality', the consensus being achieved through the successful internalisation of rules and norms in an institutional context, the ubiquity of controversy being correlated with the incommensurability of paradigms, the under-determination of theory (cf Duhem-Quine thesis) and the many instances of successful 'deviant' behaviour.

Worse, Knorr (1979) depicts the everyday life of scientific folk in a manner which may prove harrowing to the tender-minded; it seems that they have more in common with laboratory rodents than white coats and pink noses. Apparently, scientists compete for 'social credit' alias 'symbolic capital' in an 'antagonistic field' (cf Latour-Woolgar thesis), invading slack 'contingency-space' in search of 'distinctiveness', an 'asset' to be milked for all it is worth; experimentation and publication are pragmatic, tactical operations planned in anticipation of accusations of incompetence or irrelevance from territorial competitors; 'facts' are to be hoarded till useful, facilities denied; truly, a laboratory red in tooth and claw! If we learn anything, it is that scientists are, after all, much more like archaeologists than we ever thought - or feared (cf Flannery 1976 passim, 1982).

Such a 'constructivist' position practically implies the irrelevance of any correspondence with reality, attributing to Fores' knowledge-wrighting a verification through 'internal' criteria of logic (if any logic at all), in effect, an epistemology close to that of traditional Idealism. The scientific 'tinkering' with assets at hand and such as can be appropriated from slack contingency-space seems disturbingly close to the Levi-Straussian bricolage (the activity of a 'bodger' or handiman working with material at hand...) of mythopoeia (Levi-Strauss 1972, pp.16-22; Figlio 1976, pp.19,31; Fores 1983, pp.155-6): are scientists, then, no more than successful mythmongers?

On reflection, Knorr's case study appears to be a case of David Clarke's "attempt to explain the Vietnam war in terms of electron displacements" (Clarke 1973, p.10), ie of a failure to relate the scale of observations to that of the phenomenon under investigation. A more useful examplar, particularly for archaeologists, is surely Martin Rudwick's study of the Great Devonian Controversy in nineteenth century geology (Rudwick 1985) precisely because it deals with a completed cycle of scientific research resolved to the satisfaction of participants and posterity: at least we know that he got the outcome of the war right!

In an exemplary introduction, Rudwick outlines his aims - to achieve a fine-grained 'thick' description of small facts relevant to large issues through a narrative carefully crafted to convey subjective time, to eschew anachronism and Whiggism, to zoom in and out in a 'dynamic spiral of involvement and detachment, of immediacy and abstraction' (Rudwick 1985, pp.3-16). Would that a single archaeologist were as conscious of such a need, as capable of such finesse!

In brief, while Knorr's point of view is validated in terms of the detail of the controversy, ie at the level of the electron displacements, the conclusions with respect to the war are more germane to Laudan's puzzle: "plotting the trajectories against the absolute historical timescale provided by the detailed documentation clearly reveals the contrast between certain stable states (author's emphasis) of interpretation, on the one hand, and the specific occasions which destabilized them and produced rapid changes in the opinions of particular individuals, on the other" (Rudwick 1985, p.418). Rudwick confirms:

(i) the existence of tacit bodies of craft knowledge - "It was not from textbooks that the correct meaning and application of technical terms and concepts was learned, but rather from witnessing their use (author's emphasis) - whether routine or still controversial - by authoritative and established practitioners...The relatively stable core of this body of practical knowledge was learned ostensively by newcomers to the science, while more seasoned practitioners were continually refining and modifying the application of even the most well established categories" (Rudwick 1985, pp.445-6);

(ii) the existence of a 'gradient of attributed competence' trichotomised into 'elite', 'accomplished' and 'amateur' categories accepted by the participants themselves and demonstrated in their actual behaviour (Rudwick 1985, pp.418-428);

(iii) the existence of a 'core-set' of scientists whose attention was focused on this transient 'hot-spot' in science - as suggested by Collins (1981) - (Rudwick 1985, pp.426-428);

(iv) the theory-, even controversy-laden nature not only of rhetorical, persuasive presentations (lectures, papers) but even of field observations - "Modern practitioners of scientific research have often been deceived by the imposed conventions of their discipline into supposing that their papers are unproblematic reports of experiments performed, results obtained and conclusions reached...But the contrast comes as no surprise to reflective practitioners of scientific research, among whom there is a healthy tradition of deriding and unmasking the pompous obliquities of formal, scientific style. Nor should the contrast surprise any analysists of science who recognise the persuasive intentions and rhetorical character of any (author's emphasis) form of scientific discourse...A scientific paper is designed, covertly if not openly, to present the best possible case in court; except fortuitously, its structure will therefore not (author's emphasis) reflect the actual pathway of research...Any scientific paper, if it is all substantial in content, presents the favourable evidence with maximum rhetorical effect; weak links in the argument are glossed over or concealed; any antagonists' cases is (sic) attacked, and the force of their evidence subtly undermined or openly dismissed or even ridiculed...On the surface, almost all the papers relevant to the Devonian controversy appeared to be straightforward reports...yet contemporaries were not deceived by this; they were well able to "read between the lines" of formal papers, as their recorded informal comments amply demonstrate. They were under no illusions about the persuasive and argumentative intentions of what they read or heard in formal settings...From its most private layers to the most public, the scientific activities that constituted the Devonian controversy were social as well as personal; and they were expressed throughout in terms of rhetorical argument" (Rudwick 1985, pp.433-435). "In fact, as the narrative of the controversy has shown, it was not only the changing uses of technical terms and the modified conceptual meanings they expressed, but also the very reaility of alleged "facts" that were shaped through argument and debate" (Rudwick 1985, pp.447-448). "The particular observations made, and their immediate ordering in the field, were often manifestly directed toward finding empirical evidence that would be not merely relevant to the controversy but also persuasive (author's emphasis)" (Rudwick 1985, pp.431-432);

(v) the existence of an antagonistic field (in the sense of Latour and Woolgar) pervaded by imagery drawn from the battlefield, the courtrooms or Parliament - prompting the aside that "Apart from their abstract formalisms, nothing has more seriously vitiated analyses of scientific work by philosophers of science than their tacit assumption that science is governed by procedures analogous to statute law rather than common law" (Rudwick 1985, p.436 n.11);

(vi) the existence of social credit, even a social identity, as a motivation in scientific research - "On occasion the participants themselves distinguished clearly between substantive geological questions and the assignment of credit for priority in discovery..; but in the ordinary course of argument...they were mixed almost inextricably" (Rudwick 1985, p.440);

(vii) the inapplicability of the conventional distinction between contexts of discovery and of justification (Rudwick 1985, p.338);

(viii) the inapplicability of the Kuhnian concepts of 'paradigm' and 'scientific revolution' - "The Devonian controversy was not a major "scientific revolution" or episode of extraordinary science"; nor was it merely a minor puzzle within a humdrum and static tradition of "normal science" the solution of which left that "paradigm" essentially unchanged. As historians of science have long recognised, the Kuhnian terms are difficult to apply to real examples of scientific change without somewhat procrustean adjustments. In retrospect, perhaps the most important of Kuhn's insights was his emphasis on the socially embodied and socially sustained character of the traditions within which virtually all scientific work is and has been carried out. It is the activities of persons (author's emphasis), not disembodied ideas, concepts, theories or "research programs" that constitute research traditions...". "In all these ways, the resolution of the Devonian controversy left the practice of stratigraphic geology profoundly changed from what it had been before. But this major transformation was accomplished with none of the symptoms of a "scientific revolution". It was achieved within (author's emphasis) an established paradigm, or rather within a socially embedded research tradition. It entailed the gradual (author's emphasis) modification of the meanings and uses of technical terms, methods, criteria, concepts and so forth. The paradigms, like the terms and concepts in which it was expressed, proved to be gradually but indefinitely malleable and adaptable. The Devonian controversy shows how in scientific research, no less than in politics, art, and religion, human traditions continually provide from within (author's emphasis) themselves the resources for creative renewal" (Rudwick 1985, pp.445-450);

(ix) the hermeneutic aspect of scientific research: "Stratigraphical geology, like any other branch of scientific research though more patently than most, was (and is) intrinsically a hermeneutic activity. It is for this reason that the term "interpretation" has been used so freely throughout this narrative and analysis of the Devonian controversy, and terms such as "theory" so sparingly. That stylistic preference not only expresses the character of what was going on in the controversy, seen in retrospect; it also reflects the participants' own evident awareness that interpretation is what they were engaged in" (Rudwick 1985, p.447).

Nevertheless Rudwick rejects the extreme constructivism of Knorr and others in his concluding section on 'social construction and natural knowledge': "The Devonian controversy is an instructive case study...It can be used to explore the way in which a consensual product of scientific debate can be regarded as both artifactual and (author's emphasis) natural, as a thoroughly social construction that may nonetheless be a reliable representation of the natural world...In place of the implausible extremes of either naive realism or social constructivism, what is needed is not a compromise but a way of transcending the dichotomy" (Rudwick 1985, pp.451-454). Rudwick sees the solution in the analogy of a map and of mapping: "Bookish people with no practical experience of mapping often assume that a map is an unproblematic replica of reality...Those who make intensive use of cartography know on the contrary that any map is a pervasively conventional representation...it makes no sense to talk of ever achieving a uniquely "perfect" representation, or a complete "correspondence" with reality, since different kinds of maps are designed for different uses, and there is no limit to the further representations that may be needed for new and unforeseen purposes...the analaogy of mapping yields a way of retaining the constructivists' insistence on the social processes that went into the making of a piece of scientific knowledge...while also allowing the realists' insistence that the real natural world...had a more than marginal effect on that claimed knowledge" (Rudwick 1985, p.454); another analogy would be that of 'shaping' or 'forging': "To put the point another way, neither "discovery" nor "construction" is by itself (author's emphasis) an adequate metaphor for the production of scientific knowledge. The outcome of research is neither the unproblematic disclosure of the natural model nor a mere artifact of social negotiation...For the Devonian controversy shows how new knowledge is shaped from the materials of a real natural world, malleable yet often refractory; but it becomes knowledge only as those materials are forged into new shapes with new meanings, on the anvil of heated argumentative debate" (Rudwick 1985, pp.454-455). In conclusion, Rudwick argues that: "In this way, it is possible to see the cumulative empirical evidence in the Devonian debate neither (author's emphasis) as having determined the result of the research in any unambiguous way, as naive realists might claim, nor (author's emphasis) as having been virtually irrelevant to the result of the social contest on the antagonistic field, as constructivists might maintain. It can be seen instead as having had a differentiating effect on the course and outcome of the debate, constraining (author's emphasis) the social construction into being a limited, but reliable and indefinitely improvable, representation of natural reality" (Rudwick 1985, pp.455-456).

In effect it is nothing if not ironic that the New Archaeologists who profess to reject a normative model of culture have so eagerly espoused a normative model of scientific research; to employ Rudwick's metaphor of the practice of the law courts, they have simply reduced the drama of a trial to the formalities of the Judges' Rules.

The Wrighting of History

In contemplating the arrogant disdain which the New Archaeologists reserve for History, I am inevitably reminded of two passages from George Huppert's article on the psychology of historical erudition in France between the sixteenth and nineteenth centuries (Huppert 1973), appropriately enough lodged in a festschrift dedicated to Fernand Braudel:

"While the literary historians took it for granted that the corpus of histories inherited from the past could not be improved upon, the antiquarians, on the contrary, declared all inherited histories worthless and prepared to reconstruct the past starting from the documents. The lack of comprehension the antiquarians met with can be gauged from their constant need to explain what they were doing" (Huppert 1973, p.268).

"The real distinction is between erudits and litterateurs. The erudit's business - his only business as he sees it - is to make make discoveries founded on the study of original sources (author's emphasis). He does not care whether his discoveries are important, philosophical, relevant or piquant. That is why we tend to think of him as an antiquarian. The litterateur, on the other hand, is not interested in discoveries at all. He does not necessarily care whether his facts are correct or not. What he wants is an epic tale, nobly told; or else, a philosophical tale, in which the historical narrative serves only one purpose, that of providing material for moral analysis" (Huppert 1973, p.275).

Our New Archaeologists seem to believe that little has changed under the historical sun, that Clio's devotees whirl and babble in the same old rituals, grabbing in musty archives and purveying novel faction to the nostalgic throng; yet this war of the ancients and moderns closely parodies our own recent debate, particularly as regards the attitudes and behaviour of the antiquarians (our novi homines) while figures of the erudit and the litterateur are, after all, but Flannery's Real Mesoamerican Archaeologist and Great Synthesizer. No major historian since Charles Victor Langlois has argued that "the true role of the historian is to put the people of today in contact with the original documents that are the tracks left by the people of yesterday, without mixing anything of himself in them" (Keylor 1975, p.178) while striving "to establish the authenticity of the documents, clarify them, catalogue them, and thus make a bibliography to orient scholars" (Keylor 1975, p.180) or justified history purely in terms of the intrinsic raison d'etre of historical studies, the pleasures of working afforded to the historian, or his being "instinctively impelled to collect, with the same seriousness, all the traces that subsist" (Keylor 1975, p.179); according to Lawrence Stone (1971, p.49) such is the displacement activity of the anal-erotic male.

The naive realism and positivism which underly these statements, based on a misunderstanding of Ranke's historismus (Keylor 1975, pp.8-9) has waned unceasingly since the last two decades of the last century (Hughes 1959, pp.183-248), the historical breukvlak of Wesseling (1978). The first to reflect and respond were Germans such as Dilthey, Windelband, Rickert, Lamprecht and Weber - "a generalizing or theoretic historian" rather than a sociologist, remember (Birnbaum 1978, p.226) - soon followed by Croce in Italy (Cedronio 1981) and sundry fin de

siecle Americans, Britons, Swedes and Spaniards; in France, the decisive reorientation occurred with the founding of the annales in 1929 - a year before Binford's birth - eight before Clarke's (Burguiere 1978; Cedronio 1977, pp.3-18; Leuilliot 1973; Revel 1978, 1979; other references in Lewthwaite in press a). But three-score years before Braudel rejected the renascent positivism of the social scientists on account of their failure to deal with temporality, their adoption of either the timeless mathematical model or the evanescent events of actualite (Braudel 1958), before even his maitre Lucien Febvre "au lieu de pretendre que l'histoire est une science comme les autres, essayera plutot de demonstrer que la science telle que la concoivent les savants de son temps et l'histoire telle qu'il la concoit operent de la meme maniere" (Massicotte 1981, p.27), rejecting the search for laws in favour of the formulation of a problematic (l'histoire-probleme) (Massicote 1981, esp. pp.21-42), an epistemological debate had raged in and around the Sorbonne involving historians such as Seignobos, Hauser, Mantoux, Lacombe and Xenopol, philosophers such as Berr and Feuillee, the sociologist Durkheim and the economist Simiand, about which we are particularly well informed (Keylor 1975, pp.55-207, esp. 75-89, 111-140, 178-187; other references in Lewthwaite in press a). Although there is a constant need to be aware of the contextual phenomena - the French political situation in the wake of the Dreyfus affair, the attempt to institutionalise sociology - there is much about the debate which anticipates the polemic of the New Archaeologists and the various reflections and reactions of the old:
(i) Langlois' brand of pedantic positivism was implicitly criticised by his colleague and co-author, the leading theoretician of the Sorbonne historical establishment Charles Seignobos, in terms of an epistemology which strikingly anticipates that of Hawkes and Piggott. According to Seignobos, history could not be classified as a science, because the natural scientists observed reality directly, whereas the historian apprehended the past only indirectly through the fragmentary body of surviving documentation, the lacunae of which had to be filled in through the historian's reasoning from models of present reality. Despite his overall mood of pessimism, Seignobos opined that the certitude of such inferences would be enhanced by the mutual confirmation of several 'presumptions' grouped together. In the end, history, while not a science, might yet be scientific, since the final stage of historical synthesis, in discovering intelligible generalisations from a mass of refractory evidence, approximated to science in its process and its products (Keylor 1975, pp.78-80);
(ii) the critique of established history instigated by Emile Durkheim and his ally Francois Simiand evokes the charges brought by the American New (or anthropological) Archaeologists against their mentors: that they frittered away time and energy and resources on the mindless accumulation of 'facts' without an idee directrice, that they aimed at nothing more profound than showpiece monographs of specialised erudition, that they failed to search for the laws of historical development underlying the superficial manifestations of reality, that they turned away from truly scientific universal principles of causation (Keylor 1975, pp.113-115).

The economist Simiand's 'epistemological polemic' was particularly scathing (Besnard 1983a, pp.251-253): sociology truly deserved the title of science, since it referred to an objective domain, while historicizing or traditional history (l'histoire historisante) focused on unique, unrepeatable events; historians misunderstood causation as the identification of motivation in the actor or in terms of unsystematically selected anterior events, whereas social science sought through the detection of invariable and unconditional antecedence between facts of the same order to identify stable relationships between phenomena; historians (particularly Henri Hauser) regarded the interdependence of facts within a single society as a principle, whereas social scientists sought causal relationships between phenomena in a sample of several societies; historians should be verifying hypotheses rather than attempting to produce a sort of photographic facsmile of the past. In effect, history could not even accumulate the data for, or test against empirical evidence the theory of, social science - as allowed by some more moderate Durkheimians. It was presumably with this 1903 epistemological assault in mind that Finley wrote, reviewing David Clarke's Analytical Archaeology, that (Finley 1971, p.172) "we know where we are now; the familiar polemic of the social scientist against history has been carried back into prehistory".

(iii) All historians did not react with a uniform and predictable chorus of outrage, some at least meeting the sociological onslaught through an exhortation to their fellows to put their house in order. Thus, Paul Lacombe initiated what has become known as the Lacombe-Xenopol debate with an reasoned programme for a revitalised history. According to Lacombe, historians had failed to identify the meaning, and hence to adopt the aims and methods of the true sciences: in respect of the former, they had pursued the reconstruction of historical reality (mere detail) rather than historical truth (meaning) through a timid refusal to apply principles of selection, such as those of similitude and succession, whereby they might distinguish between recurrent and persistent structures ('institutions') and the mass of unique and transient events, arriving at general truths; in respect of the latter, they had failed to combine inductive and deductive reasoning, to dissect reality with the powerful heuristic devices of the hypothesis and the imaginative experience, to call into being 'fictitious categories'. Nevertheless, Lacombe remained pessimistic, appreciating the contingent nature of historical causation (Keylor 1975, p.118-121).

Lacombe's gauntlet was picked up by the Rumanian historian Alexandre Xenopol in a championing of traditional historiography which is generally reckoned unsuccessful, but which made a valiant effort to defend historical methodology as valid and appropriate for its own scientific domain, one shared with other 'historical' or diachronic sciences such as geology and palaeontology, and quite distinct from the timeless, ahistorical, synchronic sciences, both hard and soft, which occupied the 'theoretical' domain. According to Xenopol, it was not the historian's business to detect either institutions or general laws, but rather to study unique causal chains or sequences - "not to establish relations of similitude and coexistence, but rather of difference and succession" (Keylor 1975, pp.121-124).

A similar faith in the absolute, universal and unchanging quality of human reason as the historian's main heuristic instrument, and a similar distinction between the timeless truths, susceptible to mathematical formulation and symbolization, of the physical sciences and the evolutionary phenomena apprehended only indirectly by the descriptive sciences (geology, botany and zoology), the latter sharing a common method with history, are apparent in the further works of Charles Seignobos, who could not resist dismissing the sociological critique with the clearly rhetorical trope to the effect that a synchronic study of social phenomena was impossible, since social facts became past events, and hence fell into the doimain of history, immediately on occurrence! The historical method was therefore the only form of reasoning appropriate to the human sciences; occupying as it did - another rhetorical flourish - the middle ground between positivism and the metaphysical speculation of philosophers, history was the natural unifying (sc. imperialist!) force among the social or human sciences (Keylor 1975, pp.181-183);

(iv) Such rhetorical tropes were matched, inter alia, by the philosopher Alfred Fouillee (keylor 1975, pp.185-187), who argued that philosophy occupied the true middle ground between excessive specialisation and ideological polemic; indeed, the moment a historian departed from the study of the minutiae of the evidence to express ideas, he was intruding into the domain of the philosopher! History was not only ideological, he asserted, but irremediably cleft in twain by an irreconciliable contradiction between an ontology of an eternal becoming, of the evolutionary nature of reality and an epistemology based on analogy between past, present and even predictable future events, and doomed to disciplinary extinction through a bifurcation between those who advanced towards a sociological future and those who retreated in to an ever more involuted erudition. Fouillee's alliance of sociology with philosophy, it must be recalled, is but part of the rather overlooked inheritance of the philosophical tradition by the Durkheimians, particularly through the mediation of Emile Durkheim's mentor, Emile Boutroux (Keylor 1975, pp.183-184).

(v) Such a dialectic can hardly be said to have been resolved, yet in the best Hegelian mode it engendered a synthesis: specifically, the foundation of both a journal and a research-seminar centre for historical synthesis by Henri Berr, a professeur of rhetoric in a khagne (the Lycee Henri IV) and another ex-normalien inspired by Boutroux. Berr went so far as to admit the pre-eminence of history among the human sciences, arguing that it dealt in causation without losing touch with reality, while advocating an alliance with sociology, on the grounds that each was but a complimentary aspect of a single science; historians should, above all, rise above erudition and empiricism to achieve synthesis, if for no other reason than that of the deep human need for such pithy wisdom, which had been provided previously only in an imperfect form by the various metaphysical systems of philosophers of history from Vico to Hegel (Keylor 1975, pp.251-253). Such recommendations went unhindered in the Sorbonne of the day - Berr being a marginal figure - but through their radicalisation of Lucien Febvre and Marc Bloch, members of the Berr cercle (Braudel 1972; Clark 1971, 1973, pp.66-92) contributed to the eventual emergence of the Annales school of history (Siegel 1970; other references in Lewthwaite in press a).

Such episodes of epistemological renewal are far from rare in the history of History, yet remain essentially ignored by archaeologists, as if the relatively trivial differences in the form of the evidence, absolute timescales or degrees of temporal resolution outweighed the positive similarities between the diachronic sciences of man and invalidated the accumulated wisdom of the senior discipline. Two problems are fundamental to any consideration of thier mutual relationship:

(i) that of the identity or otherwise of the working practice of the historical, humanistic or social-scientific disciplines compared with the hard sciences (discussed in part above, from the perspective of the latter);

(ii) that of the appropriateness of the narrative form traditional in History as a vehicle of scientific explanation. With regard to the first question, no-one has probed the issue more deeply than the historian Max Weber, whose reflections have been summarised most clearly and concisely by McLemore (1984). In brief, Weber's argument runs as follows:

(a) The logic of the natural and cultural sciences does indeed differ, but not primarily on account of the nature of the phenomena studied: "there is no logically necessary link between the object domain of a science and the type of concept it uses." The primary distinction is rather between the nomological sciences and those dealing with concrete reality (Wirklichkeitswissenschaften), in that concept formation in the one treats of the common properties of a set of phenomena, but in the other with the peculiar properties of a particular phenomenon. Nomological sciences seek to discover "systematic causal hierarchies capable of providing deductive explanations" through a process of increasing generalisation from reality: "The definitive logical instrument (author's emphasis) of these disciplines is the use of concepts of an increasingly universal extension (author's emphasis). For just this reason, these concepts become increasingly empty in content (author's emphasis). The definitive logical products (author's emphasis) of these disciplines are abstract relations of general validity (author's emphasis)." Sciences of concrete reality, however, conceptualise essential properties or characteristic aspects of phenomena into the form of "historical indiciduals", just as abstract, just as selective as those of the nomological sciences, but uniquely individual: "a complex of elements associated in historical reality which we unite into a conceptual whole from the standpoint of their cultural significance." (McLemore 1984, pp.279-282).

(b) Both the nomological sciences and the sciences of concrete reality provide causal explanations, which differ in kind; if anything, the latter pursue the concept of causality more fully: for while the former regard explanation as a deductive process or one of subsumption under a lawlike generalisation such as a mathematical formula, thereby ultimately dissolving the dynamic bond of cause-and-effect into causal equivalence, the latter can never in principle explain a concrete historical event by deduction or subsumption, only by the imputation of a causal relationship between qualitatively different phenomena, a procedure which can be assisted but never supplanted by reference to general laws, with or without 'boundary conditions' (McLemore 1984, pp.282-285).

(c) Nor does the criterion of value-relevance - of the significance for the scientist of the object of his study - differentiate natural and cultural sciences as such, but only as a function of this primary distinction. For in effect "value-relevance is the logically necessary (implicit and explicit) criterion for selecting any time particular phenomena are chosen for study, regardless of the social or natural character of these phenomena" - but since socio-cultural science can only really deal with concrete events, according to Weber, value-relevance is inescapable. In effect Weber postulates on asymmetry: natural science can either be nomological or idiographic, but a nomological socio-cultural science would not contribute "to the understanding (author's emphasis) of those aspects of cultural reality which we regard as worth knowing" (author's emphasis)(McLemore 1984, pp.285-291).

(d) One and only one property distinguishes the socio-cultural from the natural sciences as such: the necessity of providing an interpretative understanding (verstehen) is axiomatic for the former: "As regards the interpretation of human conduct, we can, at least in principle, set ourselves the goal not only of representing it as "possible" - "comprehensible" in the sense of being consistent with our nomological knowledge. We can also attempt to "understand" (author's emphasis) it: that is to identify a concrete "motive" or complex of motives "reproducible in inner experience"... In other words, becasue of its susceptibility to a meaningful interpretation (author's emphasis)...individual human conduct is in principle intrinsically less "irrational" than the individual natural event. "Explanation necessarily involves not only "causal" but also "meaning adequacy": a historical novel lacks the former, a mere statistical correlation deviod of a chain or motivation the latter. Once again, socio-cultural phenomena require fuller explanation than those of nature (McLemore 1984, pp.292-295).

