1

Two tribes: questions of theory, scale and explanation

This book has been written with three aims in mind. First, it is a detailed study of the later prehistory of south-east Spain, an area acknowledged since the 1880s as one of importance for our understanding of the emergence of cultural complexity in Europe to the north and west of the Aegean. Although the data on Copper and Bronze cultures in south-east Spain have been cited in a number of recent syntheses of European prehistory (e.g. Champion *et al.* 1984; Coles and Harding 1979; Whittle 1985; Barker 1985), there has been no major analysis of the data in their own right. Lull's impressive and detailed analysis of the Argaric Bronze Age in south-east Spain (1983) comes the nearest to a full synthesis, while Gilman and Thornes (1985) adopt a finer focus (on subsistence intensification) but over a longer time depth (from Neolithic to Later Bronze Age). In writing this book I am trying to combine the strengths of these two books, taking the entire later prehistoric sequence and presenting our current understanding of the archaeological record. As will become clear, there are contradictory opinions about this understanding, and there are many problems with the reliability of the archaeological data. I have tried to use these opinions and problems productively, suggesting areas for future research. Thus I regard this book as a way of clearing the decks, and signposting what I believe to be directions for research over the next decade.

Secondly, the detailed analysis of south-east Spain is presented as a contribution to the comparative study of the emergence of cultural complexity, a topic which is of more than parochial importance. I have considered it essential to compare the archaeological record of south-east Spain with other regions of the Iberian peninsula, and with the west Mediterranean as a whole. Looking even further afield, I also offer some observations on differences in the emergence of complexity between the west and east Mediterranean basins. Such comparative analyses (which can be extended to other areas of Europe and beyond) highlight variability in the archaeological record, and such variability requires explanation.

Thirdly, I want to make some clear points about the study of archaeology as a discipline. During the last two decades it has been possible to distinguish a whole series of 'isms', or what some like to call paradigms, within archaeology. Indeed Norman Yoffee, of the University of Arizona, pointed out a few years ago that one could identify new 'isms' for nearly every letter of the alphabet (e.g. action archaeology, behavioural archaeology, cognitive archaeology, demographic archaeology, ecological archaeology, etc.). Positions change rapidly, are expressed through polemic, and in some cases are clearly related to the politics of academic self-advancement. Underlying these changes of archaeological style may be very real differences of theory and methodology, of aims and of the perception of archaeology's potential in the study of the past. For the purposes of what follows in this book, I think it is useful to abstract two polarised positions (the 'two tribes' mentioned in the title of this chapter) current within archaeology, and expressed in a list of 'oppositions':

Positivism	v.	Post-positivism
Generalisation	v.	Particularism
Science	v.	History
Processual explanation	v.	Contextual explanation
Group behaviour	v.	Individual action
Culture	v.	Ideology
Local systems	v.	World systems
Adaptation	v.	Social reproduction

Doubtless, other oppositions could be added to this list, at different levels of generality, and the proponents of these views may perceive their opponents in different ways. There is also, I think, a greater unity in the positions listed in the first column than could be claimed for those in the second column.

Within European, and more specifically British, archaeology, advocates of positions in the second column have argued vigorously in recent years against the positions in the first column (e.g. Hodder 1982a, 1982b, 1982c, 1985; Miller and Tilley 1984; Friedman and Rowlands 1977; Rowlands 1980; Whittle 1985). Assessing this debate has been an instructive exercise for me, as my archaeological education was within the framework of the positions summarised in the first column. These positions have guided my research on west Mediterranean prehistory, and it is out of this research that this book stems. Hence I am not concerned to launch a new 'ism' in this book, but I cannot avoid the current debates about our aims and methods in studying the past. As will become clear, I have profound objections to the view of archaeology proposed by authors such as those cited above. This book gives me the opportunity to state these objections, and to show how my own theoretical and methodological position determines my research in the west Mediterranean.

Before introducing the context of west Mediterranean prehistory, and the general problems under study, I will make some comments on my theoretical and methodological position as it relates to recent publications on archaeological theory and European prehistory.

1.1 Archaeology: a reaction to the reactionary view

I will use the recent publications of Ian Hodder and, to a much lesser extent, Alasdair Whittle as a means of expression my position on these recent debates. Hodder's arguments stem principally from his exposure to ethnoarchaeology in Africa, and three publications in the same year embody his criticisms of New Archaeology. Whittle, on the other hand, takes some of Hodder's main arguments as the basis of his synthesis of Neolithic Europe. Let us examine them in turn.