There is an all-or-nothing quality about Weber's philosophy which is not only unsettling (can archaeologists really hope ever to establish verstehen with the denizens of the Lower Palaeolithic? Is 'meaning adequacy' appropriate to, say, the origins of agriculture?) but perhaps unnecessary, if we follow Knapp (1984) into an exploration of the continuum of theory and historicity, or uniqueness. For this author "Against any view of an uncrossable gap between theory and history, I argue that observed regularities in social theory depend upon historical content and milieu...when sociologists...decide that concern with theory absolves them from concern with history, their product will not only be irrelevant historically, it will not even be adequate as theory...against the growing number of philosophers and sociologists who deny the possibility of falsifiable theory, I argue that social science can achieve it, but only if dependencies of regularities upon milieu and content are explicitly considered. Central theoretical tasks revolve around problems of content, that is, that specification of a theory's boundary conditions or domain of applicability - the historical or cultural conditions under which the theory applies" (Knapp 1984, pp.34-35).

For Knapp, it is a question once again of the relationship between form and content, or substance: "the formal elements of a theory or model are not something reality imposes on the theorist but what the theorist takes to reality...substantive elements are what the reality imposes on the observer...Because any social theory can only deal with selected aspects of any concrete reality, there are always indefinitely many variables which are left over or residual to the theory: they constitute its formal residue...A theory is falsifiable only when it is possible to state when it ought to apply. Since the substantive residue determines the domain of applicability of a theory, only a theory with a finite substantive residue will be falsifiable...A theory with a finite substantive residue is one which models a social structure, process, or reality which is so tightly interrelated, yet loosely enough connected to other realities, that it is determinative of some consequences, regardless of other aspects of human social behaviour" (Knapp 1984, pp.35-38).

Having defined what theory is, Knapp goes on to castigate ahistorical social theorizing in terms very close to those of Weber or indeed Braudel in his celebrated longue duree paper (Braudel 1958): "Relatively few social theorists today pursue historical and comparative studies to test out the substantive residue of their models. Instead they subsume all residual variables under a blanket <u>ceteris paribus</u> clause. The result is unfalsifiable theory...some theorists hold residual variables constant concretely...such a model does not permit predictions because it is not in a form which can be applied to realities other than the one it describes...some theorists hold residual variables constant abstractly by stating the model as a set of propositions such that no relations between its variables logically follow from these conditions...It is a formalist, or ahistorical, definitive model...it is vacuous and tautologous...best viewed not as models, but as conceptual schemes (Knapp 1984, p.39).

Knapp concludes that "against any view that there is some finite stock of events which are either lawful or idiosyncratic, topics of sciences or of art, I suggest that it is processes or aspects of events which have a longer or shorter substantive residue. Against the view that events are exhaustible mines which must be claimed for one or another discipline, I suggest that the disciplines increase the material available to other disciplines...these considerations suggest a dialectical process in which description of an historical configuration encourages the growth of causal theory which, in turn, requires the establishment of further configurations" (Knapp 1984, p.51). In effect, Knapp is belatedly exacting the vengeance of the tribe of historians on Simiand's social scientists: it is now they who as antiquaries of the present mindlessly multiply monographs as <u>erudits</u>, who fabricate formalist theory as <u>litterateurs</u>. In so doing, he re-establishes not only the historical nature of all socio-cultural theorising but the key role in history of socio-cultural concepts corresponding to Weber's ideal type or Lacomb's <u>corps fictif</u>: a middle-range construct dialectically suspended between formless substance and denaturised theory. Finally, he is outlining a mode of scientific behaviour fully comparable with that which Rudwick has identified in the history of geology, if as yet inadequately realised.

With regard to the second question, that of "narrativity", White (1984) will be taken to provide a concise and up-to-date summary of a long-running and complex debate. The point of departure, according to White, is the embarrassment occasioned by the feeling that narrative is a fundamentally unscientific form of discourse among those historians who would be scientists; among the traditionalists, the issue is unproblematic, since it is held that only the 'form' of the discourse could be confused with the mere telling of a story (myth, comedy, farce etc) since the 'content' is the representation of real events. In particular, the narrative or story-telling component is normally distinct from any interpretive or 'dissertative' involvement of the historian (White 1984, pp.1-6). This orthodoxy, which was canonical among the nineteenth-century historians of state, has been criticised on a number of counts, more particularly in France than among exponents of the Anglo-American analytical tradition:

(a) for the <u>Annales</u> historians (eg Furet 1975), narrative is inherently the mode of expression of <u>l'histoire evenementielle</u>, of political history at the level of the individual, rather than longer-term socioeconomic <u>conjonctures</u> and <u>structure</u> governed by impersonal forces (White 1984, pp.8-10). This would appear to be a clear case of stone-throwing by the denizens of glass houses: as Kinser (1981, p.101) has observed "What is true of "structure" is true of all of Braudel's categorizing terms: they form part of a system of argument and are nevertheless presented as facts, as "realities" for which no argument is needed. In the end, Braudel's "concreteness" is metaphysical...".

(b) For the various tribes of structuralists (anthropological, psychological and semiological) the criticism is directed at the ideological role of narrative in distinguishing 'historical' societies in the western tradition from the pre-historical, primitive world, in lauding western 'humanism' and in conditioning its citizenry through a systematic, subliminal substitution of a constructed 'meaning' for a supposedly value-free, described 'reality' (White 1984, pp.10-15). Particularly insofar as this touches upon prehistory, White concurs: "The distinction between a humanity, or kind of culture, or kind of society that is historical and another that is nonhistorical is not of the same order as the distinction between two periods of time in the development of the human species: pre-historical and historical...And this is for at least two reasons: one is that the human species does not enter into "history" only "in part". The very notion of "human species" implies that if any part of it exists "in history" the whole of it does. Second, the notion of the entrance "into history" of any part of the human species could not properly be conceived as a purely intramural operation, a transformation that certain cultures or societies undergo that is merely internal to themselves...This is that panorama of the domination of the so-called "higher" civilizations over their "neolithic" subject cultures and the "expansion" of Western civilization over the globe that is the subject of the standard narrative of the world-history written from the point of view of "historical" cultures (White 1984, pp.31-32).

(c) For Anglo-American analytical philosophers and continental semiologists alike, narrative can be termed a discursive code. Disagreement centres on the extent to which this serves a purely communicative function, meeting correspondence as well as coherence criteria of truth-value, as argued by the former. "On this view, the narrative form of the discourse is only a medium (author's emphasis) for the message, having no more truth value or informational content than any other formal structure, such as a logical syllogism, a metaphorical figure, or a mathematical equation...in the historical narrative, it is the "content" alone that has "truth-value". All else is "ornament" (White 1984, pp.15-18). For the semiologist, however, historical texts partake of artistry (shades of early Croce!): "an artistic text carries much more "information" than does the scientific text, because the former disposes (sic) more codes and more levels of encodation than does the latter...It is this complex multi-layeredness of discourse and its consequent capacity to bear a wide variety of interpretation of its meaning that the performance model of discourse (author's emphasis) seeks to illuminate...a discourse is regarded as an apparatus for the production of meaning (author's emphasis)...As thus envisaged, the "content" of the discourse consists as much of its form as it does of whatever information might be extracted from a reading of it...A chronicle, however, is not a narrative, even if it contains the same set of facts as its informational content. And this is because a narrative discourse performs (author's emphasis) differently from a chronicle" (White 1984, pp.19-20).

(d) So far, so good. Where the average archaeologist may begin to boggle is over White's discussion of historiography as a literature differentiated by the reality of its content from that of the poesy with which it shares a formal identity. White denies that it is the reality of the events which is decisive: "The historical narrative does not, as narrative, dispel false beliefs about the past, human life, the nature of the community, and so on; what it does is test the capacity of a culture's fictions to endow real events with the kinds of meaning that literature displays to consciousness through its fashioning of patterns of 'imaginary' events...Therefore, rather than regarding every historical narrative as "myth" or "ideological" in nature, it is more correct to regard it as allegorical, which is to to say it says one thing and means another" (White 1984, pp.21-22).

In particular, a narrative must by definition mean more than the 'literal' truth of the sum of its parts (as a chronicle would): by way of exemplification, White discusses Marx's representation of the 18th Brumaire of Louis Buonaparte as a 'source': "There is no way in which we could conclude on logical grounds than any set of "real" events is a farce...The transition is effected by a process of transliteration, in which events originally transcribed in the code of chronicle are re-transcribed in the literary code of the farce" (White 1984, pp.22-24). The analytical philosophers, in reducing narrative to a 'literalist' paraphrase, regarding the poetic troping as 'mere' figurative speech embedded in a straightforward representation of real events, made a serious 'category mistake': "But in this process of literalization, what gets left out is precisely those elements of figuration, tropes and figures of thought, as the rhetoricians call them, without which the narrativization of real events, the transformation of chronicle into a story, could ever be effected...A narrative account is always a figurative account, an allegory...and it is only a modern prejudice against allegoryy, or, what amounts to the same thing, a scientific prejudice in favour of literalism, that obscures this fact to many modern analysts of historical narrative." (White 1984, pp.24-25).
(e) The final straw for the common-sense digger is likely to be White's acceptance of the hermeneutic 'metaphysics of narrativity' of Ricoeur, who "has assigned historical narrative to the category of symbolic discourse, which is to say, a discourse whose principal force derives neither from its informational content nor from its rhetorical effect, but rather from its imagistic function" (White 1984, p.28). For Ricoeur "in the writing of historical text, the aim in view should be to represent (human) events in such a way that their status as part of meaningful wholes will be made manifest...to understand historical actions, then, is to "grasp together", as parts of wholes that are "meaningful", the intentions motivating action, the actions themselves, and their consequences as reflected in social and cultural contexts; this is effected by their "configuration" in a "plot" " (White 1984, pp.26-27). Passing over the triads of "degrees of organization of time" (within-time-ness, historicality and deep temporality) and the three "representations of time in consciousness", which this writer at least confesses to having found obfuscatory and verging on the mystic (why do temporalities always occur in threes?) one is confronted by White's distillation of Ricoeur's metaphysics: "narrative is more than a mode of explanation, more than a code, and much more than a vehicle for conveying information. It is not a discursive strategy ot tactic that the historian may or may not use, according to some pragmatic aim or purpose. It is a means of symbolizing events without which their "historicality" cannot be indicated." (White 1984, p.29). In effect, narrative conveys a "fuller meaning" than scientific explanation, precisely in translating both the "reality" and the "mystery" of history (White 1984, pp.29-30), in achieving that "mythistory" which the 1985 President of the American Historical Association is not afraid to exhort his colleagues to attempt (McNeill 1986).

Only a fool would wish to pretend that the above is more than a trifingly small sample of the range and richness of scholarly thought currently concerned with the scientific status and form of the historical discipline and with the narrative mode of historiography. There is therefore no point in attempting to find a spurious consistency or in concocting some lowest common denominator among the articles summarised: the point is simply to demonstrate the existence, the extent and the intensity of the debate, and to urge archaeologists both to contribute and to draw inspiration from it. Two conclusions stand out to my mind:

(i) There is an impression of a convergence at least at the conceptual level between the historians proper on the one hand and the historians and sociologists of Science on the other, exemplified by the frequency of recourse to the metaphor of 'tinkering' and by the extension of hermeneutics even to observational-experimental Science (Fores 1983; Knapp 1984; Knorr 1979; Rudwick 1985; White 1984). In effect, if there is no agreement as to the identity of the various disciplines, there is a growing sense of a common semantic field within which differences can be unambiguously identified;

(ii) precisely because the <u>fin de siecle</u> polemic among French sociologists, historians, geographers <u>et al</u> has so often been described as a classic case of wholesale misunderstanding among the participants (cf Lewthwaite in press a) a full-scale explication of the controversy, of the sort carried out by Rudwick for the Devonian episode (Rudwick 1985) in the history of geology, would greatly benefit those engaged in similar, continuing polemics; I realise of course that the parallelism is far from exact, since the <u>fin de siecle</u> controversy was never 'solved' in the same way.

Knowledge in forms?

The question of the variously 'scientific' or 'historical' affiliation of archaeological activity can be approached from yet another perspective: that of the partitioning of 'knowledge' into distinct discursive domains or 'forms of knowledge' as advocated by Prof. P.H.Hirst. In his first formulation, Hirst (1974a, p.44) argues that "by a form of knowledge is meant a distinct way in which our experience becomes structured round the use of accepted public symbols. The symbols thus having public meaning, their use is in some way testable against experience and there is the progressive development of series of tested symbolic expressions." Hirst outlined four "related distinguishing features":

(i) "central concepts peculiar in character to the form" which (ii) "form a network of possible relationships in which experience can be understood"; (iii) testability against experience "in accordance with particular criteria that are peculiar to the form"; (iv) "particular techniques and skills for exploring experience and testing their distinctive expressions "resulting in the amassing of all the symbolically expressed knowledge that we now have in the arts and the sciences" (Hirst 1974a, p.44).

Hirst initially suggested seven such "distinct disciplines or forms of knowledge": mathematics, physical sciences, human sciences, history, religion, literature and the fine arts, lastly philosophy; no place for archaeology! (Hirst 1974a, p.46). The question therefore arises: if the concept of distinct forms of knowledge be admitted, to which should archaeology be assigned?

Hirst further postulated the existence of other classifications of knowledge: "First there are those organisations which are not themselves disciplines or subdivisions of any discipline. They are formed by building together round specific objects, or phenomena, or practical pursuits, knowledge that is characteristically rooted elsewhere in more than one discipline...these organisations are not concerned, as the disciplines are, to validate any one logically distinct form of expression. They are not concerned with developing a particular structuring or experience. They are held together simply by their subject matter, drawing on all forms of knowledge that can contribute to them. Geography, as the study of man in relation to his environment, is an example of a theoretical study of this kind..." (Hirst 1974a, p.46). Is archaeology, perhaps, "held together simply by its subject matter", drawing on the fundamental forms eclectically? In a second formulation, Hirst deletes the fourth distinguishing feature, recognising as the elements demarcating mutually irreducible categories of knowledge only (i) concepts (ii) logical structure and (iii) truth criteria (Hirst 1974b, pp.84-85). At the same time, Hirst admits that "the question that for some time worried me considerably was the character of history and the social sciences...it now seems to me that both history and the social sciences...are...logically complex in character. In part they are concerned with truths that are matters of empirical observation and experiment...and...are therefore of the strictly physical science variety. That some of these truths are about the past...is irrelevant for the purposes of the fundamental distinctions being made...on the other hand, history and some of the social sciences are not concerned simply with an understanding of observable phenomena in terms of physical causation, but with explanations of human behaviour in terms of intentions, will, hopes, beliefs, etc. The concepts, logical structure and truth criteria of propositions of this latter kind are, I would argue, different from, and not reducible to, those of the former kind. For this reason it now seems to me correct to speak of one form of knowledge as being concerned with the truths of the physical world and another as concerned with truths of a mental or personal kind...In these terms, I now think it best not to refer to history or the social sciences in any statement of forms of knowledge as such. These pursuits like so many other so-called 'subjects' may well be concerned with truths of several different logical kinds and only detailed examination can show to what extent any one example of such a subject is or is not logically complex and in what ways (Hirst 1974b, pp.86-87).

It is impossible to answer any of the questions raised by Hirst without considering the obvious derivation of his classification from the idealist philosophy of Oakeshott, exemplified by his Experience and its Modes, a philosophy, like Collingwood's, strongly and specifically committed to an explanation of the historical mode of experience and discourse. In a recent review, Boucher (1984) provides not only a specific guide to Oakeshott's very individual thinking but an overview of the British idealist tradition from Bradley onwards:

(i) the idealists privileged consciousness over external 'reality', regarding 'facts' as constructs of an open mind rather than sensorily perceived data, the truth value of which would necessarily depend on internal (coherence, comprehensiveness) rather than external (correspondence) criteria, but the meaning and intersubjective understanding of which would be contextual within a given universe of discourse or form of experience: "A mode, then, is a system of ideas, or imaginings, and history is one such mode, capable of generating conclusions appropriate to itself: it is the "arbiter of fact"... Each mode, however, is limited and abstract, and the truth which it generates is conditional upon unquestioned postulates"; learned like a language, each is nevertheless capable of generating unique utterances (Boucher 1984, pp.194-202).

(ii) according to Oakeshott, the historian firstly constructs "historical events" out of the "situational identities" of "artifacts identified exclusively as survivals from the past" through the composition of a "contiguity" of antecedent "contingencies" and, secondly, the further composition of such events into historical individuals ("concrete universals") abstracted from experience on the grounds of their "character": "the historian is compelled to assert what the evidence obliges hium to believe...A history must be able to accomodate all the evidence, and stands condemned when contradicted by an alternative account which seems to incorporate the evidence more satisfactorily. The conclusions of the historical manner of enquiry are an invitation to imagine a certain set of arrangements or compositions of circumstances which have not survived, as inferred from placing together the available evidence in a particular way, and not in another" (Boucher 1984, pp.203-214).

(iii) However, this "order of enquiry" only applies to "procedures or practices, which are recognised as exhibitions of intelligence" as opposed to processes "such as the formation of rock, or the evolution of the universe "explicable in terms of general laws, or mechanical causes and effects" (Boucher 1984, p.203). It is perhaps this crucial premise which explains Hirst's reformulation of the historical and social-scientific forms of knowledge in terms of the domains of the physical and mental, and which allows us to reflect on the extent to which archaeology may be another 'logically complex' discipline - again, perhaps more complex, perhaps fuller in potential meaning, than physical science.

As this essay is intended only to provoke a full discussion of such neglected issues (in no way to resolve them!) and as I have certainly not achieved any persoanl gestalt out of the ams of reading involved I do not wish at this point to embrace an idealist position any more warmly than my limited and inchoate sampling permits, but I must confess that on reading and reflecting on Boucher's description of Oakeshott's argument I felt intuitively that it approximated to what I had been doing for some time in respect of the development of a so-called 'Filter Model' of the transition to food production in the western Mediterranean: from its genesis as an epiphenomenon of a doctoral thesis on 'Transhumant and Sedentary Pastoralism in Earlier Corsican Prehistory' in 1984-5 the model has been elaborated in a series of articles through a process which is closer to the constructionist ideal of "conferring conditonal intelligibility upon" a "whole mass of undifferentiated human activity" than any other (Lewthwaite in press b,c,d,e). Certainly I did not attempt to re-enact the thoughts of the human actors involved, a la Collingwood!

By way of conclusion to this section and linking argument to the next, it is perhaps appropriate to suggest that archaeologists in general reveal more about their actual working practices - the candid appraisal of how they do in fact go about solving their day-to-day puzzles whether in the field, the laboratory or the study - in a manner commensurate with a philosophical analysis. One suspects that it would turn out that archaeology is a logically complex field of discourse, a loose grouping in human and institutional terms of logically distinct activities conveying very little real meaning between one sub-discipline or research school and another.

Works in practice

'Know thyself' - a maxim of the Delphic oracle - has been strangely little heeded by the archaeologist, whose sense of inferiority and failure appears to have driven him to try to know practically everything else in sight, but most particularly Science. Insofar as I have learned from observing myself and my colleagues at work I would draw the following lessons:

(i) archaeology is anthropological! Science-oriented archaeologists perceive the discipline as consisting of a single vertical dimension, the observer observing the observed; in practice the discipline is discursive and dialectical. More often than not the work-practice of the discipline consists of the constant reordering and reinterpretation of a quantum of only slowly accreting data in the light of some perceptual 'gain' - an article, a conversation with a colleague, an 'aha experience' (Gruber 1981); for the remainder of the time, the observers are observing each other, the observed serving only as a reservoir of referents to a dialogue between self-conscious pragmatists;

(ii) archaeologists are a territorial species with only a very weakly developed linear dominance hierarchy: their behaviour typically consists of the acquisition of a niche (defined by region, period and perhaps topic or speciality) and its monopolisation (access to knowledge) combined with a keen sense of performance-evaluation of the competition, particularly in terms of the epideictic activity disguised none too ingenuously as conferences. I am not enough of a dilettante in palaeontology or zoological theory to pursue the topic in any depth, but I have alwyas perceived the speciation of New or critical-theoretical Archaeologists as the equivalent in terms of disciplinary evolution to the appearance of heterophages among mammals. I am of course a vegetarian myself;

(iii) insofar as archaeologists perceive failure in puzzle-solving to be endemic, it is as much a case of structural (institutional) and behavioural as of ideational deficiencies. The territorial behaviour alluded to inhibits the coagulation of 'core-sets' around theoretical 'hot-spots' as perceived by Collins (1981) and instanced by Rudwick (1985): nothing short of a major conflagration threatening the entire structure seems able to rouse and assemble the sleepers from their strictly segregated repose. Mostly, hot-spots remain confined to one chamber, where they are safely doused with the cold water of incomprehension or blanketed by the apathy of the occupant. Paradigmatic change is minimal, because paradigms are never fully explored, because archaeologists do not like practising normal, puzzle-solving science; as in the case of political theory (Wolin 1968, pp.139-43); it's a case of too many would-be-hot-shots, not enough hot-spots. Personally, I refuse to believe that a major puzzle such as the Impressed Ware or Beaker phenomena, or the exploration and application of the Sherrattian Secondary Products Revolution, would long resist the massed cerebral pressure of the profession - if only it could be cajoled into concert.

(iv) One highly efficient - because highly coercive - system of accelerating the full working-out of paradigms, or at the very least of puzzles within a widely shared paradigm, would be the institution of a number of 'research schools' as defined by J.B.Morrell. I quote at length from Geison's (1981, pp.21-24) summary of this concept, as I have not seen the original: "an 'ideal' research school requires a 'charismatic' director, whose leadership qualities are 'most effectively exerted in informal pre-bureaucratic contexts', and who already enjoys a solid (but usually not spectacular) reputation for original research. This director must then conceive, and his school must sustain, an effective research programme in which a distinctive approach is initially brought to bear on one or a very few sharply delimited problems within a broader field, often a new or growing one. Since future success depends partly on the speed with which reliable early results can be achieved, the director's initial research programme should be sufficiently accessible to new recruits to allow them to make an easy transition from learning to independent research. Particularly effective towards this end is a set of fast, simple and reliable effective techniques that can be easily applied to previously unexplored areas of research. As the research programme is successfully prosecuted, certain areas of research become recognised as the 'property' of the school, which can then move on to other related problems.

At a less immediately cognitive level, a successful research school depends upon a readily available pool of talented potential recruits (a condition that presumably works to the advantage of university-based schools), and upon the director's capacity to inspire loyalty, social cohesion, and esprits de corps among his students. To produce a school that extends beyond himself, the director must nurture early independence, self-reliance, and ambition among his students, especially by encouraging them to publish under their own names at an early stage in their career - even if and when the director has contributed importantly to the published research. Towards this end it is important that the school has easy access to (or, better still, control over) publication outlets for the work of its younger members. When their training is completed, students who have already published will have enhanced their candidacy for positions elsewhere, and if the director has 'placement power' in his discipline he can do much to ensure their employment in a propitious academic setting, thereby further extending the reputation and influence of his school. Finally, to achieve sustained success, the director must have or must quickly acquire sufficient power in the local and national institutional setting to secure adequate financial support and an institutionalized commitment to his enterprise."

Such a research school does not so far as I know exist in Britain; in some respects those Cambridge phenomena known to the initiated as the Higgery or the Coggies may have approximated to the concept in some respects, but not in many which are clearly significant. More to the point, it is difficult to see how such a school could develop in the circumstances of the present day and proximate future. On the one hand, the dearth of research studentships and fellowships and the lack of well paid career prospects deters the high-quality graduates from entering the system; on the other hand, an ageing and excessively dispersed body of lecturers will be less and less capable of either doing the research themselves or directing others except in an 'artisanal' guise of one master, one apprentice. Dissatisfaction is liable to become all the more acute, as we have acquired the model of the American university without the means (cf Berdoulay 1980).

ACADEMIC INSTITUTIONALISATION

In the necessarily short space remaining, the question of the institutionalisation of archaeology will be examined further. If it is accepted, for the reasons given above, that many of the ailments which already afflict or will come to afflict archaeology are institutional rather than personal or ideational, then it follows that a comparison with the historical processes implicated in the institutionalisation of other disciplines will be highly instructive. Only an outline sketch can be provided here, in places mere hypothesis, to be expanded in some other paper.

There are two levels to the problematic:
(i) that of the progressive adoption throughout Europe and America of the German or more specifically 'Humboldtian' model of the university in the course of the nineteenth century as the principal locus of research activity in the humanities and social sciences. Obviously this differed from country to country: France is a particularly well documented case (eg Zeldin 1967);
(ii) that of the sequential institutionalisation od the various disciplines as they developed, those earliest installed effectively impeding the emplacement of later specialities inside the university. Once again, the French case is the best documented: the early and effective institutionalisation of history favoured geography over sociology. First, with regard to the overall question, Ben-David and Zloczower (1962, pp.47-48) argue that "the still prevalent conception or 'idea' of the University, as well as the definition of the professor's role, originated in Germany during the 19th century. It was, furthermore, in the German universities more than anywhere else, that the main fields of scientific enquiry developed into 'disciplines', possessing specialised methodologies and systematically determined contents." The three principles of the Humboldtian model first given concrete form as the University of Berlin in 1910 were the unity of knowledge (wissenschaft), education by knowledge and the unity of teaching and research (Lundgreen 1980). In practice, unity soon broke down because of the unequal rates of expansion and differentiation of the fields of teaching and research: as only one chair (ordinarius) was allowed per discipline, initially, the swelling body of academics had to specialise, resulting ina speciation of disciplines; only later did expansion take place increasingly through junior staff appointments (extraordinarius, privatdozent). Indeed, Ben-David and Zloczower (1962, p.57) argue that the idea was really a myth or rather an ideology, idealising a compromise between the interests of academics and those of the state. Innovation in nineteenth-century Germany, as in the twentieth-century U.S.A., was intensified by the institutional structure - personal mobility, an open jobs market and a high level of competition among a score of universities.

Secondly, it is agreed that the 'idea' of the German university, the impression of its superiority, nevertheless exerted a powerful influence on the reformist elements within the moribund French academic institutions of the early nineteenth century: "A highly idealized image of German universities served to symbolize a variety of goods and aspirations" (Weisz 1983a, pp.61-3); "the idea of universities was equally significant, because it served as a powerful symbol for the collective advancement of the academic community" sufficiently elastic to mobilize all elements of this community to concerted lobbying with the political establishment, itself to undergo major changes in the course of the century. In particular, "assuming the identity of 'scientists' was a useful way of distinguishing academics...such distinctions strengthened claims to elite status" (Weisz 1983a, p.80)..."This professional autonomy based on scientific expertise was at once a prerequisite for improving the status of the academic profession and a public recognition of the technical competence which its members possessed exclusively (Weisz 1983a, pp.70-71), science being defined as "all knowledge that emerged from a critical and rational examination of concrete reality" (Weisz 1983a, p.76).

In effect, research productivity began to supplement, even supplant, the mere transmission of a cultural heritage as the criterion of the academic: "furthermore, any potential conflicts between the two roles (sc. of teaching and research) were resolved by regarding students exclusively as future scientists and scholars" (Weisz 1983a, p.81) although "the dilemma specific to French higher education was the structural inability to separate training for the research role from training for the liberal and teaching professions (Weisz 1983a, p.221). In effect, the higher teaching degree (agregation) remains as important in some disciplines in France as the university doctorate (troisieme cycle) because of this very dilemma.

Thirdly, the realisation of the Germanic ideal through the creation in 1896-7 of fifteen regional universities out of the various faculties of letters, science, law and medicine in the provinces retained two typically French attributes of the Napoleonic system (centralised control and the unity of careers in secondary and tertiary education) while intensifying the third - the massive concentration of the academic population into the Parisian faculties such as that of Letters (Sorbonne), the specialist Grandes Ecoles and the influential academic hothouse of the Ecole Normale Superieure (Fox and Weisz 1980; Smith 1982; Weisz 1983a). During the second half of the century, employment in a provincial faculty chair - necessitating a doctorate - was regarded as less attractive than that in a Paris lycee, because of the attractions of the cultural facilities in the capital (Karady 1980). As the century wore on and faculties blossomed into universities, more and more were drawn into a lifecycle of centripetal and centrifugal motion: schooling in a provincial lycee was followed by cramming in a Parisian super-lycee (Henri IV, Louis-le-Grand) in order to enter the Ecole Normale Superieure if at all possible and gain the licence and agregation which permitted appointment to a provincial lycee. The doctorate would be written up during the tenure of this, leading to a post in a provincial faculty and (rarely before 40) a final return to Paris as maitre de conferences and in time and with luck a full professor (Singer 1982). The successful accomplishment of the full cycle often depended on participation in a social institution peculiar to France - the cercle or cluster of scholars forming a web of clientelage around a powerful universitaire (mandarin) (Clark 1971, 1973, pp.66-92).

Such was the background against which the strategies were pursued by the academic historians which resulted in the position of history as science maitresse long before the much acclaimed hegemony of the Annales; they have been described by Keylor (1975). In the 1860's, history was still largely written as literature or philosophy by amateurs; at the local level societes des savants busied themselves with the 'ornamental learning' and 'bourgeois enthusiasms' of local history, geography and archaeology (Fox 1980, pp.242-244). "Therefore, but especially from the Second Empire, the professional academic became ever more obtrusive in French intellectual life. Not only did he 'take over' disciplines which had previously been pursued as avocations: he also refashioned these disciplines in ways that made them more esoteric and so less accessible to the self-taught" (Fox 1980, p.244). A symbol of this was the founding of the research-oriented Ecole Pratique des Hautes Etudes by Victor Duruy in 1868. The key group of historians who effected the transformation - Gabriel Monod, Ernest Lavisse, Alfred

Rambaud - linked themselves with the move to reform the universities and the pressure group Societe de l'Enseignement Superieure even before the disastrous defeat at Sedan and the trauma of the Commune. Thereafter, they simultaneously held up German 'scientific' history as a model for emulation (most of the reformists had studied there), as an erudition verging on pedantry and a cult of the disjunct fact which the Gallic flair for synthesis and rhetoric would surpass, and as a dangerous body of chauvinistic propaganda to be combatted. History was institutionalised first and foremost as the promoter of patriotism and revanchism.

As the spirit of revanche faded, a new justification for the priority of history as science maitresse was found - the need to establish and then defend the social consensus and ideology of the Third Republic against its enemies to the right and the left. This culminated in the near-unanimous allegiance of the Sorbonne elite historians in the Dreyfusard cause early in this century (Keylor 1975, p.147). Throughout the period, Monod, Lavisse, Rambaud and the Dean of the Sorbonne, Alfred Croiset, succeeded in penetrating the key ministerial councils and the bureaucracy of education, in supervising and writing textbooks down to primary school level, and in the case of the super-patriot Lavisse, in churning out masses of patriotic propaganda of all genres. Above all, they valued the scientific status of history. In this respect it is interesting that Keylor observes how "Having had little or no exposure to scientific studies, a defect in their intellectual formation which they bitterly regretted in later life, they tended to romanticize and idealise the scientific method without ever having acquired a precise notion of what it comprised. Their resentment at the inadequacy of their own educational experience led many of them to adopt a simplistic conception of the scientific method which embodied many of the very virtues that they themselves had sorely missed.." (Keylor 1975, p.7). Their reaction against philosophy, rhetoric and the classics - the culture generale - had a practical as well as an emotional edge, for one of the major hindrances to the definitive monopolisation of historiography was the existence of an independent non-academic intelligentsia which flourished in the quality press and the Institut and which popularised an alternative methodology and ideology. The academic historians such as Sagnac, Caron and Aulard not only excluded such authors from the major periodicals and societies but in the case of Aulard went so far as to "decree that any candidate for the doctorate at the Sorbonne who dared to cite Taine as an authority on a question of historical fact would be disqualified (Keylor 1975, pp.173-178). Finally, Alfred Croiset joined Durkheim in his crusade to eliminate the classical humanist learning from secondary education to favour of a utilitarian (today they would say 'socially relevant'...) historical-sociological emphasis on historical relativity and gradual progress (Keylor 1975, pp.190-194). Democratic republicanism needed no false propaganda, according to Croiset, since it was concomitant with any scientific faith (Keylor 1975, pp.195-196). Finally, Clarles Seignobos argued that "the scientific study of human institutions, therefore, implied not the unprejudiced search for truth, but rather the creation of a blueprint for inculcating students with a preordained set of verities revealed by the enlightened educational elite of the nation, whose mission it is to rescue the citizenry from the darkness to which it had been consigned by its human frailties...the democratic masses had already learned to accept the existence of a scientific tradition because of the material benefits that they received from scientific research" (Keylor 1975, p.200). Thus the strategy of institutionalism had its quid pro quo in the ideological services which the historical establishment were willing to preform for the republican ideal.

Towards the end of the last century, a policy of curriculum broadening was introduced through the initiative of the long-term head of the education department of the Ministry of Public Instruction, Louis Liard: "Descriptive geography, experimental psychology, and educational science...were among the first disciplines that took advantage of such administrative promotion. Sociology merely followed suit... Some disciplines satisfied such strong needs, both social and frequently academic, that their teaching was quickly generalised to every faculty of letters (fifteen in all). Human geography, as developed under Vidal de La Blache, is a case in point (Karady 1985, p.75). The academic legitimation of geography is a topic to which I have already drawn attention elsewhere (Lewthwaite in press a); it is perhaps only necessary to emphasize how the institutionalisation of the discipline was achieved as much through Vidal's opportunity as a historian of great eminence and as a tutor with much influence among the students at the Rue d'Ulm to achieve a sort of intra-mural coup, whereas the sociologists of the Durkheim <u>cercle</u> had to fight their way in against the resistance of the historical mandarinate; moreover their success was transient, unlike that of geography. In effect, I would argue that the very ease of institutionalisation of Vidalian geography proved to be its undoing: major theoretical problems were to remain inadequately or ambiguously resolved, buried beneath Vidal's classically polished prose (cf. Robic 1976) only to leave French geography cruelly exposed to the Anglo-American-Scandanavian paradigm of the New Geography in the post-World War Two era; at present it is ranked lowest among the humanities and social sciences in academic prestige (Bourdieu 1984, p.223).

This is an opportune occasion to review the contrasting case of the sociologists in a little more detail, taking advantage of the researches of Clark (1973) and of the essays edited by Besnard (1983b), particularly those of Weisz (1983b) and Karady (1983). Clark (1973, p.242) concludes his study by making the following basic points: "For most new fields to develop, three fundamental elements are essential: good ideas to build on, talented individuals, and adequate institutional support. A solid core of ideas, some sort of paradigm, must be sufficiently original by institutionalized criteria to command respect from persons in adjoining fields....A sizable critical mass to evaluate research, advance careers, and award grants is essential if universalistic values are to become operating institutional norms." Weisz (1983b) who explicitly rejects Clark's concept of 'clusters' or <u>cercles</u> - and Karady (1983), who does not - examining the institutionalisation of sociology in greater depth, bring out four key variables:

(1) Firstly, the socio-political context and the perceived ideological affinities of the would-be discipline: "The ideological destabilisation of the ruling classes became such that their members increasingly tend to act, in all major issues, as agents of competitive establishments....In this situation the search for a discourse on society invested with the authority of objective science gained a strategic importance for the French power elites, regardless of allegiance" (Karady 1983, p.73); "Representatives of many disciplines in search of financial resources or more elevated status invoked criteria of ideological efficacy...social scientists...unquestionably staked their institutional destiny most directly on the ideological

needs of the republic" (Weisz 1983b, p.91);..."the rise of academic social science in France was closely related to the ideological needs of the moderate republic; the new diciplines were perceived as a weapon of combat against socialism" (Weisz 1983b, p.115). The Durkheimians were careful not to overplay their personal sympathies with the Radicals: "Thus the Durkheimians' exploitation of their acquired social legitimacy hardly meant that the cluster's work would rest on political commentary...This caution proved to be...a vital asset which contributed to their success in the other than social-ideological market" (Karady 1983, p.74).

(ii) Secondly, the academic capital already accumulated by the Durkheim cercle: "The academic legitimacy of the Durkheimians was based almost exclusively on the prestigious academic backgrounds of its sponsors and members. Most of them held the agregation and a substantial number were educated in the Ecole Normale Superieure's liberal-arts section, the most prestigious training the French university system could provide...these assets virtually guaranteed job security within the state secondary- or higher education systems and, consequently, created rather high career prospects...the same high academic placement also determined the limits of their disciplinary orientations. Agreges and normaliens, they were mostly trained as philosophers or, less often, historials, hence the Durkheimians' bias for philosophy and history. These academic assets operated as general factors objectively determining the essential options open to the group in its search for recognition as legitimate representatives of a new academic profession. To obtain their collective goals the Durkheimians did their utmost to capitalize on their initial investments in Academe and to maximize the value of the institutional capital at their disposal...The 'conversion' from a classical to a sociological disciplinary pursuit and a move to make it recognised as a new academic speciality were the crucial elements of these strategies. Their success depended largely on the exceptional intellectual performance of assigned scholarly tasks, no less than on direct pressure on ministerial decision-makers and, above all, on the confirmation of the discipline's identity, autonomy and utility as they were defined by Durkheim" (Karady 1983: 75-77).

(iii) The strengths and weaknesses of established disciplines like philosophy, out of which sociology emerged, and history or law, with which it clashed. With regard to the first, Karady (1983: 78-79) observes that "The ever-worsening critical state of academic philosophy in France at the end of the nineteenth century left it unarmed in the combat with the new sociologists"...Nevertheless "Durkheimian social theory unwittingly came to take an important place in the field of academic philosophy" for the following reasons: "First sociology, by its internal cohesion, its scope and its ambition, was the most powerful discourse in an intellectual field...Secondly, philosophy was by far the most prestigious disciplinary association accessible to the new sociology because it was at the top of the traditional disciplinary hierarchy...Thirdly, the quasi-exclusiveness of the Durkheimians' association with philosophy in the faculties reflected to a large extent their failure to be received as equal partners by the dominant disciplines...Enjoying a well established disciplinary autonomy...these disciplines were reluctant to share their monopoly over professional teaching in the faculties...due, in part, to the fact that the academic market at the turn of the century...was far more favourable to historians and geographers than to philosophers. The latter could hope to improve their professional expectations by disciplinary conversion to sociology, while the others could not. These circumstances, among others, determined the disciplinary pattern of recruitment in the Durkheimian cluster, and explain its limited attraction for others than those trained in philosophy".

With regard to the second sort of relationship, Weisz (1983b: 93-98) notes that "the development of the social sciences in the faculties of letters brought these into conflict with the schools of law. Sociology was viewed with an especially jaundiced eye and Durkheim's appointment to a course of social science in 1887 provoked intense protests...Academic jurists recognized that the development of sociology in other institutions threatened their monopoly over the social sciences; but they were not especially eager to introduce the discipline to the schools of law. Essentially, they wished that it would go away." Weisz outlines at greater length than can be summarized here the tortuous negotiations with the historians over the chair of the history of social economy at the Sorbonne, featuring such notable personalities as Ernest Lavisse, 'Dick May' (sc. Mlle Jeanne Weil), Count Aldebert de Chambrun, Emile Durkheim, Alfred Espinas, Celestin Bougle, Henri Hauser and Alphonse Aulard in a characteristic saga of chicanery (Weisz 1983b: 98-115). Simiand's onslaught against the historians has of course been noted above (cf. Besnard 1983a).
(iv) The intellectual performance of the early sociologists as polemicists for their own discipline at the expense of others'. Karady (1983: 79-87) argues that the 'legitimation strategy' of the Durkheimians broke with established tradition to a significant degree, while maintaining certain tactical conventions: "reliance upon the existing or supposed legitimacy of the discipline abroad (most notably in Germany) and the strict observance of academic practices." The tactical innovations were: firstly, the elaboration of a distinctive 'imperialist' paradigm which "tended to picture the various disciplines as mere purveyors of facts, while reserving for itself the noble tasks of interpretation and explanation"; secondly, the construction of a collective organ of criticism, the Annee Sociologique, which "fixed, in practical terms, the thematic outline of the discipline", serving simultaneously as a master-key to gain "entry to every major contemporary debate related to the social sciences" in a multitude of other disciplines, as a shield and sword against rivals ("ignoring the self-proclaimed sociological works of their contemporaries...they tended actively to disregard the latter as a means of disassociating themselves from the competing groups") and as a rope thrown to established schools abroad ("the Durkheimians elevated their own status by association with highbrow international companionship"); finally, "they violated several fundamental rules that implicitly governed intellectual production in the nineteenth-century university. They ignored the traditional separation of the humanities and law, failed to respect disciplinary specialization, refused to give preference to the culturally established subjects, attacked the ethnocentrism inherent in the choice of scholarly activities and in value-judgements, scorned the exclusiveness of individual (as against collective) work, and gave little credence to the thematic unity of teaching and research...these transgressions proved to be strategic in the Durkheimians' long-term struggle for the legitimation of sociology in Academe...their success depended upon the extent to which the university system was able to change and except hitherto unacceptable elements."

Nevertheless, theirs was a Pyrrhic victory: "Durkheim was in every respect an influential grand universitaire. But in the narrow institutional sphere of sociology, he reigned supreme precisely because his domain was so tiny" (Weisz 1983b: 114). Karady (1983: 77-79) concludes that "since it did not lead to one of the useful teaching degrees (licence, agregation) sociology emerged first as an auxiliary discipline at best, a superfluous academic luxury at worst...thus, apparently, sociology achieved its complete academic recognition, but on conditions that effectively deprived the discipline of its independence and made it, institutionally a minor ancilla philosophiae. Ironically, the Durkheimians' success in this respect turned into a semi-failure. Its institutional dependence was counter-productive...Though significant for the personal career of group members, the Durkheimians' collective academic success was in fact less than conspicuous...sociology's entrenchment in philosophy turned, by its own operative logic, into a self-perpetuating process. Because the Durkheimians recruited scholars from among young agreges de philosophie, they failed to achieve the group's original ideal, which was to become the meeting point of all the social scientific disciplines. Without adequate institutional prospects, the Durkheimians' synthetic ambition was soon reduced, for all intents and purposes, to a career pattern that excluded students of the social sciences proper (historians, geographers)...Even while Durkheim lived, there was an apparent contradiction between the aggregation of disciplinary subject-matters, upon which rested the intellectual legitimacy of the new science, and its rather narrow academic base." In short, the Durkheimian cercle embraced that balance of ephemeral glory and dynastic disappointment which has been the lot of the palace eunuch throughout history.

A Hypothesis: the Unduly Easy Confinement of Archaeology

To the reader who has followed the argument so far, I offer a hypothesis which I hope others, better qualified and with more time and resources, may wish to test: that archaeology in Britain entered Academe without running the gauntlet of criticism from other disciplines which might have proved beneficial in the long run. It has often been observed that those ex-colonies which had to fight for their independence have fared better since than those to whom freedom was painlessly granted through an administrative concession; in my opinion, the lack of theoretical awareness endemic in the discipline, which so nettled David Clarke, has been due in no small way to its uncontroversial institutionalisation, which was in turn a function of its ideological acceptability to the Establishment and relative popularity among the former elite. In a superficial way, one can detect two modes of institutional acceptance:

(i) 'Historical accidents' such as the endowment of Cambridge University with the Disney chair and, in particular, the peculiarly specific conditions which John, Lord Abercromby, attached to the chair of Prehistoric Archaeology with which he endowed Edinburgh (Green 1981: 46, 56-57; Trigger 1980: 60-61). Only two years after the publication of the first edition of The Dawn of European Civilization, V. Gordon Childe was elevated from his librarianship at the Royal Anthropological Institute into this office, whereby he was enabled to pursue research of sufficient depth and breadth as to provide prehistoric archaeology with a paradigm which survived his own decease;

(ii) 'Wheelering and dealing', i.e. the adroit acquisition of resources from the great and good, in the manner typified by R.E. Mortimer Wheeler (the archaeologist as Nietzschean superman, or the genesis of archaeology and the decline of morals?) of whom John Bintliff has observed, in a review of Jacquetta Hawkes' biography (Bintliff 1982) that "... his great flair for manipulating the national media on behalf of the hitherto poorly-funded discipline...created a respectable 'profession' out of a gentlemanly diversion, squeezing out the dilettanti in favour or Wheeler's own creation - the university-trained, career archaeologist...Wheeler was in effect able to persuade the country...to offer archaeology a worthy place in the university curricula, in the share-out of government funds...As a result we can thank him for the London Institute of Archaeology and the rapid spread of the subject into other universities over the world, for the shift from minor to major state funding in Britain for the profession, and for the diet of 'popular archaeology'...through his secretaryship of the British Academy, he stirred to effective life that vital network of British Schools of Archaeology" - in short, he established the material structure within which ideational paradigms such as Childe's could thrive and a critical mass of practitioners capable of eventual self-renewal accumulate.

The researches of Fox, Rudwick, Besnard, Karady and Weisz provide models for the fine-grained explicitly interrogative historiography of archaeology which has yet to be written, but which is essential for the self-appraisal overdue in a discipline which has perhaps got more reason than most to adhere to the Delphic imperative alluded to earlier.

PEDAGOGIC PROSPECTION

Nursing the Doctors

In effect, there is very little point in introspection as to the historical or scientific affinities of the discipline if the real question is one of simple survival. Archaeological activity may go on in the HBMC/ English Heritage and its counterparts in Wales, Scotland and Ulster, in the museums, county offices and units - but does not of itself ensure an institutional future in Academe for archaeologists. Given that university appointments are regarded primarily in terms of teaching students, with a specific ratio or quotient thereof, while pure research posts are doled out in miserly numbers and in competition with more prestigious disciplines with the humanities and social sciences, the maintenance of a critical mass of practitioners capable of carrying out minimal professional obligations depends on the volume of students drawn to undergraduate courses, and to a lesser extent on their quality. Would-be students can hardly be unaware of the impossibility of their pursuing a professional career in academic archaeology, even if they make the further investment in a research degree: supply has risen, demand declined to zero. Before passing on to consider how undergraduate numbers might be held steady or even increased under such circumstances, it is advisable to contemplate the present and future status of the Ph.D. and its pursuer.

The days of the open-ended sink-or-swim peripatetic Ph.D. are clearly over; not a bad thing, many would say (I write as a coelocanth miraculously preserved in the viscously fluid lower depths of Caius from the Higgsian era). Research studentships will evermore resemble assistantships, in that the supervisory and/ or grant-giving body will specify not only the duration but the content as is already the case with SERC studentships: the tragedy of the old system was that the freedom and originality of the student (perceived as a future practitioner) were prized above the utility of the research performed; as a 'tinkerer' hired to carry out some specific task within a collective and cumulative research project the postgrad. might have performed some permanently useful service. Instead, the archaeology of the 'boom years' when the Cambridge department had at any one time some 75 research students on its books has left behind as its enduring memorial a Holywood-Western Main Street facade of a film-set, a Brazilian Gold Rush opera-house encroached upon by lianas and leeches: a monuments to unfulfilled paradigms and puzzles that passed in the night. Such eager little beavers as will be set to the Ph.D.s of the future will obviously be unable to lecture over a broad enough field to be employable unless they receive futher grooming through compulsory courses in a 'Graduate School' with or without low-level experience as teaching assistants. Two questions arise: firstly, do we regard the Research Fellowship as the logical means of allowing the Ph.D. of the future to catch up with the Ph.D. of the past, to learn how to do original research? Or, alternatively, do we reserve Research Fellowships/Readerships for established lecturers on a sabbatical or on a competitive basis, to enable them to perform teaching and research duties in turn? Secondly, will such research be carried out in individual departments under individual supervisors - on a sort of patron-client basis - or under the aegis of a national-level research institute charged with their care and cultivation as a cadre, within coherent research policy? Much will depend on rationalisation plans currently being negotiated.

To return to the question of undergraduates, it is obvious that the only way to maintain, let alone raise, the volume of students is to offer courses which reduce the vocational aspects to a minimum (i.e. the study of excavational, post-excavational and contextual techniques). Such 'Professional' training should logically be carried out by a professional body, competent to award diplomas for achieving recognised standards after experience accumulated in on-the-job practice under certified 'master-craftsmen' at recognised training centres, whether before, during or after the completion of a degree. Archaeological science and related courses which require the use of laboratories and access to collections are a different matter; it is perhaps more a matter of preventing their proliferation and the resultant dumping of a mass of semi-trained laboratory proletariat onto a market which is likely to provide about as much secure and adequately paid employment as the latifundia of Andalusia or central Sicily to their casual labourers.

Archaeology within the universities must therefore validate itself as an academic subject in terms of the idea of a liberal education (Hirst 1974a), competing with better established and more prestigious disciplines for a share of the humanities and social sciences market. This in turn requires that archaeologists decide what it is that they are teaching - what 'facts', what 'judgement', what is to be the level of competence of the student, in terms of a form or at least a field of knowledge (Hirst 1974a, b, c).

As Hirst (1976c: 117-120) points out, "In all subjects surely, we do not just want the learning of a string of propositions...What we want is that pupils shall begin, however embryonically, to think historically, scientifically or mathematically; to think in the way distinctive of the particular subject involved and even to achieve some style and imagination in doing so...This is in fact to say that the effective teaching of a subject necessarily depends on knowing certain features which characterize it, which can be disclosed only by logical analysis of the meaning of 'historical thinking'". It seems to me that Hirst's admonitions raise two questions:
(i) The necessity of defining archaeology as a field or form of

knowledge. I have explained above what this entails; it is here a question of the logical consequences of such a definition in terms of the organization of teaching within universities;
(ii) The necessity of examining course structures and introductory textbooks in the light of Hirst's (1974c: 129-130) reflections on John Dewey's concept of a 'logical order'.

With regard to the first question, the rationalisation of archaeology departments currently contemplated only makes sense if it is assumed that archaeology really is an independent form of knowledge, rather than a 'field' like geography or perhaps merely a sector within a historico-social scientific whole. Current thinking makes sense only if it is indeed a form, if indeed specialist teaching and research will advance only when a critical mass of practitioners is assembled on a given campus, in order that they offer not only a broad core curriculum but a range of course options sufficiently diverse to attract the potential consumers. On the research front, one might suppose that such a critical mass could be coerced, cajoled or convinced the more readily to undertake useful integrated projects of genuine value; but there is that old saw about horses and water. Colleagues who have no time for each other apart are unlikely to submit to the ego-bypass surgery or altruism-grafting necessary to effect the postulated harmonious interaction. There was, I recall, a spate of attempted couplings of Giant Pandas not so long ago along similar lines. Whatever became of them?