Hodder accuses the New Archaeology of being functionalist, over-generalising, failing to understand material culture, ignoring meaning and ideology and treating human beings as passive dupes. In his 'reactionary view' (1982a) he argues that human cultures exist and develop within unique, historical sequences, that human choice mitigates against simple generalisations about such sequences, and that the individual acts as 'an active component in social change, since the interests of individuals differ and it is in the interplay between different goals and aims that the rules of society are penetrated, reinterpreted and reformed' (1982a, p. 11). Rather than treating material culture as 'fossilised action', as he claims is the practice of New Archaeology, Hodder argues that material objects have symbolic meanings which vary according to their context, and that we cannot understand material culture unless we come to terms with these meanings. Thus 'artifacts and their organisation come to have specific cultural meanings as a result of their use in particular historical contexts' and 'each use of an artifact, through its previous associations and usage, has a significance and meaning within society so that the artifact is an active force in social change' (1982a, p. 10). Rather than the study of 'the Indian behind the artifact' as once recommended by Robert Braidwood, we are now urged to study the artefact behind the Indian behind the artefact!

These arguments are developed in relation to African ethnoarchaeological data in Hodder's book *Symbols in Action* (1982b). His principal target is the assumption that 'material culture patterning is a distorted but predictable reflection of human behaviour', with its related belief that 'material items of all forms function to enhance adaptation to the physical and human environment' (1982b, p. 11). Artefacts are 'symbols in action' and 'play an active part in forming and giving meaning to social behaviour' (1982b, p. 12). The stress on understanding material culture in relation to its meaning in particularistic, historical contexts is repeated, as is the belief that social change stems from conscious, intentional action by individuals and groups. These arguments are also repeated elsewhere (Hodder 1982c) and in discussions about the archaeological reord of Neolithic Europe (Hodder 1984).

The indebtedness of Whittle to Hodder's approach becomes immediately clear from his statement that 'material culture must not be seen as a passive reflection of social reality, but . . . is . . . an element fully involved in the web of social relations and is actively employed by people in the maintenance and transformation of social orders' (1985, p. 5). As a much-cited example, Whittle uses the caution expressed by Hodder and some of his students at the use of the archaeological evidence for death and burial to infer social differentiation, when 'burial can be variously used to mask, distort or invert social reality' (p. 5). Elsewhere in the book he clearly believes in a particularistic, historical approach to prehistoric Europe, and the intentional actions of human beings feature in 'explanations' of change (e.g. 'one of the motives for adopting agriculture in the first place in many areas was to exploit the social possibilities of settling down and intensifying production', p. 306).

It seems to me that the arguments employed by Hodder, Whittle and others embody misunderstandings, misconceptions and a kind of thinking which is inappropriate to archaeology. The accusation of functionalism may in some cases be justified, but, as has been pointed out recently by Salmon (1982, pp. 84–7), it is important to distinguish between functionalist explanations (which assume that *every* cultural feature contributes to maintaining the whole cultural system in equilibrium) and functional explanations (which examine the functions of particular features without assuming that all features function to maintain the system). Such explanations are also capable of dealing with structural change and, as I shall discuss further, do not necessarily require that the functions be conscious. Hodder himself does actually produce some functional arguments, for example relating overt differences in material culture to access to resources in the Baringo area (1982b, p. 26).

The assertion that archaeologists should espouse historical particularism and cultural relativism is certainly 'reactionary', with its legacy of Boasian anthropology. It is also strange, as Hodder has written that archaeological anthropology is 'a *generalising* science of human culture' (1982c, p. 9, my emphasis). But as Salmon (1982, pp. 8–32) and Renfrew (1984, pp. 14–19) have pointed out, not all generalisations need be universal laws, even in the hard sciences, and such generalisations as there are may have differing degrees of specificity. Indeed a comparative study of human behaviour is denied by cultural relativism (Yengoyan 1985).

A further misunderstanding relates to material culture, which is a principal component of the archaeological record, but it is not synonymous with that record. Archaeologists study that record with the aim of understanding what behaviour (as well as natural processes) produced that record. When they practise ethnoarchaeology they seek to understand the links between behaviour, material culture and the formation of the archaeological record. Without these links, we cannot hope to understand the record. To argue that New Archaeology studies material culture as 'fossilised action' misrepresents this process of theory building. Elsewhere Miller (1985, p. 34) argues that the prehistoric *record* was not 'a mere passive reflection of a past society but . . . a process of representation which acted to constitute as well as to reflect social relations'. Again this seems to confuse the role of material culture in a living society with the attempt to understand the archaeological record of a dead society.