If, however, the professional consensus were to admit that archaeology is more of a field than a form of knowledge then there would be much more to be said for staying put and indulging in some long-overdue bricolage with such departments of history, geography, and the social sciences, the earth sciences or whatever is available in order to build up courses which feature 'archaeology' as a body of data and a rather slimmer skeletal theory relevant to a wide range of multi- or inter- disciplinary issues such as the emergence of social complexity, life in small-scale societies, man's use and misuse of the earth, etc. It is perhaps symptomatic of the inferiority complex which grips archaeologists that they assume that they would inevitably lose out in such an arrangement.

Respecting the second, I confess to only belatedly discovering the sheer inadequacy of the literature available, on appointment to teach an introductory course in later prehistory (sc. the transition to food production and the emergence of the first civilisations in the western Old World). If one conceives of the problem as one of Saussurian langue and parole - of importing the learning necessary for the novice eventually to be capable of uttering a statement which is grammatically correct, meaningful, yet a unique creation, then the procedure followed in, say, setting an essay topic resembles rather a demand that a certain piece of particularly tortuous figurative prose be translated by someone with no knowledge of the tongue, provided only with a range of grammars and dictionaries all of which disagreed over fundamentals. There are of course some singularly primitive primers, mostly written as pot-boilers by pensioners, handbooks on digging techniques, collections of readings from the pens of the illustrious, and the excellent if eccentric work-book of Daniels and David (1982), but there is nothing which looks as if a concern with pedagogic practicality let alone the philosophy of education, had entered its author's head. How many high-quality students are thus needlessly alienated? Bearing in mind Hirst's suggestion that there may not be in fact a precise logical order in which the network of concepts must be taught, that learning may proceed by a sort of 'mapping' of unfamiliar terrain - elsewhere he uses the analogy of finding one's way through a strange town - presumably towards the achievement of a personal '_gestalt_' experience of the logical grammar of a subject, the workbook approach offers the most promising channel. It seems to me that we could turn once more to the history of the discipline to identify historically significant 'hotspots' or paradigmatic puzzles, whether at the level of the interpretation of the features on a site or the genesis of a civilization, in order to scale them down, simplify them, perhaps incorporate an element of the 'playfulness' characteristic of mammalian learning patterns, in order to allow the student to assimilate through a series of such '_gestalt_' experiments ranging from the conceptually simple to the complex.

CONCLUSIONS

(i) If my interpretation of the working practice of scientists and historians is at all valid, then archaeologists must pay more attention to constructs and concepts and less to the concrete. In other words, they must stop blaming the inadequacy of the data for all their woes and concentrate on 'tinkering' with a few well chosen models in order to achieve solid if unspectacular success through a series of infinitesimally small adjustments to some sort of given 'asset'. The rule is to bring such an initial model into the best possible agreement with observations of reality without sacrificing logical coherence at any point. The trouble with what passes for theoretical debates at present is that concepts and models have a shorter half-life than some fundamental particles; the established _rite de passage_ of the research student attending his first conference is to prove his virility by demonstrating at least as much skill in vituperation against all previous solutions as in upholding the paternity of his own. The difference between physics and political science is very much that of the hedgehog and the fox, respectively - the physicists concentrate on the One Big Truth while the political scientists rush around being clever Renard and end up foxing themselves - to mix Berlin with La Fontaine! No-one supposes that scientists do this because they are cleverer, or because they want to - the institutionalisation of their discipline has simply operated so as to select for an optimum between creativity and 'normal science', achieved very largely through hierarchic discipline and the control of assets.

(ii) such activity must be carried out collectively through a restructuration of research, conference and publication norms. Conferences should resemble workshops in scientific disciplines; they should take place in well stocked libraries issuing a draft resolution on a given puzzle at the end of a given period. Publication reviews are another Achilles' heel of the discipline, as is the peer refereeal of manuscripts in a small profession where all are either friends or enemies. Nothing will stop the individual scholar from having the right to go off and write his own big book, but something must be done to cope with the increasingly chaotic channelling of ephemeral knowledge and the creation of a consensus on given topics, if only for pedagogic purposes and in order to impress competitive disciplines.

(iii) Archaeology possesses a body of characteristically archaeological puzzling every bit as important as its material assets. Archaeologists should spend less time demonstrating the manipulation of materials, more that of ideas, if the high-quality students are to be drawn to the subject and high-quality graduates attractive to business turned out. Academics should be freed from the chores of training excavations, field trips, museum visits and teaching 'practicals' unless they specificallly choose to undertake such activity as extra-curricular professional commitments under separate auspices. The rest can thereby concentrate on teaching and on major collective research projects.

(iv) Teaching cannot be regarded as a tiresome nuisance to be sped through if it is to be the key to disciplinary survival and perhaps expansion (imperialism). The abundant literature on the Machiavellian intrigues of fin de siecle French disciplines should go a long way towards providing stolid Anglo-Saxon minds with the necessary strategies and rhetorical tropes. In principle, there is no logical reason why strong archaeological departments should not swallow weak groupings of historians, geographers, etc.; it is time for a release from institutional confinement.

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BIBLIOGRAPHY

Ben-David,J. and Zloczower,A., 1962 - "Universities and Academic Systems in Modern Societies" Archives Europeennes de Sociologie, Vol.3, pp.45-84.

Berdoulay,V., 1970 - "Professionnalisation et institutionnalisation de la geographie" Organon, Vol.14, pp.149-156.

Besnard,P., 1983a - "The Epistemological domain: Francois Simiand", pp.248-262, in Besnard, P. (Ed), The Sociological Domain. Cambridge University Press, Cambridge and Maison des Sciences de L'Homme, Paris.

Besnard,P., 1983b - The Sociological Domain. Cambridge Univerity Press, Cambridge.

Bintliff,J.L., 1982 - "Founder's Day in the age of 'disciplinary self-consciousness', (Review of Mortimer Wheeler, "Adventurer in Archaeology", by Jacquetta Hawkes), History Today, Vol.32(10), pp.54-55.

Birnbaum,N., 1978 - "The Annales school and social theory" Review Vol.1(3-4), pp.225-235.

Boucher,D., 1984 - "The creation of the past: British idealism and Michael Oakeshott's philosophy of history" History and Theory, Vol.23(2), pp.193-214.

Bourdieu,P., 1984 - Homo Academicus. Editions de Minuit, Paris.

Braudel,F., 1958 - "La longue duree" Annales: Economies, Societes, Civilisations, Vol. 13, pp.725-753.

Braudel,F., 1972 - "Personal testimony" Journal of Modern History, Vol. 44, pp.448-467.

Burguiere,A., 1979 - "Histoire d'une histoire: la naissance des annales" Annales: Economies, Societes, Civilisations, Vol.34(6), pp.1347-1359.

Cedronio,M., 1977 - Profilo delle "Annales" attraverso le pagine delle "Annales",pp.1-72, in M.Cedronio, F.Diaz, C.Russo (Eds) Storiografia Francese di Ieri e di Oggi. Naples, Guida editore.

Cedronio,M., 1981 - "Croce, Gentile, la storia e le scienze sociali" Melanges de l'Ecole Francais de Rome Moyen-Age, Temp, Modernes, Vol.93(1), pp.361-400.

Collins,H.M., 1981 - "The place of the 'core-set' in modern science: social contingency with methodological propriety in science" History of Science, Vol.19, pp.6-19.

Clark,T.N., 1971 - "Le patron et son cercle: clef de l'Universite francaise" Revue Francaise de Sociologie, Vol.12, pp.19-39.

Clark,T.N., 1971 - Prophets and Patrons. Harvard University Press, Cambridge, Mass.

Clarke,D.L., 1973 - "Archaeology: the loss of innocence" Antiquity, Vol.47, pp.6-18.

Daniels,S. and David,N., 1982 - The Archaeology Workbook. Edinburgh University Press, Edinburgh.

Figlio,K.M., 1976 - "The metaphor of organisation: an historiographic perspective on the bio-medical sciences of the early nineteenth century" History of Science Vol.14, pp.17-53.

Finley,M.I., 1971 - "Archaeology and History" Daedalus Sp. No. Historical Studies Today Vol.100(1), pp.168-86

Flannery,K.V., 1973 - "Archaeology with a capital S", pp.47-53, in C.L. Redman (Ed), Research and Theory in Current Archaeology. J.Wiley and Sons, New York.

Flannery,K.V., 1976 - The Early Mesoamerican Village. Academic Press, New York.

Flannery,K.V., 1982 - "The Golden Marshalltown: a parable for the archaeology of the 1980's" American Anthropologist Vol.84(2), pp.265-78.

Fores,M., 1983 - "Science and the 'Neolithic paradox'" History of Science Vol.84(2), pp.141-63

Fores,M., 1984 - "Constructed science and the seventeenth century 'Revolution'" History of Science Vol.22, pp.217-44

Fox,R., 1980 - "The savant confronts his peers: scientific societies in France, 1815-1914", pp.241-282, in Fox,R. and Weiz,G. (Eds) The Organisation of Science and Technology in France, 1808-1914. Cambridge University Press, Cambridge and Maison des Sciences de l'Homme, Paris.

Fox,R. and Weisz,G., 1980 - "Introduction: the institutional basis of French science in the nineteenth century", pp.1-28, in Fox,R. and Weisz,G. (Eds), The Organisation of Science and Technology in France, 1808-1914. Cambridge University Press, Cambridge and Maison des Sciences de l'Homme, Paris

Furet,F., 1975 - "From narrative history to history as a problem" Diogenes, (English Edition), Vol.89, pp.106-23

Geison,G.L., 1986 - "Scientific change, emerging specialities, and research schools" History of Science, Vol.19, pp.20-40.

Green,S., 1981 - Prehistorian. A biography of V.Gordon Childe. Moonraker Press, Bradford on Avon.

Gruber,H.E., 1981 - "On the relationship between 'Aha Experiences' and the construction of ideas" History of Science Vol.19, pp.41-59.

Hirst,P.H., 1974a - "Liberal education and the nature of knowledge", pp.30-53, in Hirst,P.H. Knowledge and the Curriculum. Routledge and Kegan Paul, London.

Hirst,P.H., 1974b - "The forms of knowledge re-visited", pp.84-100, in Hirst,P.H. Knowledge and the Curriculum. Routledge and Kegan Paul, London.

Hirst, P.H., 1974c - "The logical and psychological aspects of teaching a subject", pp.116-131, in Hirst,P.H. Knowledge and the Curriculum. Routledge and Kegan Paul, London.

Huppert,G., 1973 - "La liberte du cerveau: notes on the psychology of historical erudition", pp.267-277, in Melanges en Honneur de Fernand Braudel, t.2. Privat, Toulouse.

Hughes,H.S., 1959 - Consciousness and Society. MacGibbon and Kee, London.

Karady,V., 1980 - "Educational qualifications and university careers in France", pp.95-124, in Fox,R. and Weisz,G., The Organisation of Science and Technology in France, 1808-1914. Cambridge University Press, Cambridge and Maison des Sciences de L'Homme, Paris.

Karady,V., 1983 - "The Durkheimians in Academe: a reconsideration", pp.71-89, in Besnard,P. (Ed), The Sociological Domain. Cambridge University Press, Cambridge.

Keylor,W.R., 1975 - Academy and Community. Harvard University Press, Cambridge, Mass.

Kinser,S., 1981 - "Annaliste paradigm? The geohistorical structuralism of Fernand Braudel" American Historical Review, Vol.86, pp.63-105.

Knapp,P., 1984 - "Can social theory escape from history? Views of history in social science" History and Theory, Vol.23(1), pp.34-52.

Knorr,K.D., 1979 - "Tinkering toward success: a prelude to a theory of scientific practice" Theory and Society, Vol.8, pp.347-376.

Laudan,L., 1982 - "Two puzzles about science: reflections on some crises in the philosophy and sociology of science" Minerva, Vol.20(3-4), pp.253-268.

Leuilliot,P., 1973 - "Aux origines des "Annales d'Histoire Economique et Sociale" (1928)". Contribution a l'historiographie francaise, in Melanges en Honneur de Fernand Braudel, t.2, Toulouse, Privat, pp.317-324.

Levi-Strauss,C., 1972 - The Savage Mind. Weidenfeld & Nicholson, London, (French original 1962, Paris, Plon.).

Lewthwaite,J.G., in press a - "Trial by duree: the application of historical-geographical concepts to the archaeology of settlment on Corsica and Sardinia", in J.L.Bintliff, D.Davidson, E.Grant and M.Shackley (Eds), Conceptual Issues in Environmental Archaeology. Edinburgh University Press, Edinburgh.

Lewthwaite,J.G., in press b - "The transition to food production: a Mediterranean perspective", in M.Zvelebil (Ed) Hunters in Transition. Cambridge University Press, Cambridge.

Lewthwaite,J.G., in press c - "Isolating the residuals: the Mesolithic basis of man-animal relationships on the Mediterranean islands", in C.Bonsall and A.Morrison (Eds), The Mesolithic in Europe. Edinburgh University Press, Edinburgh.

Lewthwaite,J.G., in press d - "From Menton to the Mondego in three steps: application of the availability model to the transition to food production in Occitania, Mediterranean Spain and southern Portugal" Arqueologia (Oporto), 13 (Easter 1986).

Lewthwaite,J.G., in press e - "The beginnings of food production in the western Mediterranean: the case for the 'Corse Filter'" Scientific American.

Lundgreen,P., 1980 - "The organisation of science and technology in France: a German perspective", pp.311-32, in R.Fox and G.Weisz, The Organisation of Science and Technology in France 1808-1914. The University Press, Cambridge and Maison des Sciences de l'Homme, Paris.

Massicotte,G., 1981. L'Histoire Probleme. La Methode de Lucien Febvre. Edisem' St. Hyacinthe, Quebec and Maloine, Paris.

McLemore,L., 1984 - "Max Weber's defence of historical inquiry" History and Theory Vol.23(3), pp.277-295.

McNeill,W.H., 1986 - "Mythistory, or truth, myth, history and historians" American Historical Review. Vol.91(1), pp.1-10.

Revel,J., 1978 - "The Annales: continuities and discontinuities" Review, Vol.1(3-4), pp.9-18.

Revel,J., 1979 - "Histoire et science sociale: les paradigmes des Annales" Annales: Economies, Societes, Civilisations, Vol.34(6), pp.1360-1376.

Robic,M.C., 1976 - La Conception de la Geographie Humaine chez Vidal de la Blache apres les Principes de Geographie Humaine. (Cahiers de Fontenay 4 Geographie). E.N.S., Fontenay-aux-Roses.

Rudwick,M.J.S., 1985 - The Great Devonian Controversy. Chicago University Press, Chicago.

Siegel,M., 1970 - "Henri Berr's Revue de Synthese Historique" History and Theory, Vol.9, pp.322-324.

Singer,B., 1982 - "The ascendancy of the Sorbonne: the relations between centre and periphery in the academic order of the Third French Republic" Minerva Vol.20(3-4), pp.269-300.

Smith,M.J., 1982 - The Ecole Normale Superieure and the Third Republic. SUNY Press, Albany.

Stone,L., 1971 - "Prosopography" Daedalus Sp. No. Historical Studies Today, Vol.100(1), pp.46-79.

Trigger,B.G., 1980 - Gordon Childe. Revolutions in Archaeology. Thames and Hudson, London.

Weisz,G., 1983a - The Emergence of Modern Universities in France 1863-1914. Princeton University Press, Princeton.

Weisz,G., 1983b - "The republican ideology and the social sciences; the Durkheimians and the history of social economy at the Sorbonne", pp.90-119, in P.Besnard (Ed) The Sociological Domain. Cambridge University Press, Cambridge and Maison des Sciences de l'Homme, Paris.

Wesseling,H.L., 1978 - "The Annales school and the writing of contemporary history" Review Vol.1(3-4), pp.185-94.

White,H., 1984 - "The question of narrative in contemporary historical theory" History and Theory Vol.23(1), pp.1-23.

Wolin,S.S., 1968 - "Paradigms and political theories", pp.125-152 in P.King and B.C.Parekh (Eds) Politics and Experience. Essays Prsented to Professor Michael Oakeshott on the Occasion of his Retirement. Cambridge University Press, Cambridge.

Zeldin,T., 1967 - "Higher education in France, 1848-1940" Journal of Contemporary History Vol.2(3), pp.53-80.

EXPLANATION AT THE METHOD AND THEORY INTERFACE

C.F.Gaffney

During the last ten years archaeology has drifted into two factions; the split is said to be between the most esoteric of theoreticians and the grind of the full-time excavator. In retrospect, it is not only felt that the full- time excavator had so much pressure due to the growing concept, and reality, of rescue archaeology so that he or she could not contribute to the wider issues, but also that the theoreticians were producing theories and hypotheses that were impossible to test. Of course, this divide arose during a boom period, not only in funds but also in theory for archaeology. It was an academically 'free' period where, in many cases, it did not matter what the practitioners of our discipline did as long as we were seen to be doing something. As funds for both unit and university projects have been cut back, it has been realised how unacceptable the resulting pot-pourri is to the paymasters and the profession as a whole. Hodder (1984) described this divide as an unnatural one. He correctly based his reasoning on the fact that archaeological theories are not 'tested' on archaeological data, but merely on other theories. Following on from that statement, Hodder argued that if data is viewed as inherently dependant on theory, then field work must be valued as an interpretative experience, rather than as a practical experience. Therefore we are all theoreticians. This confident theoretical stance is a positive by-product from the 'New Archaeology' of the Sixties. Certainly this school raised the conciousness of a whole generation of archaeologists, but the 'New Archaeology' also "...raised an image of Man the passive and efficient animal controlled by laws which cannot be usurped...The human past legitimated and made universal the principles of the technocratic West." (Hodder 1984, p.30). Not unnaturally there has been a healthy reaction against the law and order brigade in archaeology.

Pryor's observation that Middle Range Theory was "a sorry attempt to cloak honest interpretive fieldwork in a cap and gown of respectibilty" (Pryor 1983, p.99) was deserved. The implication that the area where the vast majority of university educated graduates employed in professional archaeology are working as "low" was somewhat negative, if not self-defeating. For perhaps the first time in its brief history our discipline has a professional body working day-in, day-out in the field; we are a profession that can improve methodology on the ground, not for its own sake, but to approach relevant and topical theoretical problems. Pryor has suggested that we are limited only if we attempt to structure our information within models that are exclusively derived from non-archaeological sources (Pryor 1983). Undoubtedly archaeologists have listened with a sympathetic ear to the view that most theoretical and anthropological studies, because of their limited time span, give us models with no context in our studies of greater time depth. However, we must be wary of demanding simply explicitly archaeological theory, because in the real world hardly any of our data are interpreted via only archaeological inference. As a subset of the study of man, we have to go further afield and experience the full diversity of his adaptation to the environment. In doing so we must relate to many disciplines if we are to reconcile ivory towers with empirical barricades.

THE NEW ARCHAEOLOGY AND THE SCIENTIFIC METHOD

Historically, the advent of the New Archaeology brought with it a myth-like concept called the scientific method (eg Watson et al, 1971). This method aimed to lower the input of illogical assumptions and fanciful imagination in archaeological thinking. Such imagination, it was argued, could only be personal and convey little in terms of general or covering laws. To be sure when such fancies erode our ability to explain archaeological phenomena then they must be rejected if we are to be regarded with more respect than the 'fringe' archaeology that we readily condemn. Most archaeologists are aware that philosophers of science disagree on a definition of the scientific method and that among such scholars there are at least three major schools of thought that attempt to explain how science works. They are:-

1. The Hempelian school, which looks at the 'probability' of an event being true in the light of experience.

2. The Popperian school of falsification which requires criteria of 'improbability' or 'a degree of falsification'.

3. The group of behavioural concepts of explanation proposed by Kuhn and others, who suggest that the rejection of an hypothesis or theory is a matter of faith rather than logic.

(After Yorston, forth)

However, what most archaeologists would regard as the scientific method is no more than the classic 17th century scheme suggested by Bacon (Richards 1983, p.128). In such a method data are, via observations, recorded, and an hypothesis may be deduced; a prediction made from such an hypothesis is testable by more observations. Hence from observations, we may deduce generalised, universal laws. Within science we are told that there is a degree of falsification emeshed within the overall concept of 'the scientific method' (Yorston, forth). In many scientists' eyes the two rationalist schools mentioned above are inherently linked. An alternative to these 'cook book' methods is to be found in the literature from the 'behavioural school'. This school suggests that we have to broaden out the scientific perspective and treat theories as structures (eg Kuhn 1962, Lakatos 1978).

What, then, is the evidence for suggesting that any of the above verification routes should be used in archaeology? The most quoted justification stems from Kaplan's discussion of the work of Mill, the 19th century positivist, who said "There can be no fundamental logical differences between the principles according to which we explain natural changes and the way we explain social changes" (Quoted in Winch 1958, p.71). Kaplan (1964) has interpreted this as meaning that the same logic of justification and the same syntax may be employed in all disciplines. This does not imply that either the techniques or the conceptual content of explanation should be similar. Indeed, those choices are undoubtedly pragmatic and need justifications of their own. What was believed in the Sixties, was that if we could embrace the methodology that had made science work, then archaeology would be more scientific and, at least at a conceptual level, would have a methodology that could cope with the quantitative revolution that the discipline was carried away by.

However, an increasingly attractive alternative to the above three validation schools suggests that these methods are too simple and do not explain the essentially anarchistic way in which science works (eg Feyerabend 1975). By definition, such a school cannot provide archaeologists with easy rules to follow, but it would increase the freedom of the individual through the removal of all methodological constraints. It would allow us to make implicit ideology explicit and to recognise that archaeological knowledge cannot be divorced from the archaeological community from which it was derived. Yorston (forth) states that scientists accept what amounts to no more than the public image of science, based on inductivism with falsification embellishments; nor do they make a conscious effort to keep to such a methodology. The implication for archaeology is that if we want to have a scientific methodology then perhaps we ought to ignore what scientists say they do. Science as a whole has concentrated on small, easily defined problems. A result is that science does not want, or perhaps does not need, to go beyond simple mechanical determinism and has not got a methodology that is appropriate for archaeology. It may be argued that archaeologists have turned away from simple determinism. However, we must ask ourselves, is this reversal a product of rational argument against the low-level, 'Mickey Mouse' laws (Flannery 1976)? Indeed is this reversal a product of the traditionally conservative nature of our discipline. If so did our discipline ever really change?

THE RATIONALITY OF EXPLANATION

Toulmin (1960) has suggested that the need for explanation originates from a reaction of suprise to some experience which has been generated by a conflict between our expectations in a given situation and our actual experience of it. The major problem with such a definition is that in a practical discipline we are attempting to elucidate substantive archaeological problems and not simply attempting, as the philosophers of science have done, to elucidate the form of experience (Harvey 1969). Undoubtedly, experience and explanation must go hand in hand, but to what extent? We have to examine the relationships between our experience and the positive avenues that may be pursued and explained by such experience. It is only through such reasoning that we can credibly focus upon the significance of philosophy to solve archaeological problems. At best we can suggest that explanations are only acceptable if they answer questions within a paradigm (or set of rules) common to practising archaeologists.

At a philosophical level, Braithwaite has suggested that the aim of scientific explanation "...is to establish general laws covering the behaviour of empirical events or objects with which the science in question is concerned, and thereby to enable us to connect together our knowledge of the separately known events, and to make reliable predictions of events yet unknown." (Braithwaite 1960, p.1). In reality, the major problem is that the criteria for judging satisfaction are highly subjective. Scientists are said to judge whether or not explanations are reasonable by setting up conventions or 'norms'. Although these levels ensure similar conclusions from similar results, they are dependant upon the initial setting up of levels. Not only is there a problem of applicability in a semi-quantitative subject such as archaeology, but such tests do not necessarily convey any broader sense of explanation.

The major problem at the method and theory interface is therefore a conceptual one. The problem concerns the control of the symbiotic relationship between data and theory. It is probably true, as Harvey has suggested, that all disciplines experience periods where they have become too enamoured of questions that have no real interpretation in terms of everyday experience or that they have simply set up unrealistic questions with a neat mechanism for providing seemingly satisfactory, if equally unreal answers (Harvey 1969, p.13). Archaeology certainly does have to compromise, either due to difficulties of controlling the phenomena being examined or, alternatively, on account of the poor theoretical development at the interface. It is evident that the syntax and goals of 'theoretical' and 'field' archaeologists must be intrinsically linked if theoretical modelling is to be efficiently pursued. Some confusion exist over the definition of models and theory. Terminolology is often loosely used, creating the false impression that models and theory are coextensive. If the two concepts could be equated then we would call all theorising or model building theory. Models are simply an interpretation of a theory; or more likely, partial, formalised expressions of a theory. They should be pieces of machinery that relate observations to theoretical ideas (Clarke 1978). Due to the fact that any definition of 'models' depends upon the function of a model, and that such functions are infinite it would appear that a more useful definition would be that "Essentially models are hypotheses which simplify complex observations whilst offering a largely accurate predictive framework structuring these observations - usefully separating 'noise' from information. Which aspects are noise and which count as information are solely dependant upon the frame of reference of the model." (Clarke 1978, p.31)

If such models work efficiently, then, models may act as cognitive, visualising devices, as organisational or classificatory devices, as explanatory devices, or as constructional devices in the search for or extension of existing theory (Harvey 1969, p.141). However, the way in which we construct and use archaeological models has been criticised in recent years. Pryor (1983) has criticised the archaeological inapplicability of most models, whilst Evans (1984) has suggested that the archaeological models are cognitively inadequate. As archaeological goals and models become increasingly separated, then so must the elusive concept of explanation give way to imagination. The problem may be simplified since not all models are theories, nor are all theories models, hence a theory may exist in limbo; in fact, only in the imagination of theoreticians. The format in which many theories are stated often makes the theories redundant, as they have no obvious practical consequences.

The rationale for models in archaeology has been clearly linked with the quantification or 'scientific' horizon of arts subjects during the sixties (eg Clarke 1968, Harvey 1969). However, if such models of analysis as described in the Sixties and the early Seventies are not realistic then our goal of the explanation of archaeological phenomena may prove illusive. It is time to reassess the whole foundations of model building which are inexorably linked with scientific method. Morgan, in his critique of 'Explanation in Archaeology' (Redman et al 1971), criticised the narrowness of one analytical method. Indeed, "Scientists should be pragmatists - whatever leads to knowledge is worthwhile, and that which interferes with the search for knowledge is to be abandoned" (Morgan 1973, p.275). Moreover, it must be stressed that archaeologists also must be flexible if they wish to explain the complex phenomena with which they deal (Gaffney, forth). The very foundations of scientific model building are increasingly under attack and are being eroded. The author does not wish to argue against a scientific archaeology, or against an archaeology that is challenging in its use of theoretical concepts. However, it is thought that a reconsideration of the fundamental 'building blocks' of our discipline would be of great value. The challenge is to explain archaeological data in a satisfying manner to public and colleagues alike. Such a challenge is worthy of a discipline that seeks to understand the whole of man's past.

BIBLIOGRAPHY

Braithwaite,R.B., 1960 - Scientific Explanation. Harper Torchbooks, New York.

Clarke,D.L., 1968 - Analytical Archaeology. Methuen, London.

Clarke,D.L., 1978 - Analytical Archaeology (Revised Edition, R. Chapman (Ed)). Methuen, London.

Evans,C., 1984 - "Tradition and the cultural landscape: an archaeology of place" Archaeological Review from Cambridge, Vol.4(1), pp.80-94.

Feyerabend,P., 1975 - Against Method; Outline of an Anarchistic Theory of Knowledge. NLB, London.

Flannery,K., 1975 - The Early Mesoamerican Village. Academic Press, London.

Gaffney,C.F., forth - "Pragmatism: a working philosophy", in C.F.Gaffney and V.L.Gaffney (Eds).

Gaffney,C.F. and Gaffney,V.L. (Eds), forth - Pragmatic Archaeology: Theory in Crisis?

Harvey,D., 1969 - Explanation in Geography. Edward Arnold, London.

Hodder,I., 1984 - "Archaeology in 1984" Antiquity, Vol.58, pp.25-32.

Kaplan,A., 1964 - The Conduct of Inquiry. Chandler, San Francisco.

Kuhn,T.S., 1970 - The Structure of Scientific Revolutions. Chicago, Chicago University Press.

Lakatos,I., 1978 - Methodology of Scientifc Research Programmes. Cambridge University Press, Cambridge.

Morgan,C.G., 1973 - "Archaeology and Explanation" World Archaeology, Vol.4 (3), pp.259-276.

Pryor,F., 1983 - "Talking Heads" Scottish Archaeological Review, Vol.2(2), pp.98-100.

Richards,J.D., 1983 - "Against Archaeological Method" Scottish Archaeological Review, Vol.2(2), pp.128-133.

Toulmin,S., 1960 - Reason in Ethics. Cambridge University Press, Cambridge.

Watson,P.J., Leblanc,S.A. and Redman,C.L., 1971 - Explanation in Archaeology: an explicitly scientific approach. Columbia University Press, New York and London.

Winch,P., 1958 - The Idea of a Social Science and its Relation to Philosophy. Routledge and Kegan Paul, London.

Yorston,R., forth - "Theory and Method: some observations from a scientist", in C.F. Gaffney and V.L. Gaffney (Eds), forth.

HUMAN SOCIOBIOLOGY AND ARCHAEOLOGY

J.Chapman

INTRODUCTION

The crystallization of the synthetic field of discourse known as 'Sociobiology' in the mid-1970's provided an instant battleground for politicians, radicals, revolutionaries, biologists, ecologists, sociologists, anthropologists and historians. The acrimonious exchanges of the 1970's (Allen et al 1975; Sociobiology Study Group 1976; Wilson 1975) have for the most part been avoided in the 1980's (but see Leach's (1981) review of Lumsden and Wilson 1981) in favour of the more patient research required to test and refine the earlier conceptualizations. Now that aggressive eidetic competition has given way to a more peaceful colonization strategy, it is clear that sociobiology has much to offer her sibling human sciences as a source of adaptive hypotheses about human social action and organisation. For the human problem, one must leave 'genes' in the formula because no-one has produced a human without them yet!

In this article, it is my aim to examine the origins of sociobiology, discuss the relationship between general sociobiology and human sociobiology and analyse the relationship of the individual to group dynamics and cultural change. In the final section, I shall discuss the theoretical links between sociobiology and other current archaeological paradigms.

HISTORICAL BACKGROUND

It was Sir Julian Huxley, writing in 1923, who was one of the earliest to discuss with enthusiasm the idea of "the correlation of biology with sociology" (Huxley 1923). Misled down the blind alley of social Darwinism, pre-war biologists trod basically divergent paths from social scientists who focused on other more sociological paradigms. But in the period of the 1930's - 1950's, concepts such as 'cultural evolutionism' became part of the anthropological mainstream

(Bintliff 1984; Slaughter 1984), despite the dangers of using the terms cultural and natural 'selection' and cultural and biological 'evolution' as interchangeable or analogous descriptors (for a fuller discussion of these dangers, see Dunnell 1980). Despite the early introduction of ecological concepts and taxonomic procedures into archaeology, these approaches remained unquantified for much of this century (cf. Doran and Hodson 1975). This was one of the reasons why archaeology could not benefit from the great theoretical advance of the 1960's in biology - the so-called 'Modern-Synthesis' (Note 1). The Modern Synthesis represented an integration of numerical taxonomy, population genetics and quantitative ecology into neo-Darwinian evolutionary theory (Wilson 1975). In this breakthrough, the full theoretical force of natural selection at the individual level was applied to the hitherto more static fields of taxonomy and ecology in order to provide a dynamic and quantified baseline for evolutionary theory. The success of the new synthesis can be judged from the innovative mathematical modelling of animal behaviour which became the hallmark of the sociobiology of the 1960's and early 1970's (Hamilton 1964; Trivers 1971; Clutton-Brock 1974).

The theoretical advances of early non-human sociobiological research can be summarised under two headings: the level at which natural selection operates, and the explanation of altruism by kin selection. With respect to the former, there are four levels at which natural selection can potentially operate:- (1) kin selection (where natural selection operates at the level of the gene, not only on individuals), (2) individual selection, (3) inter-demic or group selection (where selection acts upon the entire breeding population) and (4) species/families level (where entire species or groups of species are selected on). The debate over which level was more important has lasted many decades (Fisher 1930; Huxley 1942). The Wynne-Edwards hypothesis of group selection was developed to explain the widespread tendency of animal population sizes to be regulated rather than fluctuate wildly (Wynne-Edwards 1962; see Wright 1922; Carr-Saunders 1922). But Williams (1966) was able to demonstrate that those individuals who chose a selfish reproductive strategy of non-reproductive restraint would be selected for faster than the time necessary for group selection to come into effect. Williams' study laid the basis for the primacy of individual selection over other slower-moving forms of selection, without solving the problem of co-operative behaviour between individuals.

One of the major developments in sociobiological theory is the discovery of the principle of kin selection. Hamilton (1964) proposed that altruistic behaviour by ego in favour of consanguineous relations could actually carry greater inclusive fitness for ego's genes than many forms of selfish behaviour, since the fitness of kin relations could be substantially improved by such altruism. Since Hamilton's work the kin selection hypothesis has been tested against a wide variety of animal behaviour, and has received much support (for aggression, see Colman 1982; for eidetic behaviour, see Eibl-Eibesfeldt 1975; for social behaviour, see Clutton-Brock 1974; for mating theory, see Emlen and Oring 1977).

Another important area of development in sociobiology is the modelling of evolutionarily stable strategies (ESS)(Maynard Smith and Price 1973; Hamilton 1967). Briefly, game theory as developed in Economics (Von Neumann and Morganstern 1953) has been used in biology to model evolutionary processes at the phenotypic level, when the success of each actor depends on what other actors are doing and when the Darwinian fitness of particular phenotypes depends on their frequencies in the population. An ESS is a long-term, stable and optimal solution to such a game. In the biological application, the criteria of rationality and self-interest are replaced by those of population dynamics, stability and Darwinian fitness. In sociobiological applications so far, ESS has been modelled for contests between animals (Maynard-Smith 1982), sexual allocation (Charnov 1982), inter-specific competition for resources (Lawlor and Maynard-Smith 1976), animal dispersal (Hamilton and May 1977) and plant growth and reproduction (Mirmirani and Oster 1978). In a paper of particular interest to social scientists, Axelrod and Hamilton (1981) use game theory to explain the evolution of cooperation. Likewise, it is also instructive to view reciprocal altruism (Trivers 1971) as an ESS for cooperation.

The more sophisticated models in game theory take account of the two main objections to using ESS in human situations, viz. the absence of perfect information about the game, and the mechanism for transfer of information about the ESS. Whilst there are few examples of game theory which include competitions against relatives or multi-facetted competitions between many actors through time, there is no <u>a priori</u> reason why such complex models cannot be developed.

A third important area of sociobiological research concerns theories of learning and cultural transmission. Since strategies of learning are at once biological and cultural, recent research has focussed on co-evolutionary processes of transmission (Lumsden and Wilson 1981; Sforza and Feldman 1981; for discussion of this and other work on learning, see Chapman nd a). One of the links between ESS theory and thoeries of learning is the search for a suitable mechanism of cultural heredity which would permit the reintroduction of evolutionary game theory into the social sciences.

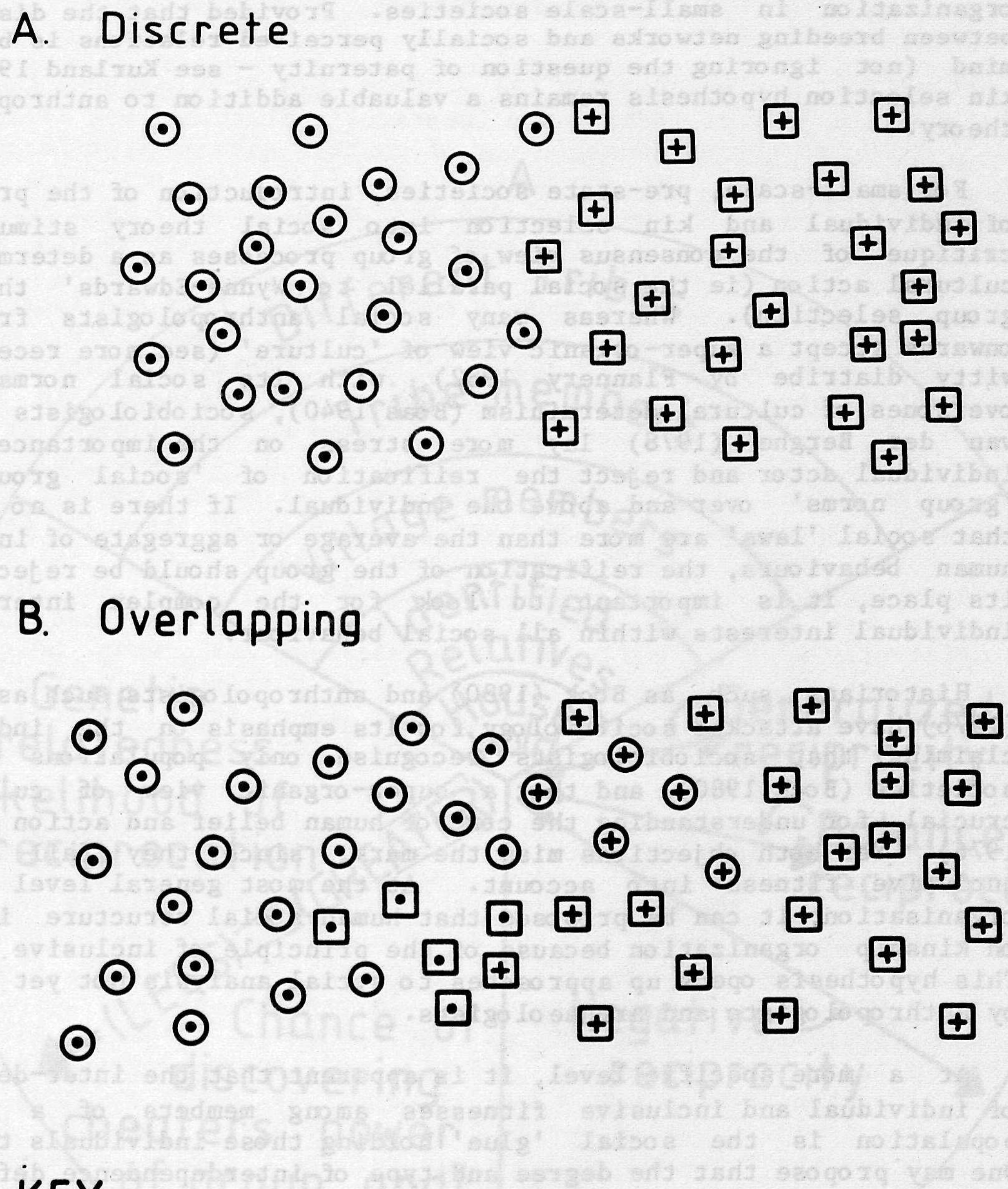
The crucial and disputed advance in sociobiology concerns the application of its kin selection and other hypotheses to human behaviour. For the last decade, human sociobiology has grappled with the convoluted inter-relations between nature and culture, developing ever more complex models of human action. At this juncture, I shall not attempt more than a summary of these research strategies concerned with kin selection and social behaviour.

GENERAL SOCIOBIOLOGY AND HUMAN SOCIOBIOLOGY

In Wilson's (1975) dictum, the crux of sociobiology is the relationship between populations (as controlled by gene flow) and societies (as controlled by information flow) (Fig 1). In the human as in the animal context, these relationships take a spatial and chronological form; in the human context, there is an additional complicating factor of cultural groupings which are not necessarily co-terminous with either 'populations' or 'societies'. An important primary problem in studies of human behaviour is the definition of the degree of overlap between cultures, societies and populations as defined above.

Application of any theory of general sociobiology to the human biogram inevitably means adaptation of the general principle to different historical and cultural circumstances. Before such adaptations can be discussed, it is important to recognise and meet an

Figure 1: Two patterns of population-society interaction



anthropological objection to sociobiological theories of kin selection. The anthropological record is full of examples of the non-coincidence of biological and social parentage. Both Sahlins (1976) and Leach (1981) have argued that, since human relations are determined socially rather than biologically, the theory of kin selection is inapplicable. This objection has been met by Robin Fox (1979) who maintains that breeding classifications are the key to social and biological organization in small-scale societies. Provided that the distinction between breeding networks and socially perceived relations is borne in mind (not ignoring the question of paternity - see Kurland 1979), the kin selection hypothesis remains a valuable addition to anthropological theory.

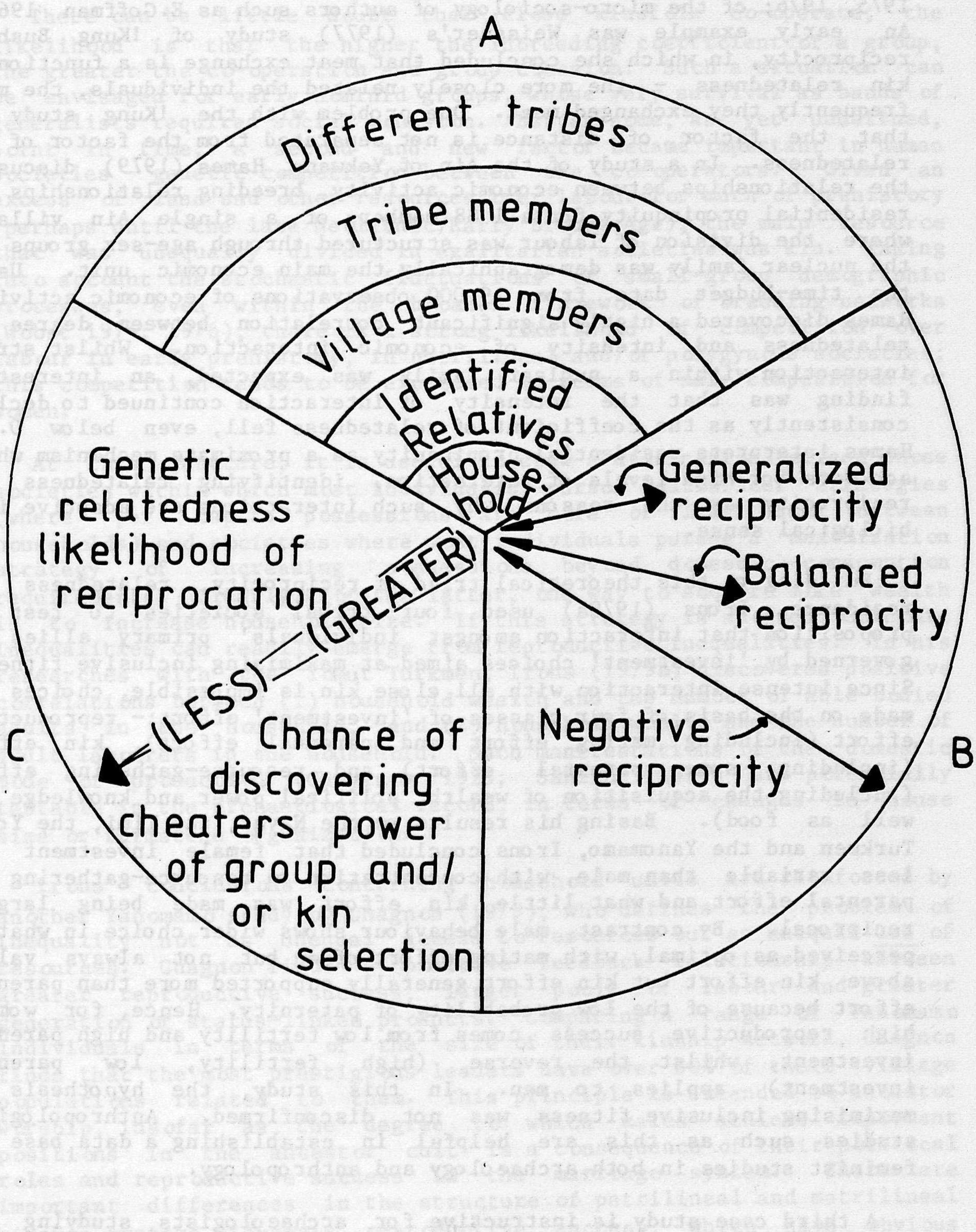
For small-scale, pre-state societies, introduction of the principles of individual and kin selection into social theory stimulates a critique of the consensus view of group processes as a determinant of cultural action (ie the social parallel to Wynne-Edwards' theory of group selection). Whereas many social anthropologists from Tylor onwards accept a super-organic view of 'culture' (see more recently the witty diatribe by Flannery 1982), with its social norms and its overtones of cultural determinism (Boas 1940), sociobiologists such as van der Berghe (1978) lay more stress on the importance of each individual actor and reject the reification of 'social groups' and 'group norms' over and above the individual. If there is no evidence that social 'laws' are more than the average or aggregate of individual human behaviours, the reification of the group should be rejected. In its place, it is important to look for the complex interplay of individual interests within all social behaviour.

Historians such as Bock (1980) and anthropologists such as Sahlins (1976) have attacked sociobiology for its emphasis on the individual, claiming that sociobiologists recognise only populations and not societies (Bock 1980), and that a super-organic view of culture is crucial for understanding the core of human belief and action (Sahlins 1976). Yet both objections miss the mark, since they fail to take inclusive fitness into account. At the most general level of human organisation, it can be proposed that human social structure is based on kinship organization because of the principle of inclusive fitness. This hypothesis opens up approaches to social analysis not yet explored by anthropologists and archaeologists.

At a more specific level, it is apparent that the inter-dependence of individual and inclusive fitnesses among members of a breeding population is the social 'glue' holding those individuals together. One may propose that the degree and type of interdependence defines the characteristics of the 'social structure' of any given group. And because of the interdependence of individual and inclusive fitnesses, it follows that cultural action must be a compromise between individuals and groups. The effects of this compromise may well be very similar to the results of a 'superorganic' view of cultural action.

In summary, the spatial and social framework for kin selection in small-scale societies can be derived from a biological extension of Sahlin's (1965) well-known typology of reciprocity (Fig 2). In the case of failed reciprocity, the other widespread biological behavioural strategy is coercion (viz. reciprocity for the few at the expense of many). Aspects of sociobiological theories of aggression are currently under analysis (see Chapman, n.d. (b)).

Figure 2 - Correlation of (a) social relationships, (b) patterns of reciprocity, and (c) factors selecting for altruism in primitive human cultures. The first two phenomena were identified by Sahlins (1965); they accord with sociobiologic predictions based on genetic relatedness as well as the conditions required for the evolution of a reciprocal altruism. (After Friedman 1979).



THE RELATIONSHIP BETWEEN INDIVIDUALS, RELATIONS AND GROUPS: SOME EXAMPLES

The basis of social life is the interplay between social belief, whether individually or collectively held, and social action, whether undertaken individually or collectively. Whilst the individual and group basis of ritual beliefs is discussed elsewhere (Chapman n.d. (c)), the analysis of social action has been attempted in several small-scale societies.

Relatively few anthropological studies have been designed to answer the question 'how do individuals interact?' (Blurton-Jones 1972; Draper 1975, 1976; cf the micro-sociology of authors such as E.Goffman 1966). An early example was Weissner's (1977) study of !Kung Bushmen reciprocity, in which she concluded that meat exchange is a function of kin relatedness - the more closely related the individuals, the more frequently they exchanged meat. One problem with the !Kung study is that the factor of distance is not separated from the factor of kin relatedness. In a study of the Ain of Yekwana, Hames (1979) discussed the relationships between economic activity, breeding relationships and residential propinquity for all 88 members of a single Ain village, where the division of labour was structured through age-sex groups and the nuclear family was demographically the main economic unit. Using the time-budget data from 29,000 observations of economic activity, Hames discovered a highly significant correlation between degree of relatedness and intensity of economic interaction. Whilst strong interaction within a nuclear family was expected, an interesting finding was that the intensity of interaction continued to decline consistently as the coefficient of relatedness fell, even below 0.25. Hames interprets residential propinquity as a proximate mechanism which accounts for high levels of interaction, identifying relatedness and reciprocity as the reasons why such interactions are adaptive in a biological sense.

Building on this theoretical triad of reciprocity, relatedness and residence, Irons (1979a) used four tribal societies to test the proposition that interaction amongst individuals' primary allies is governed by 'investment' choices aimed at maximising inclusive fitness. Since intense interaction with all close kin is impossible, choices are made on the basis of four classes of 'investment' effort:- reproductive effort (including mating effort and parental effort), kin effort (including some parental effort) and resource-gathering effort (including the acquisition of wealth, political power and knowledge as well as food). Basing his results on the Nayar, the Timi, the Yomut Turkmen and the Yanomamo, Irons concluded that female investment was less variable than male, with concentration on resource-gathering and parental effort and what little kin effort was made being largely reciprocal. By contrast, male behaviour shows wider choice in what is perceived as optimal, with mating effort often but not always valued above kin effort but kin effort generally supported more than parental effort because of the low probability of paternity. Hence, for women, high reproductive success comes from low fertility and high parental investment, whilst the reverse (high fertility, low parental investment) applies to men. In this study, the hypothesis of maximising inclusive fitness was not disconfirmed. Anthropological studies such as this are helpful in establishing a data base for feminist studies in both archaeology and anthropology.

A third case study is instructive for archaeologists studying the spread of human communities into new areas. Chagnon has directed a long-term research programme into the Yanomamo Indians of South America, a tribe of warlike shifting agriculturalists whose responses to attacks or village population increase is often village fissioning. An analysis of coefficients of relatedness (r) measured for parental villages before fissioning and daughter villages after fissioning revealed a clear patterning of higher values in the new settlements (Chagnon 1980). This was interpreted as a tendency for close relatives to migrate together. Once again, the proximate mechanism of residential propinquity is not discussed; it is possible that households moved as single units in the village migration.

There can be little doubt that close kinsfolk co-operate; the likelihood is that the higher the inbreeding coefficient of a group, the greater the co-operation and group cohesion. Such a situation can be envisaged for early hominid groups, whose very survival as bands of generalists required social cohesion. But at some, as yet undefined, point in time, a second and new factor became important in human societies - genetic competition between the co-operators. Given an excess of land and other resources over labour for much of prehistory (perhaps until the late Neolithic/Early Bronze Age), the main resource that was unequally divided in egalitarian societies was kin. Taking into account the stochastic fluctuations of small-group demographic processes, even within the broader framework of breeding networks (Wobst 1974; 1976), there is a strong likelihood of competition over labour in early prehistory, in patrilineal and/or polygynous societies; this competition tends to be expressed in terms of male competition for women.

At this juncture, it is useful to draw a distinction between those societies within which most individuals pursue satisficer strategies (where per capita possessions are more or less equal between households) and societies where most individuals pursue a maximization strategy of increasing possessions beyond domestic consumption requirements. Clearly, for the latter, one way to acquire more wealth is to increase household size. If this strategy is adopted, economic inequalities can readily emerge from reproductive inequalities. In his researches with the Yomut Turkmen, Irons (1979b) discovered positive correlations between (1) household wealth and the number of able-bodied adults in the household, and (2) household wealth and the number of adult labourers in the household. Such manifestations of the domestic mode of production (Chayanov 1923; Sahlins 1974) are potentially testable in the archaeological record, in terms of changes in house size or farmstead organization.