Further criticism may be levelled at the conception of ideology in Hodder's work since, as Yengoyan has argued, it seems to be confused with the concept of culture: while culture comprises 'symbols and meanings which give coherence to a society', the basis of ideology is 'the ability of a group of individuals to utilize cultural symbols for certain wilfully designed ends' (1985, p. 332). In any case it is quite possible for there to exist common cultures or ideologies, but variable behaviour exhibited by members of those cultures. This is a point which Binford has argued consistently, and with some force, over the years (e.g. 1978, 1985a). In relation to the supposed problems created by ideology for the interpretation of mortuary practices, meaningful variability in the archaeological record may still be discerned in contexts of uniform ideologies (e.g. Kramer 1982, pp. 76–80).

The conception of behaviour as meaningful, intentional action, and the attribution of social change to such behaviour, begs all sorts of questions. Is all behaviour conscious and intentional? Anthropologists have long recognised that this is not the case (e.g. Harris 1968) and one of the contributors to *Symbolic and Structural Archaeology* even made this quite explicit: 'it is important to note that emulation can occur without being understood as a process, or the relevant groups even being aware of the changes that are taking place' (Miller 1982, p. 91). Can the individual be attributed with a major role in social change? According to Yengoyan, 'it is practically impossible to deal with the individual in any sense, unless the individual is reconstituted as an "abstract" entity which is posited on rationality, self-interest, maximisation and the conscious decision-making attributes which propel individuals into action' (1985, p. 333). Such a conception is directly at odds with Hodder's desire to 'break away from the tendency to impose modern western bourgeois values on ethnographic and archaeological data' (1982c, p. 67). What is the time-scale over which change takes place and how does this relate to the unit of analysis within archaeology? Clearly the unintended consequences of human behaviour must figure largely in any account of cultural evolution, as in biological evolution. The absence of such unintended consequences, and the emphasis upon purposive action in the work of Hodder and others, reflects their concern with short-term processes of change. Also they fail to get to grips with the notions of proximate and ultimate causality (see below).

It should be clear by now that I consider the theoretical framework embodied in the work of Hodder, Whittle and others to be misconceived and inapplicable to the study of the past. What is more, I believe that archaeology is a science (which does not, of course, rule out its study of the historical past), in that it should be concerned with the rational, empirical evaluation of what we claim to know about the past (e.g. Wylie 1985). As Binford has written, 'scientific investigation is the conscious and designed attempt to obtain an objective evaluation of the utility and accuracy of proposed ideas and propositions' (1982, p. 127). To put the same point another way, such evaluation is the 'public testing by a diversified and uncontrollable community of scientists' (Gellner 1985, p. 109; see also the discussion on the traits of 'science' which are found in the social sciences in the same reference, pp. 125–6).

Finally, there are some further misconceptions about the utility of systems thinking (see Renfrew 1984, p. 248), which has been used by critics as one of the main examples of functionalism in archaeology. However, in spite of all the criticism that is has provoked in recent years, it remains a widely used framework for the analysis of change. Of course, the definition of systems, in time and space, requires the imposition of arbitrary boundaries. This procedure has aroused the criticism that it created 'closed' systems, devoid of their wider regional context of interaction. For example, van der Leeuw has argued that the systems approach is 'atomistic' and that 'assuming that the world is made up of entities, we cannot conceive of, let alone study, phenomena as continua' (1981, p. 363). While I find much of interest in his subsequent discussion of human institutions as 'information transfer systems', I do not understand his criticism. Our knowledge of living cultures shows the use of boundaries to mark distinctions between groups of people. Given the recognition that material culture is used to express distinctions of this and other kinds (e.g. Hodder 1982b), then the definition of boundaries in the archaeological record seems a useful task. Presumably, through time, our definition of such boundaries will become less arbitrary. Even if the infinite complexity of human behaviour and its reflection in material culture appeared to defy the imagination, some arbitrary boundary definition would be necessary, simply to allow analysis to proceed.