Irons' conclusions concerning household units are reinforced by another Yanomamo study by Chagnon (1979), who defines the problem of inequality not as unequal access to resources but as unequal <u>use</u> of resources. Chagnon finds a positive feedback relationship between greater reproductive success, larger pools of labour and greater generation of status-linked products. Comparing a range of Yanomamo individuals in terms of the size of their kinship network, Chagnon finds that the most prestigious leaders have over 60% of their village populations related to them. This principle is extended to ancestor worship, insofar as the degree to which males achieve important positions in the ancestor cult is a consequence of their political roles and reproductive success in the marriage system. There are important differences in the structure of patrilineal and matrilineal kinship systems vis-a-vis reproductive success, which have obvious archaeological significance. In summary, whilst there is less variety in the size of matrilineages, the political advantages of patriliny are predicted on the greater potential for expansion, in turn underpinned by the sexual asymmetry of reproductive success. It is also interesting to note that, as material items become more important for status definition, there is a decline in the significance and variability of reproductive success, matched by a comparable decline in the frequency of polygyny (Chagnon 1979).

A final case study emphasises an important question that is central to sociobiological theory - the level at which selection operates in human populations. Using the Yomut Turkmen as his study population, Irons (1980) designed a field test of the Wynne-Edwards group selection hypothesis versus the hypothesis of individual selection versus a hypothesis of non-adaptive behaviour. For Irons, one form of behaviour is more adaptive than another if it consistently leads to higher genetic representation in future generations for individuals exhibiting that behaviour in that particular environment. Irons proposes the following predictions of the competing hypotheses:
(1) <u>The Wynne-Edwards hypothesis</u> when per capita resources increase, the expectation is a lower mean age of marriage and higher birth rates. This pattern was not found.
(2) <u>The individual selection hypothesis</u> when per capita resources increase, net reproduction will increase as a result of competition over key resources. This pattern was indeed found.
(3) <u>The non-adaptive behaviour hypothesis</u> no relationship between increases in per capita resources and reproductive levels. This pattern was not found.
In the Yomut case, the scarce resource was reproductive women, and the commonest strategy to gain women was the formation of coalitions of young men converting resources into brideprice for mates.

In the Yomut study, then, Irons (1980) has demonstrated that natural selection at the individual level explained the relevant data in the most effective way. Irons argues that in so far as cultural systems of meaning are consistent with behaviour which increases the inclusive fitness of individuals, such systems of meaning can be integrated with hypotheses based on kin selection.

In summary, the evidence presented in the above case studies demonstrates the relevance of sociobiological principles of kin selection and inclusive fitness to studies of the social behaviour of small-scale societies. Given appropriate modifications, the principles of general sociobiology can be transformed into important principles of human sociobiology. Two questions remain:- what are the limits, if any, of the relevance of the kin selection hypothesis, and how is this theoretical framework related to archaeological theory-building?

THE LIMITATIONS OF THE APPLICATION OF SOCIOBIOLOGICAL THEORY

It is a truism of socio-cultural evolution that the larger and more differentiated the society in question, the more circumscribed the significance of kinship in the overall social structure. In his discussion of social evolution, Flannery (1972) underlines the importance of general-purpose as well as special-purpose institutions in state-level societies for the integration of social action (cf Johnson 1978). More recently, the causes of the decline in the importance of kinship in complex societies have been identified, in a biological context, by Dunnell (1980) as the fast rate of culture change, the importance of functionally-interdependent units over equal and analogous units and the specialization consequent upon increase in information load until excessive for any single individual. In a more general context, Washburn (1978) argues that the more people there are, the greater their mobility, the greater the variety of selective pressures and the shorter the time-span, the less useful will be the principle of inclusive fitness.

For all these reasons, then, it appears likely that kin selection is of little explanatory value in complex, state-level societies. Yet if the individuals concerned in state societies are considered in terms of their roles in politics and production, a different viewpoint emerges. If one follows the epigenetic model of Friedman and Rowlands (1977) as an example of the development of stratified societies, the inclusive fitness of the leaders plays an important role, at each successive phase of social development. At local lineage and conical clan level, kinship principles are generally accepted as important and kin selection clearly applies. As the conical clan develops into an 'Asiatic state', all the noble lineages are said to owe their position to their kinship relationships to a single royal line. At a lower hierarchical level, the court nobles are sent out to local centres to begin new domains, linked to the centre by marriage alliance. Whilst kinship links between royalty and commoners are severed, this does not preclude the continuation of kinship ties, linked to kin selection principles, in different social strata.

Similar kinship principles occur in the prestige goods economy (the importance of royal lineage for the definition of royal status, the kinship links between ruling mother at state centre and aristocratic son in peripheral chiefdom) and even in territorial and city states with their more commercial economies (the rivalry between different conical clans, the development of family entrepreneurial activities, etc). The assumption over dwindling importance of kinship depends on viewing states in their complex, inter-dependent entirety. If relationships between different individuals in each stratum of society are analysed separately, kinship principles and kin selection re-emerge as important and neglected principles of sociobiological organization.

SOCIOBIOLOGY AND ARCHAEOLOGICAL THEORY-BUILDING

There is a central contradiction about the political perceptions of human sociobiology and the potential of its theory for social analysis. Sociobiology has been pressed as a scientific support for laissez-faire, traditional attitudes to individuals whose future choices are strongly constrained by the genetically-determined human biogram. In point of fact, although sociobiologists such as Lumsden and Wilson (1981) are vitally concerned with genetically-coded frameworks for cultural transmission, major research efforts are being devoted to the biological and cultural frameworks of choice for individual actors. The radical individualism of this research confronts reified social formations with the realities of conflict and compromise within human groups. The stress laid on competition and conflict between co-operators provides a theoretical alternative to the search for Marxist principles of contradictions between mode and relations of production in pre-capitalist societies (Spriggs 1984). In this sense, the emphasis on individual social action of the recent 'post-processual archaeology' of Ian Hodder is close to sociological theory, whilst lacking any framework for integrating biological and cultural insights (Hodder 1986).

Sociobiological theory is ripe for integration with the recent developments in bio-archaeology and palaeo-pathology (Buikstra and Cook 1981; Wing and Brown 1979). Palaeo-pathological studies of group demography with their emphasis on quantified statements about the public health of biological populations (eg Cohen and Armelagos 1984) are beginning to provide data with which sociobiological hypotheses can be tested. An example is the data collected for a cross-cultural test of human health, diet, stature and demography at the transition from foraging to farming (Cohen 1986). The overall trend in the data is that the early farmers combined better health with a lower rate of population growth than late foraging groups, a juxtaposition of trends that stimulate interpretation from a kin selection viewpoint.

A second important development in physical anthropological studies is the determination of consanguinal relations based on inherited dental and other abnormalities (for the Natufian of the Levant, Smith 1973; for the Bronze Age of C.Europe, Ullrich 1972). Such innovatory research provides new insights into the social relationships of prehistoric skeletal populations.

The potential for sociobiological contributions to the demography of small-scale societies is therefore linked to developments in the general field of bio-archaeology. The utility of kin selection theory in the historic period, in the context of state formation and consolidation, has been largely ignored until now because of what appear to be spurious arguments of the complexity of the total society rendering kinship principles less relevant. As argued above, kinship principles continue to be important within complex societies but generally within mutually exclusive social strata. The challenge to explore inclusive fitness hypotheses with the help of the written records of literate states has yet to be met.

In conclusion, few archaeological paradigms since the birth of 'New Archaeology' have attempted to integrate a genetic perspective into theories of social change. Whilst critics may object that no-one has yet demonstrated the feasibility of testing sociobiological hypotheses using archaeological data, the point remains that sociobiological theory is too important to exclude from middle-range theory building. This paper presents not a commentary on past inter-disciplinary interactions but rather a challenge to current and future researchers - to develop testable sociobiological hypotheses suitable for bio-archaeological data. The motivation for meeting this challenge is straightforward: the impossibility of developing mature human sciences without a biological perspective on cultural development.

NOTES

(1) Other reasons included the lack of attention paid to evolutionary theory by those interested in post-Pleistocene research, and the importance of 'functional' explanations in then current archaeology.

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evolutionary to archaeological theory.

BIBLIOGRAPHY

Allen,E. et al, 1975 - "Against sociobiology" New York Review of Books, Nov.13th, 1975, pp.182, 184-6.

Axelrod,R. and Hamilton,W.D., 1981 - "The evolution of co-operation" Science Vol.21, pp.1390-1396.

Barlow,G.W. and Silverberg,J., (Eds), 1980 - Sociobiology: beyond nature. Westview Press, Washington, D.C.

Bintliff,J., 1984 - "Introduction", pp.13-39, in Bintliff,J. (Ed) European Social Evolution: Archaeological Perspectives. University of Bradford, Bradford.

Blurton Jones,N., 1972 - Ethological Studies of Child Behaviour. C.U.P., Cambridge.

Boas,F., 1940 - Race, Language and Culture. Knopf, New York.

Bock,K., 1980 - Human Nature and History. A Response to Sociobiology. Columbia University Press, New York.

Buikstra,J. and Cook,D.C., 1981 - "Palaeopathology: an American Account" Annual Review of Anthropology Vol.9, pp.933-970.

Carr-Saunders,A.M., 1922 - The Population Problem, a study in evolution. Clarendon Press, Oxford.

Cavalli-Sforza,L.L. and Feldman,M.W., 1981 - Cultural Transmission and Evolution: a quantitative approach. Princeton University Press, Princeton, NJ.

Chagnon,N.A., 1979 - "Is reproductive success equal in egalitarian societies?", pp.374-401, in Chagnon and Irons (Eds).

Chagnon,N.A., 1980 - "Kin-selection theory, kinship, marriage and fitness among the Yanomamo Indians", pp.545-571, in Barlow and Silverberg (Eds).

Chagnon,N.A. and Irons,W. (Eds), 1979 - Evolutionary biology and human social behaviour. An anthropological perspective. Duxbury Press, N. Scituate, Mass.

Chapman,J.C., n.d.(a) - "The spread of innovation and information: a sociobiological viewpoint", (in prep.).

Chapman,J.C., n.d.(b) - "Warfare and aggression in European prehistory", (in prep.).

Chapman,J.C., n.d.(c) - "Anthropological and sociobiological approaches to ritual". paper presented to 4th TAG conference, Durham, 1982.

Charnov,E.L., 1982 - The Theory of Sexual Allocation. Princeton University Press, Princeton.

Chayanov,A., 1923 - Die Lehre von der Bauerlichen Wirtschift. Patey, Berlin.

Clutton-Brock,T., 1974 - "Primate social organisation and ecology" Nature, Vol.250, pp.539-542.

Cohen,M.N., 1986 - "The Neolithic Revolution and public health". Seminar presented at Univ. of Newcastle upon Tyne, 1986.

Cohen,M.N. and Armelagos,G.J. (Eds), 1984 - Palaeopathology and the Origins of Agriculture. Academic Press, New York.

Colman,A. (Ed), 1982 - Co-operation and Competition in Humans and Animals. Van Nostrand Reinhold, Wokingham.

Doran,J.E. and Hodson,F.R., 1975 - Mathematics and Computers in Archaeology. Edinburgh University Press, Edinburgh.

Draper,P., 1975 - "The cultural ecology of Bushmen children", in Lee,R. (Ed) Studies of Bushmen hunter-gatherers. Harvard University Press, Cambridge, Mass.

Draper,P., 1976 - "Social and economic constraints on child life", in Lee,R. and DeVore,I. (Eds) Kalahari hunter gatherers. Harvard University Press, Cambridge, Mass.

Dunnell,R.C., 1980 - "Evolutionary theory and archaeology", pp.35-99, in Advances in Archaeological Method and Theory, Vol.3, Academic Press, New York.

Eibl-Eibesfeldt,I., 1975 - Ethology, the Biology of Behaviour. Holt, Rinehardt and Winston, New York.

Emlen,S.T. and Oring,L.W., 1977 - "Ecology, sexual selection and the evolution of mating systems" Science, Vol.197, pp.215-223.

Fisher,R.A., 1930 - The Genetical Theory of Natural Selection. Dover, New York.

Flannery,K.V., 1972 - "The cultural evolution of civilizations" Annual Review of Ecology and Systematics, Vol.3, pp.399-425.

Flannery,K.V., 1982 - "The Golden Marshalltown: A parable for the archaeology of the 1980's" Am. Anthrop., Vol.84, pp.265-278.

Fox,R., 1979 - "Kinship categories as natural categories", pp.132-144, in Chagnon and Irons (Eds).

Freedman,D.G., 1979 - Human Sociobiology: a holistic approach. The Free Press, New York.

Friedman,J. and Rowlands,M.J., 1977 - "Notes towards an epigenetic model of the evolution of 'civilization'", pp.201-276, in Friedman,J. and Rowlands,M.R. (Eds) The Evolution of Social Systems. Duckworth, London.

Goffman,E., 1966 - Behaviour in public places: notes on the social organization of gatherings. Free Press, New York.

Gregory,M.S., Silvers,A. and Sutch,D. (Eds) 1979 - Sociobiology and Human Nature. Jossey-Bass, San Francisco.

Hames,R.B., 1979 - "Relatedness and interaction among the Yekwana: a preliminary analysis" pp.239-249, in Chagnon and Irons (Eds).

Hamilton,W.D., 1964 - "The genetical evolution of social behaviour" Journal of Theoretical Biology, Vol.7, pp.1-16.

Hamilton.R.D., 1967 - "Extraordinary sex-ratios" Science Vol.156, pp.477-488.

Hamilton,W.D. and May,R.M., 1977 - "Dispersal in stable habitats" Nature Vol.269, pp.578-581.

Hodder,I., 1986 - "Post-processualist archaeology". Paper presented at Univ. of Durham, 1986.

Huxley,J., 1923 - Essays of a Biologist. Pelican, London.

Huxley,J., 1942 - Evolution: the modern synthesis. Allen and Unwin, London.

Irons,W., 1979a - "Investment and primary social dyads", pp.181-213, in Chagnon and Irons (Eds).

Irons,W., 1979b - "Cultural and biological success", pp.257-272, in Chagnon and Irons (Eds).

Irons,W., 1980 - "Is Yomut social behaviour adaptive?", pp.417-463, in Barlow, G.W. and Silverberg,J. (Eds).

Johnson,G.A., 1978 - "Information sources and the development of decision-making organisations", pp.87-112, in Redman,C.L. et al (Eds) Social Archaeology: beyond subsistence and dating. Academic Press, New York..

Kurland,J.A., 1979 - "Paternity, mother's brother and human sociality", pp.145-180, in Chagnon and Irons (Eds).

Lawlor,L.R. and Maynard Smith,J., 1976 - "The co-evolution and stability of competing species" American Naturalist Vol.110, pp.79-99.

Leach,E., 1981 - "Biology and social science: wedding or rape" (Review of Lumsden,C.J. and Wilson,E.O. 1981 Genes, Mind and Culture. The Co-Evolutionary Process. Harvard University Press, Cambridge, Mass.), Nature, Vol.291, pp.267-268.

Lumsden,C.J. and Wilson,E.O., 1981 - Genes, Mind and Culture. The Co-Evolutionary Process. Harvard University Press, Cambridge, Mass.

Maynard Smith,J., 1982 - Evolution and the Theory of Games. Cambridge University Press, Cambridge.

Maynard Smith,J. and Price,G.R., 1973 - "The logic of animal conflict" Nature Vol.246, pp.15-18.

Mirmirani,M. and Oster,G., 1978 - "Competition, kin-selection and evolutionary stable strategies" Theoretical Population Biology, Vol.13, pp.304-339.

Montagu,A., 1976 - The Nature of Human Aggression. Oxford Univerity Press, Oxford.

Sahlins,M.D., 1965 - "On the sociology of primitive exchange", pp.139-236 in The Relevance of Models for Social Anthropology. ASA Monographs 1. Tavistock, London.

Sahlins,M.D., 1974 - Stone Age Economics. Aldine, Chicago.

Sahlins,M.D., 1976 - The Use and Abuse of Biology. Univ. of Michigan Press, Ann Arbor.

Slaughter,C., 1984 - "Social Evolution: some sociological aspects", pp.41-67, in Bintliff,J. (Ed) European Social Evolution: Archaeological Perspectives. University of Bradford, Bradford.

Smith,P., - 1973 "Family burials at Hayonim" Paleorient, Vol.1, pp.69-71.

Sociobiology Study Group of Science for the People, 1976 - "Sociobiology - another biological determinism" BioScience, Vol.26(3), pp.182-190.

Spriggs,M. .(Ed), 1984 - Marxist perspectives in Archaeology. C.U.P., Cambridge.

Trivers,R.L., 1971 - "The evolution of reciprocal altruism" Quarterly Review of Biology, Vol.46, pp.35-39, 45-7.

Ullrich,H., 1972 - Das Aunjetitzer Graberfeld von Grossbrembach. Teil 1. Anthropologische Untersuchungen zur Frage nach Entstehung und Verwandschaft der thuringschen, bohmischen und Inarhrischen Aunjetitzer. Bohlaus, Weimar.

van der Berghe,P.L., 1978 - "Bridging the paradigms: biology and the social sciences", pp.33-52, in Gregory,M.S., Silvers,A. and Sutch,D. (Eds).

Von Neuman,J. and Morganstern,O., 1953 - Theory of Games and Economic Behaviour. Princeton University Press, Princeton.

Washburn,S., 1978 - "Animal behaviour and social anthropology", pp.53-74, in Gregory, Silvers and Sutch (Eds).

Weissner,P.W., 1977 - Hxaro: a regional system of reciprocity for reducing risk among the !Kung San PhD, Univ. of Michigan, Ann Arbor.

Williams,G.C., 1966 - Adaptation and Natural Selection. Princeton University Press, Princeton.

Wilson,E.O., 1975 - Sociobiology: the new synthesis. Harvard University Press, Cambridge, Mass.

Wing,E.S. and Brown,A.B., 1979 - Paleonutrition: method and theory in prehistoric foodways. Academic Press, London.

Wobst,H.M., 1974 - "Boundary conditions for paleolithic social systems:

a simulation approach" Amer. Antiq., Vol.39, pp.147-177.

Wobst,H.M., 1976 - "Locational relationships in Paleolithic society" Journal of Human Evolution, Vol.5, pp.49-58.

Wright,S., 1922 - "Coefficients of inbreeding and relationship" Amer. Naturalist, Vol.56, pp.330-339.

Wynne-Edwards,V.C., 1962 - Animal Dispersion in relation to Social Behaviour. Oliver and Boyd, Edinburgh.

PALAEOPATHOLOGY: COTTAGE INDUSTRY OR INTERACTING DISCIPLINE?

C.A.Roberts

INTRODUCTION

The study of human bones from archaeological sites has traditionally been undertaken in the past, and indeed today, by interested parties such as the local doctor, dentist, archaeologist or anthropologist in their own homes. This has led to, and is still leading to, a proliferation of bone reports consisting merely of a catalogue of skeletons with no interpretation. This may be because many of these workers have no archaeological background. This is not the ideal situation for any specialist to work in and indeed, the bone reports produced have often not been the ideal way of presenting the data. Should we not be asking that human bone study be rationalised and only undertaken in organised departments of archaeology/anthropology where there can be co-ordination between the archaeologist and all the other specialist disciplines? The existence of a 'cottage industry' of bone report producers is perhaps why some archaeologists are heard to say that,

"palaeopathological studies, in Britain at least are unco-ordinated and desperately understaffed [therefore] there is little possibility of constructive exchange of views between archaeologist and palaeopathologist" (Cramp 1983, p.19).

HUMAN BONE REPORTS

The human bone report is perhaps the central focus of human bone studies even today. To pose several questions is perhaps appropriate. How many bone reports have been what the archaeologist wanted or indeed expected? Did he/she know what should or could be expected and what potentials there were for studying human bones with reference to the rest of the archaeological site? It is likely that many archaeologists are not aware of the possible information that human skeletal remains can provide, not because they are unwilling to learn, but because the palaeopathologist has not enlightened them. Until specialists, including palaeopathologists, explain their data in a comprehensive fashion they will not be taken seriously.

Rosemary Cramp (1983, p.12) has said that she believes archaeologists are often more concerned with the social (i.e. funerary archaeology) than the pathological side of burials. Is this because bone specialists have not tried to develop and explain this aspect of study in simplified terms for the archaeologist to understand or is it that the archaeologist is dubious about the contribution that patterns of pathological process can make to the rest of the site data? Archaeologists should not wish to see the bone report forever relegated to the end of the main archaeological report and neither should the specialist.

Human remains have probably never been a fashionable preoccupation in archaeology, unlike pottery, flint or metalwork. To emphasise this point, archaeology is about excavating the past lives of people and not just their buildings, animals or pottery.

PALAEOPATHOLOGY

Palaeopathology is the scientific study of disease processes in earlier peoples. It represents an examination of the corporeal changes resulting from the interaction of man with his environment. Much of the pathology seen on skeletons is the product of developmental accident, chance exposure to pathogens or the ravages of the inevitable ageing process. Improved methods of excavation and analysis have encouraged research on specific pathological conditions found in human skeletal remains. Now some pathologists are beginning to understand more fully how they fit into the archaeological scene. Having said that, as archaeologists we should be trying to determine the total picture of past societies and not a fragment of their lives. There are obvious limitations in all archaeological data but this is not the subject of the present paper. There should be a more positive approach to the problem providing that the limitations in the data are recognised.

In the Institute of Field Archaeologist's code of conduct, archaeology is defined as the "study of the nature and past behaviour of man in his environmental setting. [This study] is carried out through the investigation and interpretation of the material remains of man's activities". As the archaeological heritage is a finite, vulnerable and diminishing resource, it is the archaeologist's responsibility to make the most of the material remains available.

In an ideal world every archaeological site would generate all types of evidence from structures to insects, but often, certainly in skeletal study, there may only be a cemetery with no associated settlement. In these cases inferences from nearby sites of the same period could help to make our primary data more interpretable.

In order to explain how palaeopathology can contribute much more than has been believed in the past, use of several examples of specific pathologies found in skeletons will be made.

(i) PRESERVATION

First, something of the preservation of human remains and their excavation should be discussed. In the past there was frequently incomplete recovery of skeletons, often only the skull and long bones were retained. There are different states of preservation from various archaeological sites depending on the soil conditions and excavation techniques. A necessary condition of palaeopathology is the completeness of the skeleton.

The completeness of the skeleton is vital to our understanding of pathological processes. Brothwell (1972, p.86) has said that, "there are still very few British archaeologists.....who are prepared to spend enough time and care to ensure that the skeletal material will provide unbiased information". Excavation techniques and processing have fortunately improved since that time.

There are two points concerning the preservation of skeletons which are of extreme importance in palaeopathology.

a. There is a need to know the sex and age of individuals, not only to produce mortality rates for populations which of course are directly related to pathology, but to interpret specific pathological processes. The completeness of the skeleton determines whether we can age and sex a skeleton accurately. Diseases are, in many cases, age and sex related in incidence. For example, rheumatoid arthritis affects females three times more frequently than males and the average age of onset is about 35-40 years today. If it was believed that a skeleton had suffered from rheumatoid arthritis (a difficult diagnosis to make at the best of times) an accurate age and sex assessment could be a valuable aid to diagnosis.

b. Only specific areas of the skeleton are affected in some diseases. For example, specific changes in the face, hands and feet may be pathognomonic of leprosy (Moller-Christensen 1961 and Figure 1).

In the past the small bones of the hands and feet were missed or ignored on excavation but with the advent of sieving on archaeological sites recovery of these small bones has improved.

Before considering some pathological conditions it should be emphasised that it is not only the pathological condition which is important in palaeoclinical interpretation but also medical history (art and literature), ethnography and modern clinical medicine. They all have their part to play in the interpretation of palaeopathology and should not be ignored.

(ii) PALAEOPATHOLOGICAL EXAMPLES

There are four examples of particular pathological conditions which simply illustrate the value of palaeopathological studies with reference to the other archaeological site data.

a. A study by Merbs published in 1983 concerning the occupationally induced pathologies of a Canadian eskimo population from Southampton Island, Canada must be one of the most pioneering studies of human bones and their relevance to the population's lifestyle. It showed one of the directions which pathologists should be working towards. Merbs of course had an ideal population, well preserved and almost complete. With historical ethnographic accounts, he could be more or less certain of their basic behavioural patterns and that their behaviour was sexually dichotomous (1983, p.4). Admittedly there are not many past populations in Britain with such complete recovery and associated ethnographic accounts.

Merbs stated that he believed some kinds of actions will be performed over and over again especially if the culture defines them as 'correct'/necessary for survival (1983, p.4). Some actions will be intermittent or seasonal. Bone pathology is either due to abnormal stress on an individual over a short period (traumatic) or normal stress for a long time e.g. osteoarthritis. Merbs was looking at trauma and osteoarthritis specifically. Interesting patterns emerged in the patterns of osteoarthritis (a degenerative disease related specifically to age and occupation).

Osteoarthritis of the foot (Figure 2) was seen more in males. He could relate this to the fact that men were hunting on foot over rough terrain and females were not engaged in these activities.