Once a system has been defined, along with relevant subsystems or variables, then the interactions may begin to be analysed. The criticism may be raised that existing practice in selecting a limited number of variables is 'reductionist' (van der Leeuw 1981), but, as we shall see later in this chapter, this represents an early stage in the analysis. More critical is the problem, raised by Renfrew (1979b, pp. 34–7; see Cherry 1983, pp. 36–7), that systems approaches only deal with the interaction between the variables specified at the beginning of the analysis. They cannot specify the emergence of new system properties as yet. This is the problem of 'renetworking' within the system, which leads to the emergence of new properties in the system as a whole (Clarke 1968, pp. 60–1; G. P. Chapman 1977, p. 130). While this is an important problem, it appears at present to be one of mathematical modelling rather than theory (Renfrew 1979b, p. 37).

Essentially any use of systems thinking in this book is, as stated recently by Ellen, that it 'can provide an integrated framework for the analysis of change and for directing the search for explanations' (1982, p. 202). It does not assume that equilibrium maintenance is a characteristic of all systems, nor that change has an exogenous cause.

These, then, are my reactions to some of the positions taken in recent publications on archaeological theory and methodology. My conception of archaeology is one of a generalising social science, which studies the products of long-term group, rather than individual, behaviour in past cultural systems. My interest is in cultural evolution. But how should we conceive of the spatial scale of this evolution? This is an important question, and forms a focal point of the discussion in the next section, which also introduces the reader to the study of prehistory in the west Mediterranean.

1.2 The spatial context of cultural change

Central to opposed points of view about local systems and world systems (see above, p. 2) is the spatial context of cultural change. Since the beginnings of anthropology and archaeology, endogenous and exogenous causes of change have been debated (Harris 1968), whether the changes themselves were the origins of agriculture, urbanism, metallurgy or the state. Within European archaeology, the major synthesis before the 1960s was that of Gordon Childe, whose modified diffusionism enabled him to interpret the prehistoric record of change in terms of that much-quoted phrase 'the irradiation of European barbarism by Oriental civilisation' (1958, p. 70). Since the advent of systematic radiocarbon dating, this interpretative framework has been discarded substantially, as scholars such as Renfrew (e.g. 1973a, 1979a) have championed models of endogenous causality. The scale of analysis has been local. Interaction, either in the form of trade/exchange or less tangible 'communication' has been analysed as one subsystem *within* the local cultural system, rather than being given a determinant and dominant role in change (e.g. Renfrew 1972).

More recently an opposing view has developed, that such an approach makes a fundamental mistake in trying to study systems in isolation. As Gledhill and Rowlands argue, 'this position becomes untenable if the society in question cannot reproduce itself in isolation from other societies, to the extent that it would have developed differently if it were independent of its articulation into a larger system' (1982, p. 146). Even hunters and gatherers are linked through extensive kinship networks which facilitate the kind of demographic flux that is advantageous to mobile economies. Not only that, but the historical and contemporary records document their interrelationship with, and dependence upon, colonial and post-colonial economies. Tribal systems are linked through alliance networks, and the mobilisation of goods and resources which plays a central role in the building and maintenance of alliances (as well as in the emergence of hierarchical societies) may incorporate societies within a wide spatial distribution. In the context of early state societies, Friedman and Rowlands (1977) have argued that spatial expansion was part of a process designed to secure a constant flow of resources from 'peripheral' to 'core' regions. The concept of a regional economic system, derived from the world systems approach of Wallerstein (e.g. 1974), has been employed to link together periods of political expansion and contraction in the early east Mediterranean states (Mycenaean, Hittite) and in central, northern and western Europe during the second millennium bc (e.g. Rowlands 1980; Kristiansen 1981), as well as in the later Greek and Etruscan states and in the Early Iron Age of central Europe (Frankenstein and Rowlands 1978). The same arguments have been used to understand the Phoenician activity in Iberia during the eighth and ninth centuries BC (Frankenstein 1979).

In many respects this argument is strongly analogous to that advanced by Gordon Childe to link the 'core' areas of the Near Eastern civilisations to the 'peripheral' areas of barbarian Europe. Indeed one might be forgiven for commenting that the explanation of cultural change over large areas of central and western Europe in terms of rather intuitively observed 'pulsations' of political expansion and contraction seems lacking in rigour. The degree and scale of interaction and interdependence between cultural systems should be a matter for empirical determination using the archaeological record. Changes may also be observed to take place simultaneously at different scales in local, regional and inter-regional systems. There is no reason why the scale should stay constant through time, or why dependency should be assumed once complex society has emerged in one part of a continent.

But whatever the precise models of interaction used by the archaeologists (of which I will say more below), one area of Europe, the Mediterranean, has exemplified the debate over endogenous as against exogenous causality. To begin with, the sources of cultural change can be specified in the successive early states of the eastern basin and the Near East: Mesopotamia in the fourth millennium BC, Egypt in the third millennium BC, the Aegean in the second millennium BC. When we add the Etruscan and Roman states of the first millennium BC, the spatial expansion of the state appears clearly defined. The presence of the Mediterranean sea and the early

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development of boats would permit interaction over a wide area, in fact, as historical analogies demonstrate, uniting the entire basin from the Levant to Iberia (e.g. the Phoenician activity of the ninth–seventh centuries BC). Given this potential for interaction and any combination of the Childe/Wallerstein model, the spatial expansion of early state society from east to west can be argued and, indeed, has been supported by many archaeologists over the last century.

Alternative views about the emergence of the state in the Mediterranean have been forcefully expressed by Renfrew (e.g. 1972), who has used systems thinking to support an interpretation of endogenous change in the Aegean, leading to the emergence of the Minoan and Mycenaean states. Both subsistence adaptations (in the form of the intensification of production, including polyculture) and local interactions (in the form of trade and, more recently, the concept of peer polity interaction) have figured prominently in Renfrew's arguments. In addition he has pointed out, along with others (e.g. Cherry 1986, pp. 38–42), the weakness of the empirical evidence for the dependence of Aegean state formation upon subordination to earlier Near Eastern states.

While Aegean scholars may question seriously the degree to which the current archaeological data may sustain many of the key elements in explanations of Minoan and Mycenaean state origins (e.g. Cherry 1984a on deficiencies in the knowledge of settlement hierarchies, as well as the internal plans of the palatial sites), they are still working with a better database than their colleagues in much of the west Mediterranean. This is an important point for two reasons. First, it has partly conditioned the argument that cultural complexity in the prehistoric west Mediterranean was not only later, but also derivative, and secondly it has made it difficult to put the Aegean cultures within a broader, comparative framework. Before considering these points individually, let us turn to the archaeological record of the prehistoric west Mediterranean.

It is no exaggeration to refer to an explosion of new data on the west Mediterranean since the late 1960s. This may be witnessed in recent syntheses (e.g. Barker 1981; Phillips 1975; Waldren 1982) and in the proceedings of large international conferences (e.g. Barker and Hodges 1981; Waldren, Chapman, Lewthwaite and Kennard 1984; Malone and Stoddart 1985). In a review paper, I have tried to document this explosion by region (Italy, southern France, Corsica, Sardinia, Balearic islands, Iberian peninsula) and by theme (cultures and chronologies, interaction and exchange, subsistence and settlement patterns, social organisation) (Chapman 1985). In all regions, and for all themes, there have been advances, but there is great variation. For example, research into cultures and chronologies is still attracting the greatest support and is moving us forward to a position of greater resolution in the measurement of cultural change in time and space. On the other hand, research into social organisation is predictably less refined or intense. Study of subsistence and settlement patterns through on- and off-site data, as well as the use of field survey techniques, has encouraged Italian (and to a lesser extent French) scholars to move beyond cultural systematics and attempt the reconstruction of local adaptations through time. To take one example, for south Etruria (central Italy) it is now possible to begin to study the cultural, settlement, economic and demographic trends involved in the emergence of the Etruscan state out of its local Bronze Age antecedents (e.g. Potter 1979). Interaction between different regions and cultures, particularly before the emergence of the Etruscan state, is now documented by an increasing number of characterisation studies and analyses of style, from the obsidian network beginning in the sixth millennium bc, through the ivory, ostrich-egg-shell and Beaker network linking southern Iberia and north Africa in the third and early second millennia bc, to the Mycenaean, Phoenician and Greek networks of the second and first millennia bc.