Vertebral compression (due to a force acting through the vertebral column causing collapse of the vertebral bodies) was found to be greater in females (Figure 3) and was related to them riding on toboggans (minus shock absorbers). The shock of riding over rough terrain would have been transmitted directly to the spine. Women also carried babies on their backs (Figure 4) which also contributed to vertebral compression.

Vertebral osteophytosis is a pathological condition related to degeneration of the intervertebral disc. It is recognised on the vertebral bodies as bony outgrowths usually on the anterior and lateral margins. Vertebral osteophytosis occurred in the thoracic region in females and more in the lumbar vertebrae in males (possibly due in the past to lifting heavy objects).

The general picture of osteoarthritis (Figure 5) showed that the shoulder was affected more in males and related to harpoon throwing and kayak paddling. The temporomandibular joint was more affected in females due to their occupation of softening frozen or dried skins with their teeth before making items for everyday use.

Merbs concluded his study by stating that, "the general findings of this study .....suggest that the reconstruction of activity patterns from patterns of pathology has considerable potential" (Merbs 1983, p.184). In circumstances such as these there is no doubt of the value of such a study.

b. If the diet of past populations is considered, there are basic observable features of the bones to indicate the type of deficiencies a person may have been suffering. Without even considering the chemical analysis of bone to reach conclusions about whether a person was a vegetarian or a carnivore (strontium:calcium analysis - Sillen and Kavanagh 1982) there is a vast potential for the contribution of human bone studies to past diets.

Cribra orbitalia and porotic hyperostosis are pathological features easily recognised in the bone of the eye sockets and skull vault. They are indicators of anaemia (probably iron deficiency, in the temperate zones) in childhood (Stuart Macadam 1982). Any palaeopathologist who isolates these conditions should be looking at the archaeological site as a whole. What diet were these people eating? It is accepted that iron mainly occurs in meat. Were there any animal bones recovered from the site? Was the population vegetarian rather than carnivorous? Seeds and pollen from a site can also help to reconstruct diet. Does the diet reconstructed by a seed, pollen or animal bone specialist have any relation to the pathologies which are being observed?

To diverge further, anaemia is not always due to a dietary deficiency in iron. There are many causes. It could be due to a disease of the gastrointestinal tract preventing absorption of iron from the intestine or chronic blood loss therefrom. In some cases there may be parasite eggs preserved on the site to give an indication of the prevalence of human intestinal infection (Jones 1983). In exceptional circumstances there may be documentary records available to reconstruct the picture. For example, in Medieval Winchester much of the manure from the stables and byres was dumped in the side streets (Keene 1982). Apparently in the Medieval period the problem of rubbish was paramount in town governmental activities. This is relevant to what is being considered here, especially palaeopathology.

Perhaps Greig's study of the 15th century barrel-latrine deposit published in 1981 illustrates what there is to learn about the diet of a particular population of a specific period solely from the analysis of material from one barrel (Figure 6).

He could interpret the kind of diet the population may have been eating, their living conditions, health and general surroundings. If there had been any human bones from the site there would have been a little more to say on these aspects. As for the environment, he isolated seeds and mosses with possible origins ranging from wetlands through cultivated woodland to heathland. A possible origin for all the contents of the barrel were suggested (Figure 7).

It is not proposed that this was the correct interpretation, in fact this is only a possible reconstruction, or, that we could reconstruct every site like this; but isn't this what we should be thinking about?

c. Dental disease is, again, a feature that is common in archaeological populations but is rarely interpreted with reference to other archaeological data.

Caries or rotten teeth is a condition which is associated with a diet high in carbohydrates especially sucrose. We know from documentary evidence that sucrose was not introduced into this country until about the 12th century A.D. (Figure 8).

Correspondingly, the caries rate increased after that time (Moore and Corbett 1978). Apart from an indication of the diet which was being eaten, caries can begin to illustrate the standard of oral hygeine which our ancestors had, and even a little about dentistry. Who were these people practising dentistry? Were they like the dentists we know today? Certainly documentary evidence for dentistry in the past is extensive. Here we are beginning to think more about the social aspects of a society.

Calculus (plaque), a calcium deposit which builds up on the tooth surface is a good indicator of an individual who did not possess a toothbrush and probably didn't even know what one was. Again we have evidence of the level of the oral hygeine being practised in a population.

d. Looking at a specific disease in antiquity we see tuberculosis, a chronic infection caused by a known bacterium, as a disease of considerable antiquity which affects the soft tissue of the body, primarily the lungs and gut and secondarily the bone. Today there is effective treatment available at least in the Western world. It is a condition most often seen in the spines of skeletons and as iconographic representations of the signs of spinal collapse.

The earliest evidence of human tuberculosis in Europe comes from the Neolithic. Osteoarchaeological and documentary evidence indicates that, in Britain, tuberculosis remained as an endemic disease from the Roman period, increasing in prevalence and reaching a peak incidence in the post-Medieval period.

Tuberculosis is a mycobacterial disease, and the two bacteria responsible for the infection in humans are M.tuberculosis and M.bovis. In taxonomy, these two may be considered as human and bovine subvariants of a single species, M.tuberculosis. A recent hypothesis proposes that tuberculosis as a human infection developed by transmission from infected cattle (Figure 9), and that this transmission was facilitated by domestication (Manchester 1984).

The infection is mainly transmitted via the respiratory system if the infection is from another human or mainly via the gut if the infection is from the infected cow (via infected meat or milk). It is also possible that cow to human transmission could occur through the respiratory tract dependent upon the close association of uninfected man and infected animal, and a lung focus of infection in the animal. The disease is obviously relevant in terms of the type of economy which a population was pursuing and the conditions in which it was living. Even today a constant preoccupation of the World Health Organisation is the connection between malnutrition, bad housing and disease. From experiments on animals, it is believed that poor nutrition in an individual would have to be extreme for it to affect the immune status of the person to tuberculosis (Bates 1982). However, poor housing, hygeine and overcrowding is linked to the development and spread of tuberculosis.

There is corporeal evidence in Egypt for tuberculosis as early as 1000 B.C. (Zimmerman 1979) although there is earlier (4th millenium B.C.) iconographic evidence (Grmek 1984). At this time, multistoried and close packed houses were built on narrow winding streets in fairly large urban centres. This probably favoured the spread of infections with close personal contact. Consider an example af a Medieval house plan to appreciate how different living conditions, certainly in the Western world, were in the past (Figure 10). This must have had an effect on disease patterns and spread (Figure 11).

CONCLUSION

It has become clear in this short review of some pathological conditions that whilst other classes of evidence can help put flesh on the bones, the bones can give information to supplement the archaeological evidence and are, in essence, the nearest we can get to past peoples. The evidence of past lives of people is well known from depictions in art but whether this is an honest picture is debateable. Hopefully in the future we may move towards this type of presentation and look from distant cottages to castles in our synthesis of archaeological evidence, and not only in palaeopathology. All types of data are equally important. But it is essential that the relegation of bone reports to mere appendages to archaeological reports must stop before the stage is reached where the archaeologist feels that it is not even worth analysing the bones. Greig in his work on the latrine deposit believed that, "The scientific analysis of these remains cannot be done by any one person, for each evidence needs to be dealt with by the appropriate specialist, and the various data brought together and discussed to obtain the maximum information" (Greig 1981, p.281).

There may be widespread agreement for this statement but does it in practice actually happen? Archaeologists are reconstructing the lives of people regardless of whether they are the common folk or from the upper echelons of society. They are both important. Finally what is the aim of this work? Are archaeologists working for each other in the academic interest or are they working ultimately for the public? Finance for archaeology comes from the public so should there perhaps be an effort on everyone's part to make the data intelligible for them more than anybody?

ACKNOWLEDGEMENTS

Thanks to Keith Manchester for making useful comments on the text and to Jean Brown of the Photography Department at Bradford University for the figures. The National Museums of Canada (Jerome Cybulski) and Prof. Charles Merbs gave permission to use Figures 2,3,4 and 5. James Greig allowed reproduction of Figures 6 and 7.

BIBLIOGRAPHY

Bates,J.H., 1982 - "Tuberculosis: susceptibility and resistance" Bull. New York Acad. Med., Vol.55(6), pp.604-8.

Bianca,W., 1976 - "The significance of meteorology in animal production" Int. J. Biometeor, Vol.20(2), pp.139-156.

Brothwell,D., 1972 - "Palaeodemography and earlier British populations" World Archaeology, Vol.4, pp.75-87.

Cramp,R., 1983 - "The archaeologist's view-general" in G. Hart (Ed), Disease in Ancient Man. Clarke Irwin, Toronto.

Greig,J., 1981 - "Investigation of a Medieval barrel latrine from Worcester" J. Arch. Sci., Vol.8, pp.265-82.

Grmek,M., 1984 - Les maladies a l'aube de la civilisation occidentale. Paris.

Jones,A.K.G., 1983 - "A coprolite from 6-8 pavement", pp.225-229, in A.R.Hall, H.Kenward, D.Williams and J.Greig (Eds), Environmental and living conditions at two Anglo-Scandinavian sites. Council for British Archaeology, London.

Keene,D.J., 1982 - "Rubbish in Medieval towns" in A.R.Hall and H.Kenward (Eds) Environmental archaeology in the urban context. C.B.A. Res. Rep., Vol.43, pp.26-30.

Manchester,K., 1984 - "Tuberculosis and leprosy in antiquity: an interpretation" Medical History, Vol.28, pp.162-73.

Merbs,C., 1983 - Patterns of activity in a Canadian Inuit population. Museums of Canada.

Moller-Christensen,V., 1961 - Bone changes in leprosy. Copenhagen, Munksgaard.

Moore,W.J. and Corbett,M.E., 1978 - "Dental caries experience in man" pp.3-19, in N.H.Rowe (Ed), Proc. of Symposium on Diet, Nutrition and Dental Disease. Universy of Michigan School of Dentistry and Research Institute.

Sillen,A. and Kavanagh,M. 1982 - "Strontium and palaeodietary research: a review" Yearbook of Physical Anthropology, Vol.25, pp.67-90.

Stuart Macadam,P., 1982 - A correlative study of a palaeopatholgy of the skull. Cambridge University Department of Anthropology unpublished PhD thesis.

Zimmerman,M.R., 1979 - "Pulmonary asseous tuberculosis in an Egyptian mummy" Bull. New York Acad. Med., Vol.55, pp.604-8.

Figure 1 - Principle areas of the skeleton affected by leprosy

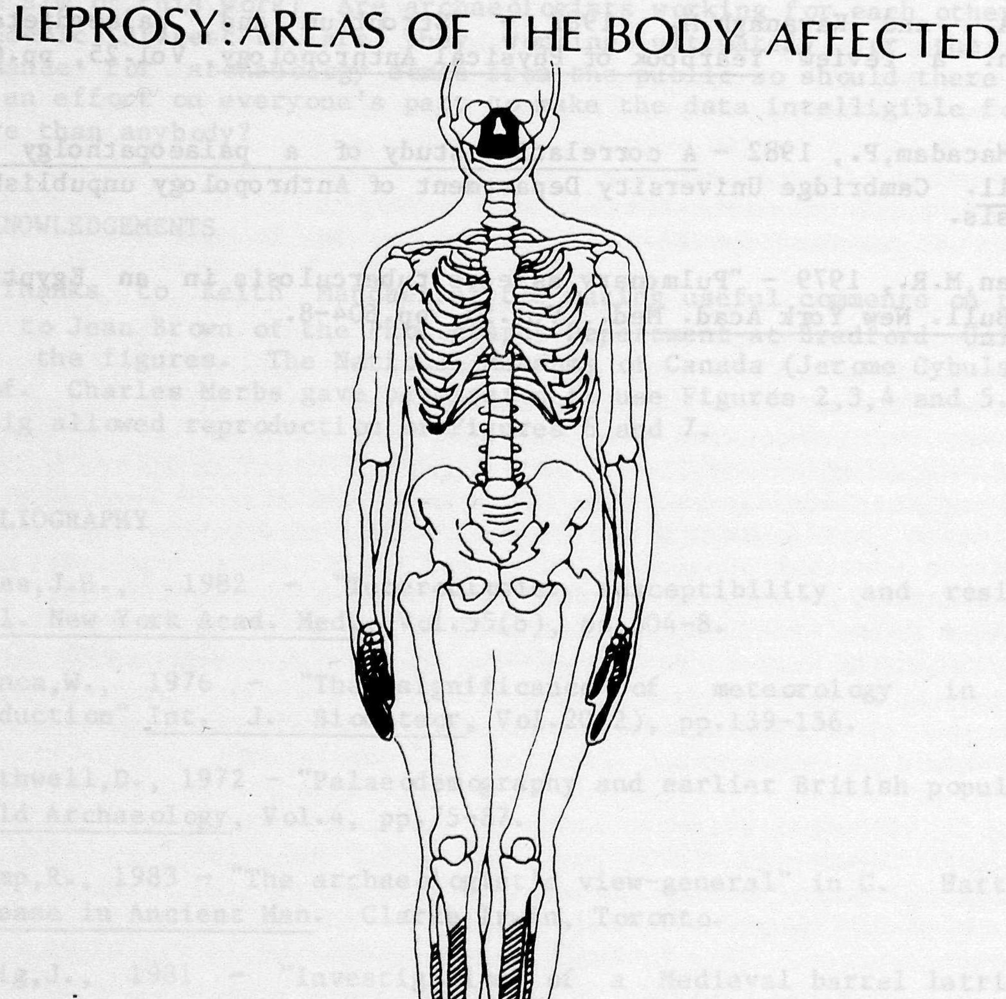
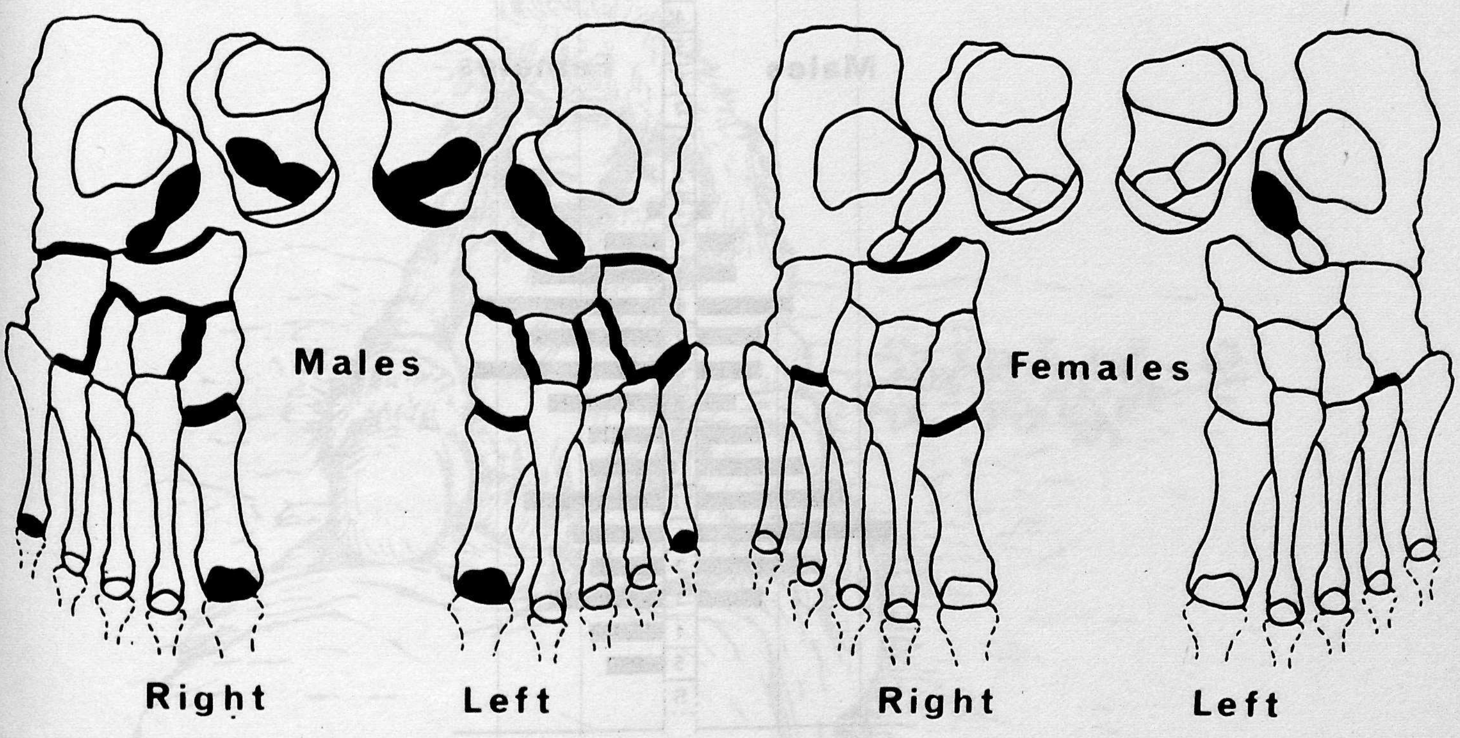
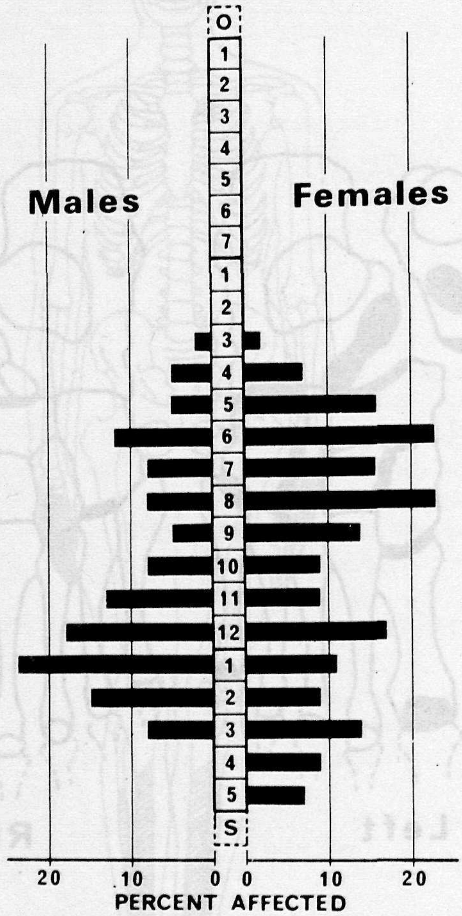


Figure 2 - Osteoarthritis distribution in the foot (after Merbs 1983, fig.43)



Distribution of osteoarthritis in the foot. Darkened areas indicate frequency values of at least 10 percent or intensity values of at least .05.

Figure 3 - Distribution of vertebral compression fractures (after Merbs 1983, fig.50)



Distribution of vertebral compression fractures according to sex.

Figure 4 - Sadlermint woman (after Merbs 1983, fig.76)

Sadlermiut woman and infant riding on sled. (Drawn by Charles Joslin.)

Figure 5 - General picture of the distribution of osteoarthritis (after Merbs 1983, fig.45)

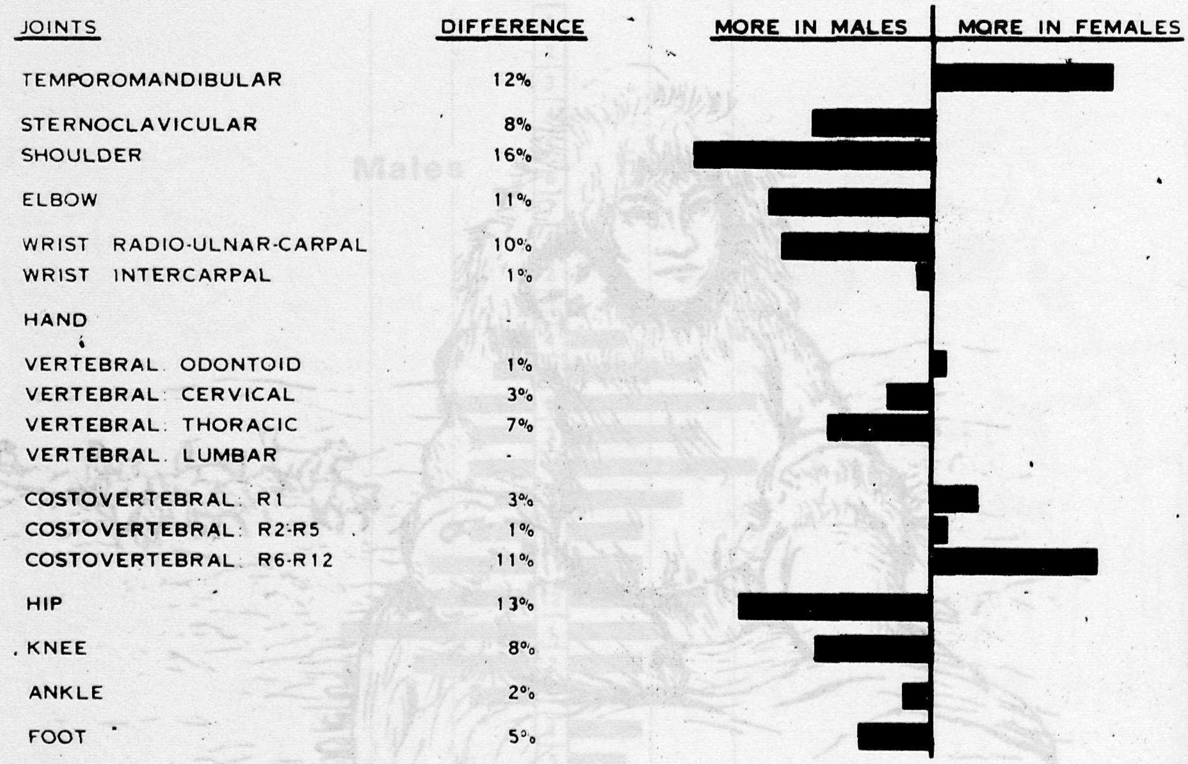
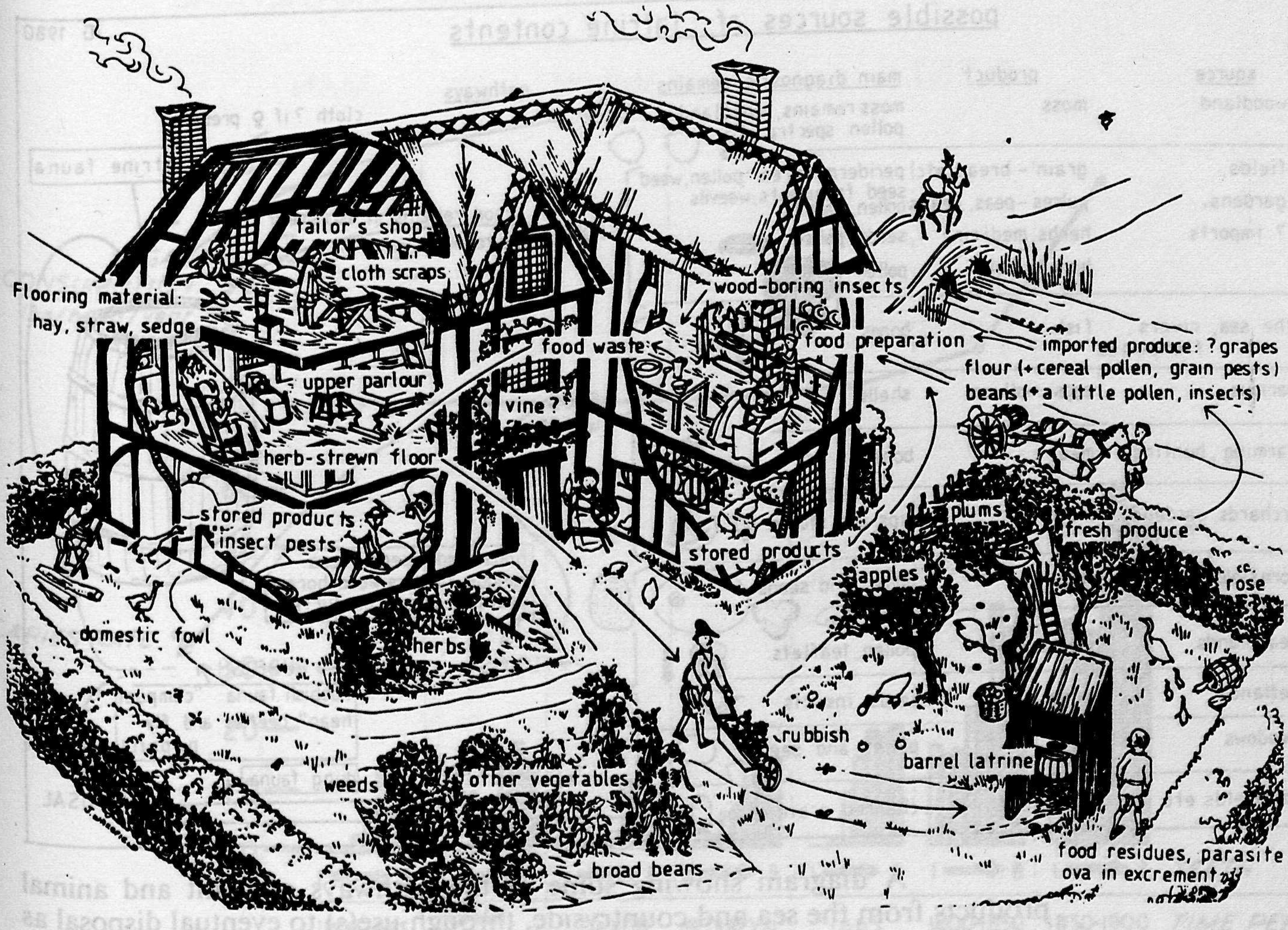
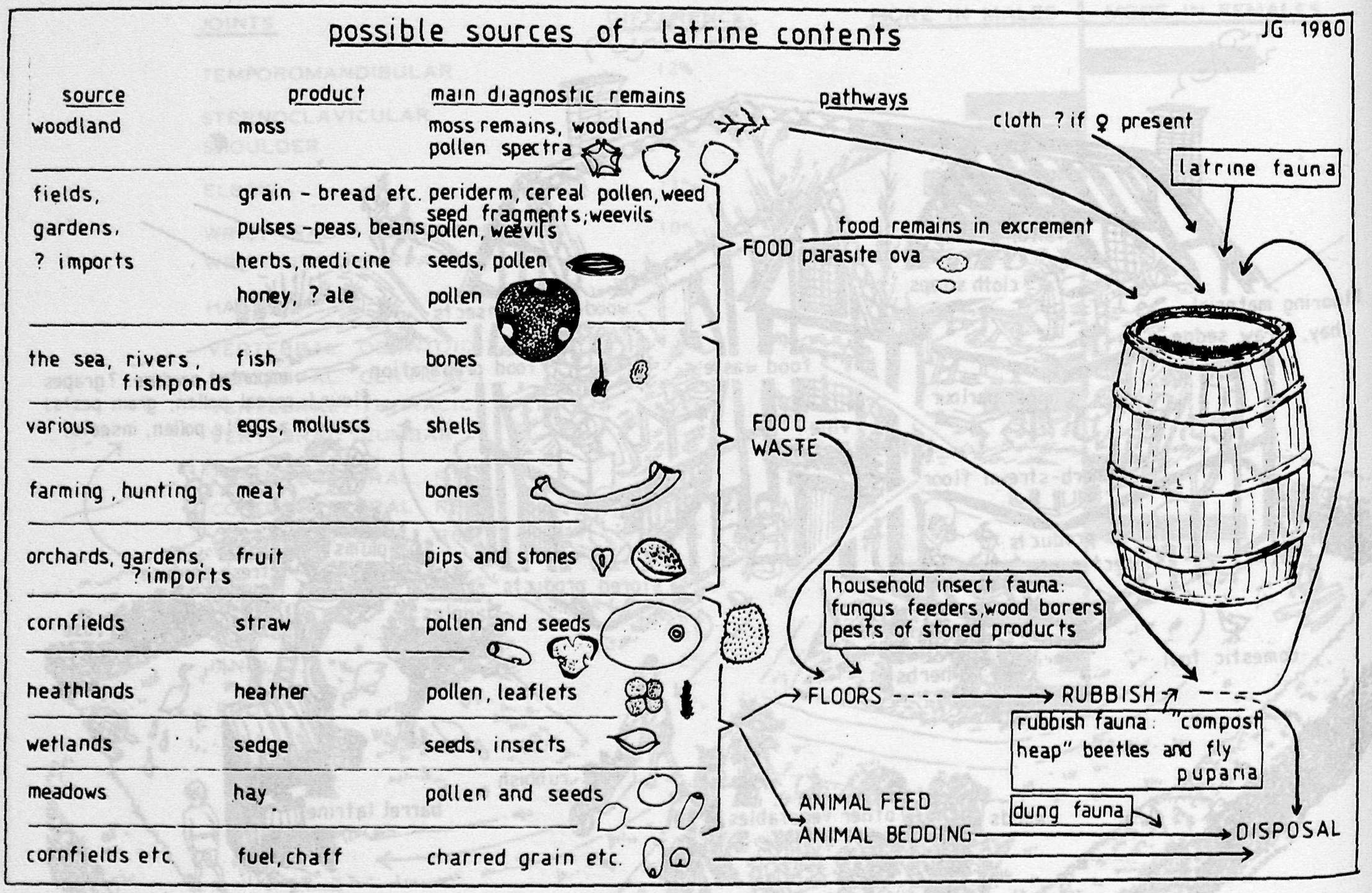


Figure 6 - Possible sources of the finds from a 15th century barrel in Winchester (after Greig 1981)



An illustration showing possible sources of the finds from the barrel (not a reconstruction).

Figure 7 - Pathways of plant and animal products (after Greig 1981)



A diagram showing some of the pathways of plant and animal products from the sea and countryside, through use(s) to eventual disposal as various types of rubbish, accumulating various kinds of insect fauna in the process.

Figure 8 - Caries rate and sugar consumption from the Iron Age to 1900 AD (after Moore and Corbett 1978)

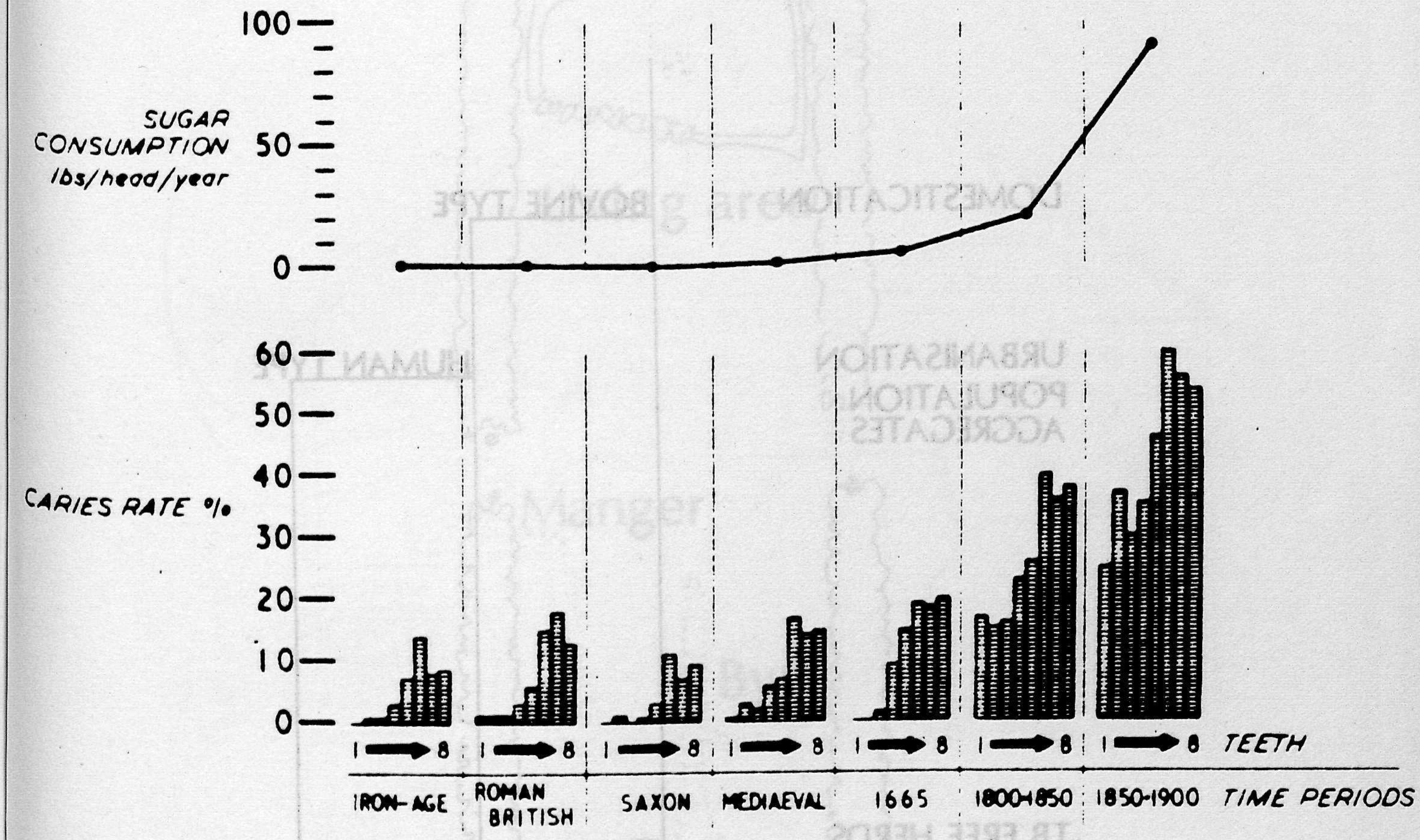


Figure 9 - Evolution of tuberculosis

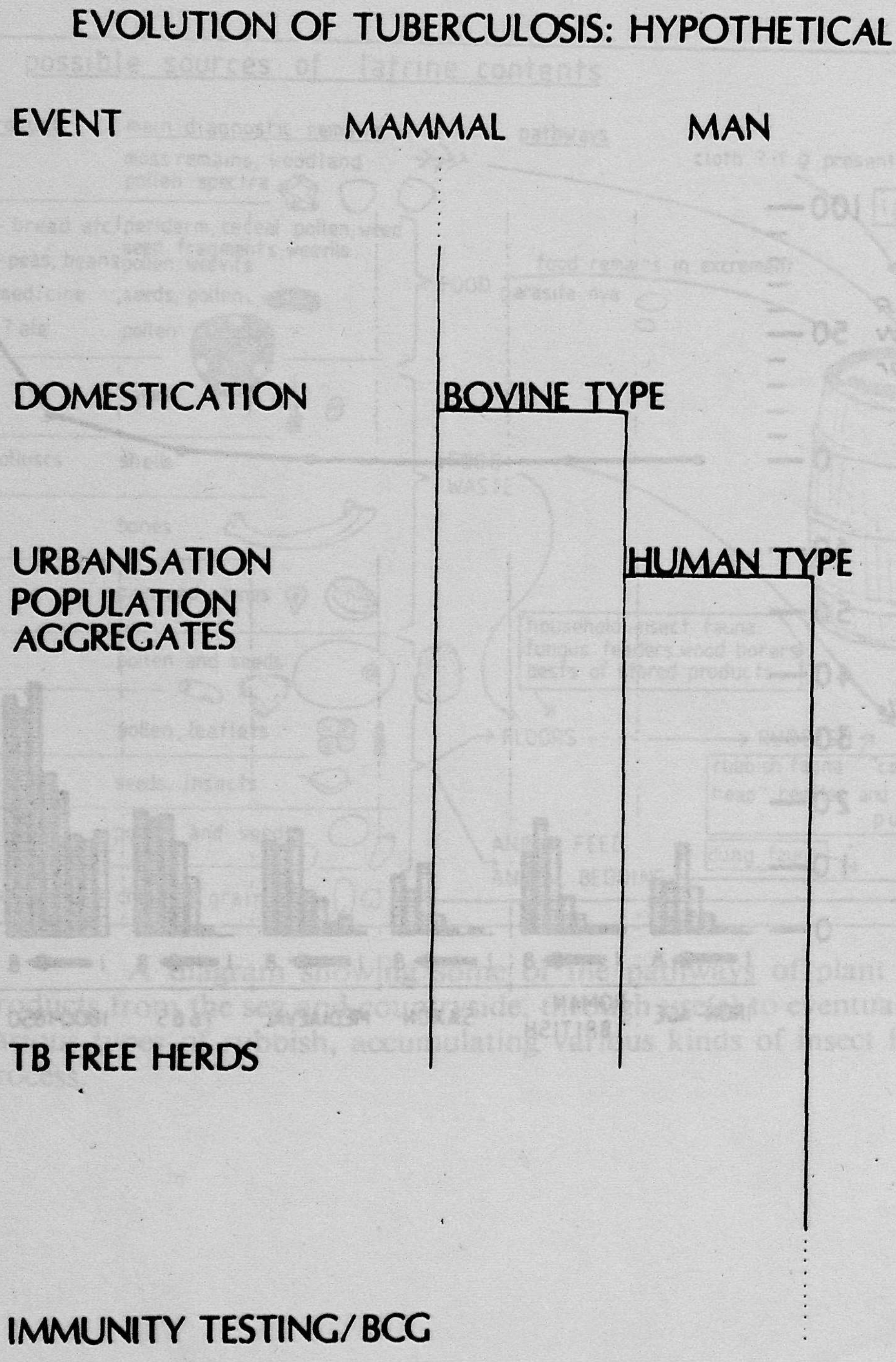


Figure 10 - Medieval house plan: both human and animal living under one roof

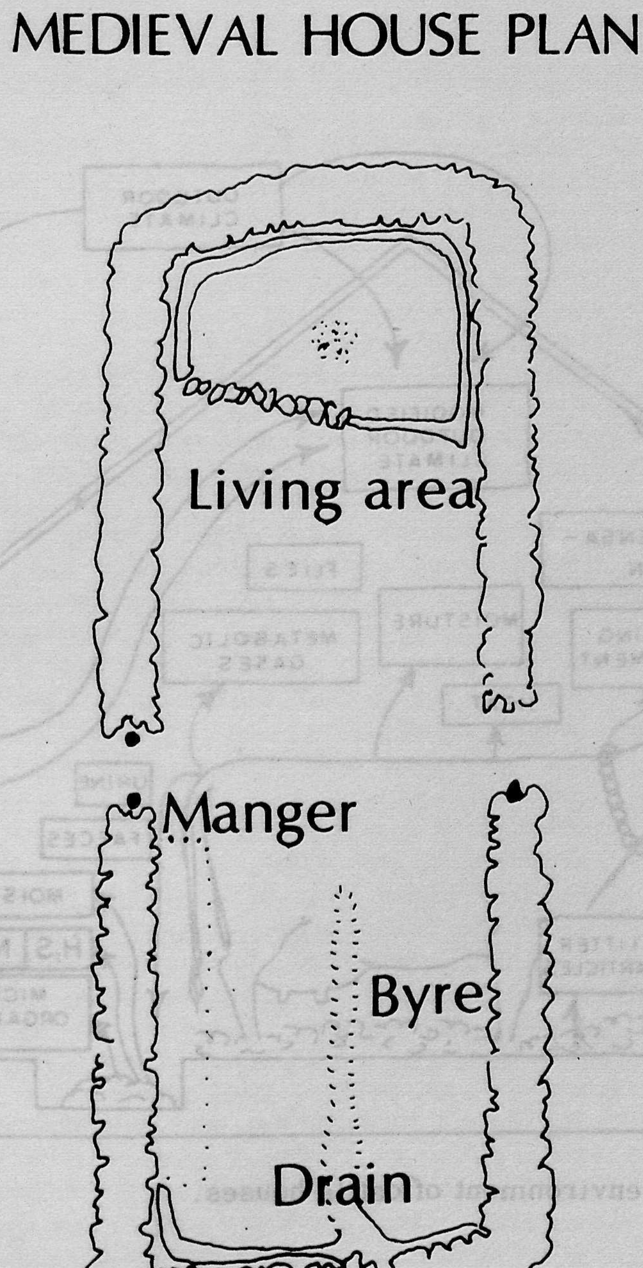
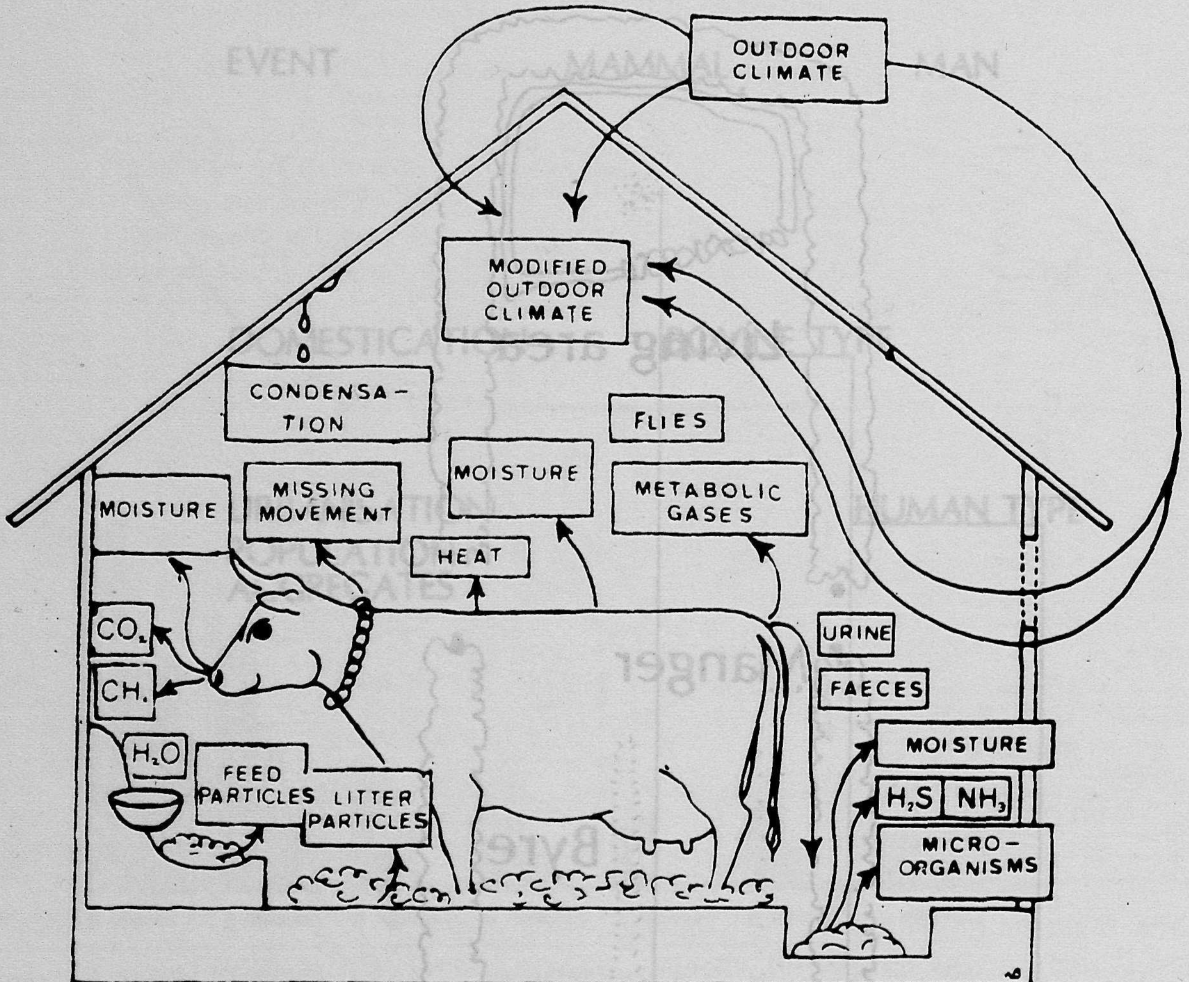


Figure 11 - The possible environmental changes inside a building housing cattle (after Bianca 1976, fig.7)



The indoor environment of cattle houses.

2) Dating methods

3) Analyses of materials

4) Mathematical treatment

5) Studies of the ancient environment

Of all the disciplines I feel that it is in mathematical treatment that the "hard" in hard scence becomes oppressive for the archaeologist. This is not surprising when we examine the diversity of disciplines reliant on the mathematical sciences. Thus an example of three-dimensional analyses arising in pure nuclear physics bears a striking superficial resemblance to the projective geometry used in the rectification of oblique aerial photographs containing archaeological features.

If we examine any one of the main divisions of Archaeometry as defined above we can approach it on a series of levels, and the question confronting the archaeologist is 'at what level?', before, as Aitken suggests, he starts to [illegible]. I suggest that the archaeologist must be prepared to tackle the level at which he is satisfied that, by objective thought on his own part, he knows that the scientific data provided is reliable archaeologically. The scientist (correctly) will tell the archaeologist that the science is good - is the technique is good - but is the result archaeologically valid? The archaeologist [illegible] that the former [illegible] latter's requirements.

[illegible]

can locate coin hoards as effectively as [illegible] understandable. Even so there is again a need for appreciation at a "hard science" level of the interpretation [illegible] limitations. Often complex techniques of data filtering and

HARD SCIENCE: TOO HARD FOR ARCHAEOLOGY?

A.Aspinall

The title chosen for this paper may be appropriate in view of our claim at Bradford that we are providing training in, as well as an awareness of, aspects of hard science in archaeology. Our experience is that these goals are difficult both for students and tutors.

How do we define hard science? As a physicist my immediate reaction is to think of physics and mathematics but also acknowledge the influences of chemistry and other physical sciences. The biological sciences present faces of science in archaeology which, in environmental archaeology, give us the closest integration with the least anguish of "traditional" archaeology and, therefore, does not, perhaps, qualify as "hard" science for the archaeologist. If we limit our attention to the physical sciences we find ourselves describing Archaeometry. It is useful, at this point, to quote from Aitken referring to the growth of Archaeometry: "There has been much crossing of barriers between disciplines, and archaeologists have shown themselves as capable of this as physicists. This book does not attempt to spoon-feed archaeologists by isolating technicalities but presumes on their ability to step back when they find themselves sinking, so that they may move on to drier land. For those that survive there are several journals..." (Aitken 1974, p.v). For those who have not read Aitken's book, let me state that there is an excellent exposition of the science of Archaeometry pitched at about the University entrance level in Physics, and pointing the finger at aspects of hard science which go to far realms of the basic properties of matter. In fact, Aitken's book limits itself to the clearly defined concepts of Physics with the chemical and biological sciences in a subsidiary role. If, however, we embrace them all, we may, somewhat arbitrarily, list the occurrence of scientific techniques in Archaeology as below:

1) Geophysical prospection

2) Dating methods

3) Analyses of materials

4) Mathematical treatment

5) Studies of the ancient environment

Of all the disciplines I feel that it is in mathematical treatment that the "hard" in hard scence becomes oppressive for the archaeologist. This is not surprising when we examine the diversity of disciplines reliant on the mathematical sciences. Thus an example of three-dimensional analyses arising in pure nuclear physics bears a striking superficial resemblance to the projective geometry used in the rectification of oblique aerial photographs containing archaeological features.

If we examine any one of the main divisions of Archaeometry as defined above we can approach it on a series of levels, and the question confronting the archaeologist is 'at what level?', before, as Aitken suggests, he starts to sink! I suggest that the archaeologist must be prepared to tackle the level at which he is satisfied that, by objective thought on his own part, he knows that the scientific data provided is reliable archaeologically. The scientist (correctly) will tell the archaeologist that the science is good - ie the technique is good - but is the result archaeologically valid? The archaeologist must have sufficient appreciation of the "goodness" of the scientific data to ask for a 'recount' if necessary. This implies a mutual understanding between the "hard" scientist and the archaeologist such that the former understands the weakness of his result relative to the latter's requirements.

There are good examples of the interplay, and lack of it in all branches of Archaeometry. Perhaps the most striking arise in the matter of dating, through the methods of radiocarbon and thermoremanent magnetism, where the refinements of calibration have followed necessary criticism from archaeologists who "know" that experimentally determined dates are incorrect and can point to errors. Thus we now have the so-called "precision" radiocarbon calibration which was derived from dendrochronology and high quality laboratory determinations of radiocarbon contents. It now falls to the archaeologist critically to appreciate this curve and to apply his skills to the procurement of samples for dating commensurate with the calibration he has been given. In magnetic dating the ambiguities arising from the convolutions of the calibration curve require close collaboration and understanding between the scientist and the archaeologist. Furthermore, the archaeologist must be abreast of current scientific controversy in the methods of obtaining dates so as to step back, or preferably contribute constructively to the argument. This implies risking getting the feet wet in the joint cause!

The requirement for hard science understanding in geophysical prospection is, on the face of it, not so acute. In fact, the understanding of elementary science would be often appreciated. The cynicism of the physicist to an enquiry as to whether the magnetometer can locate coin hoards as effectively as a metal detector is understandable. Even so there is again a need for appreciation at a "hard science" level of the interpretation of geophysical data and its limitations. Often complex techniques of data filtering and presentation are used by the scientist. Whether these are actually relevant to the particular problem can often only be answered by the archaeologist knowing what effect such processing may have on the data (this is as much a matter of economic use of resources as of academic usefulness).

Inevitably however the question as to the understanding of hard science, or lack of it, by the archaeologist falls back to interpretation of data and thus the mathematical treament aspect. More and more the archaeologist is asked to accept mathematical procedures which "justify" data. Methods of sampling traditionally employed in field and post-excavation studies are open to question by statisticians. It is only by understanding the questions asked that the archaeologist can defend himself. Perhaps these methods are indefensible. Then, and only then, should he accept a change! Data processed from scientific analyses of artifacts etc are often presented in a convincing form to archaeologists. Notorious amongst the techniques employed is cluster analysis which is often applied in one of its several forms to produce acceptable results and is often deceptively elegant. The question should be offered - why one method rather than another? Other approaches, more intuitively acceptable, must be used to test apparent groupings and, even then, their reliability verified. We are often presented with attractive pictures showing apparent groups but there should be enquiry about the reliability of the experimental work and even the significance of the scales used.

In many ways the methods of hard science are too naive for archaeological problems. They have often been developed within the confines of the problems of physical science. The problems of archaeology, involving temporal, cultural, technological and geographical parameters are, by their nature, very complex in the scientific sense. Quoting Renfrew in a statement made to the Washington Round Table on 'Future Directions in Archaeometry' in 1981: "I have come to believe that (the solution) lies, not only in the need for the archaeologist to understand something of the scientific techniques and their limitations but, more particularly, for the analyst to have some grasp of the complexity of the archaeological problems". Perhaps I ought to start again therefore with the quotation "Hard Archaeology: too hard for scientists?"

Bibliography

Aitken,M.J., 1974 - Physics and Archaeology. 2nd Edition. Clarendon Press, Oxford.

Renfrew,C., 1982 - "Interfacing Problems in Archaeological Science", pp.93-99, in Olin,J.S. (Ed) Future Directions in Archaeometry: A Round Table. Smithsonian Institute, Washington D.C.