The emphasis on themes of study makes it quite clear that the new data from the west Mediterranean have not been collected and analysed in a theoretical vacuum. Indeed it has been a noticeable trend of research that Mediterranean prehistorians (particularly in Italy and Iberia) are now contributing to international debates about the aims, philosophy, theory and methods of archaeology. In Spain (the area with which this book is most concerned), there have been major conferences on methods and methodology in prehistoric archaeology (*Primeras Jornadas de Metodología de Investigación Prehistórica*, 1984) and on spatial archaeology from the palaeolithic to the medieval periods (*Arqueología Espacial*, 1984). In these volumes and elsewhere is seen a concern with archaeology as a discipline and its position in the social sciences (e.g. Lull 1983, p. 15; Estévez *et al.* 1984), the need for a specifically designed philosophy of archaeology (Vicent 1982; 1984), the need for theory and the design of analytical techniques which are most suitable to answer the questions now being posed about the archaeological record (e.g. Estévez *et al.* 1984; Gasull, Lull and Sanahuja 1984), and a concern with the limitations of archaeological inference. For the first time, a major journal published a lengthy paper on archaeological methodology and philosophy (Vicent 1982). A younger generation of Spanish archaeologists is rejecting normative culture history (Martínez Navarrete 1984), which stemmed from strong ties with German thought, and French empiricism, both of which dominated the interpretations of the older, historically orientated generations. Although there are still some notable gaps in these debates (e.g. the formation processes of the archaeological record), they have encouraged different views of culture change and a more problem-orientated, comparative and inter-disciplinary approach to prehistory.

The product of this change of thought, and its accompanying data 'explosion', is to enable us to argue strongly against the 'derivative' assumption which has dogged west Mediterranean prehistory for the last century. Different questions form the basis of current research. What were the local cultural adaptations in different regions? How different in complexity were they? Were the same developmental processes visible in different regions? What were the scales of interaction at different periods and levels of complexity?

This reorientation of research leads us logically to my earlier point about viewing the emergence of the Aegean states in a wider context. John Cherry has reminded us recently that 'the rise of Aegean states cannot claim to have been explained unless we can also account for the many other negative cases both within and beyond Greece'

(1984a, pp. 21–2). These examples of the non-emergence of complexity he refers to as 'null cases', and the claimed non-appearance of complex society in the west Mediterranean until the first millennium BC is in great need of explanation (see Barker 1981, pp. 212–19 for an attempt to explain this absence in central Italy). Clearly there is a need to focus attention on similar models and variables in studying both east and west Mediterranean basins, and then to compare the results.

Thus, the view taken in this book is that the spatial scale of cultural evolution, like that of interaction (which is treated as one variable determining such evolution – see below), has to be subjected to empirical analysis. Within the context of Mediterranean prehistory, and the comparative approach to the study of cultural complexity, it is no longer assumed that the evolution of such complexity in the western basin was derived from, or dependent upon, a prior evolution in the eastern basin (for further discussion, see chapters 2–3). The data 'explosion' in west Mediterranean prehistory since the late 1960s enables us to pursue comparative analyses, to determine the scale at which such analyses should be carried out, and to explain the forms taken by cultural evolution before the first millennium bc. How is this task to be approached?

1.3 Towards explanation

Our understanding of Iberian and west Mediterranean prehistory will not increase simply as a linear function of the amount or data, or 'facts' that we collect. The palaeontologist Stephen Jay Gould expressed this view well in a different context: 'New facts, collected in old ways under guidance of old theories, rarely lead to any substantial revision of thought. Facts do not "speak for themselves", they are read in the light of theory' (1978, p. 161). This view of 'progress' in science demands a clear adoption of theory and methodology. Let us start from my expressed interest in cultural evolution. One point is of immediate relevance here. It would be a mistake to expect this evolution to be gradual, progressive, or unidirectional, as Cherry has indicated clearly in the context of archaeological research on Aegean states (1983, pp. 35–8). There is a danger that the detailed stratigraphic record of the west Mediterranean, with its definition of 'continuity', will encourage gradualism in our thinking. But disciplines as divergent as archaeology, anthropology, palaeontology and biology can provide case-studies of discontinuous evolution, with periods of 'stasis' being followed by comparatively short periods of rapid change (see references cited in Cherry 1983; also Kohl 1984). The degree of continuous change is a matter for empirical determination, rather than one of invariable assumption.

Given that we attempt to trace cultural evolution, and indeed to explain it, what constitutes an adequate explanation? Do we analyse traits (as is still the case in much of west Mediterranean archaeology) or variables? How adequate are our temporal and spatial scales of measurement? The question of explanation in archaeology is controversial and much debated (e.g. Salmon 1982; Renfrew, Rowlands and Segraves 1982). As Whallon has noted recently, 'colloquially "to explain" can often mean to paint a convincing picture, one in which gaps in what is actually known may be filled by inferential reconstruction, or even by "reasonable" conjecture' (1982, p. 156). He goes on to argue that 'a "scientific" explanation is something more than