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European Theoretical/Social Archaeology: studies in ambiguity

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Perhaps no one regional archaeology has been as impacted by the "return of grand theory in the human sciences" (Skinner l985) as European prehistory, where discussions and publications of the past 8-10 years have focused more on debating social theories than on the discovery of new "finds" or the manipulation of new techniques.

There are many tantalizing questions as to why social (in the broadest sense of the word) interpretations are now more tolerated and encouraged in European prehistory - at least from the Neolithic on. Is it more acceptable and plausible that the ancestors of the European researchers themselves had social lives of some significance? Or--although we don't agree-- many might argue that it has "merely" been that there has has been more research carried out in Europe, as if there might be a justifiable evolution from tackling questions of chronology to questions of technology and economics to investigating the relatively unknowable: the social and symbolic domains of life. Thus the lucky Europeans had reached that stage in their evolution when they could ask such questions (can eat dessert now that they have finished their potatoes). In discussing some recent research being carried out by one of us (Conkey) - research having to do with social geographies of Magdalenians in the French Pyrenees - one supporter assuaged a questioning student by reminding them that I was lucky and could do this kind of inquiry, since, after all, an economic prehistory (Bahn l984) had already been written!].

We feel that it is more likely that what we are seeing in European prehistory right now is not so much a specific interest in the social and the symbolic, but an explicit revival of interest in formulating theoretical trajectories of specific (pre)historical contexts on the continent of Europe. It is a reaction against the ahistorical modelling of the strong arm of North American archaeology and a re-affirmation of what is misappropriately called "particularistic" writing of the contexts of prehistory. The Europeans are quite self-aware and proud of the nature of this reaction and re-affirmation, as may be seen in the articles and books and conferences on the topic (Hodder, Shanks and Tilley, TAG, Bradley, Rowlands, Shennan). The point is that an interest in the context and historical trajectories of prehistory has never not been a part of European archaeology and the training of its researchers. What is appearing in the last 10 years is a veritable mosaic of scenarios on the course of European prehistory, based on much more sophisticated

theoretical modelling than before and inspired from many different sources. Not only is this mosaic unlike anything ever thought up before in Europe, but in addition much of it owes virtually nothing to - nor is it like - any of the modelling that has laboriously been developed and tested during the last 20 years in the United States.

This network of historical trajectories are synthesized into continental wide social evolutionary schemes of a kind that is rarely seen among the practitioners of North American prehistory. The social archaeologists of the United States brought up on the edict"archaeology is anthropology or it is nothing" have aimed to seek evidence for the general propositions on the working of human behaviour which could be applied to different contexts of time and space. In this quest, the prehistory of social relations (social archaeology) in the US contrasted right from the beginning with European social prehistory in that it was greatly concerned with rigorous testing and evaluation of hypotheses.

Modelling of social behaviour in US archaeology was heavily influenced by the neo-evolutionism of White, Service and Steward in testing general models of the evolution of social complexity. In that these latter general models were especially concerned with efficiency in the human extraction of energy from an ecosystem as a measure of complexity, archaeological investigations that were based on these models were only interested in monitoring human to human relationships   as epiphenomena of the relationship of humans to nature. The social system which is best adapted to an ecological situation (i.e. uses the energy of that system most efficiently in rational terms) succeeds. Transformation in the the social system will come as an adaptive response to changes in ecological system, change in resource base, growth of population etc. What the archaeologists' role is in this case is to monitor above all the relations of humans to the material world. The rigorous testing of one's hypotheses by the scientific method of logical positivism is made more convincing by reference to the material world and its links with physical, chemical and biological laws, as with investigating environmental conditions, subsistence, production, than are the workings of human-human relations.

The criticisms of the ecological-evolutionary models of social change, and cultural materialism, its most outspoken arm, is that in identifying general principles of long-term change of environmental modification and population growth, that the context of shorter term changes through time - what one might call the historical trajectory as opposed to the longer-term evolutionary trajectory - are pushed to the background as irrelevant. Thus we have a dehumanization of prehistory. The proponents of this view of archaeological research firmly state that the ecological-evolutionary/adaptationist model-building and testing is the only

valid objective "scientific" prehistory and that the alternative(s) are story-telling, scenario-building if not pure folly.

Our stereotyping of the nature of US anthropological archaeology has been to provide a background to our analysis below of how new and how specifically European are the recent developments in European theoretical archaeology. For it is our suggestion in this paper that what is new in European archaeology is the multitude of informed scenarios that are being built about European prehistory, not the new discoveries (the earliest textile at 6000 BP) nor the case studies of tests to demonstrate ever more forcefully with ever more sophisticated retrieval and manipulation of data from Europe that the best adapted society succeeds. You may disagree with us.

We have been happily distinguishing US social archaeology and European archaeology, but we are aware that this is a misleading simplification. Apart from the problem of possibly misrepresenting the uniformity of the adaptationist "school" of US social archaeology, there is no unified "European" archaeology. The "Europeans" to whom we mostly refer in this paper are either British (arguably not part of Europe) and Scandinavians. In addition, there are a number of US trained archaeologists working in Europe, especially with hunter-gatherer societies. And there are European archaeologists from Europe who have been in long contact with US archaeology. You have examples of both extremes in the writers of this paper! How do they fit in with the New Wave of theoretical musings in European prehistory?

There is no doubt that the Bronze Age and the Neolithic have been the preferred periods for the investigation of prehistoric social life in Europe (out of 14 edited volumes in the series New Directions in Archaeology put out by CUP, there are 12 that address the Neolithic and later periods, and 2 that are specifically addressing pre-Neolithic periods).

Colin Renfrew - following a well accepted doctrine - set the scene with a statement in 1972 that for hunting-gathering societies and simple agricultural societies it was enough for an archaeologist to document the ecological background and subsistence basis of the the group to be able to understand much of what was going on. Even in his review of the "return to grand theory" in archaeology, Shennan (1986) notes that the application of neo-Marxist and structural-symbolic perspectives is "at least from the Neolithic onwards". Because many of these perspectives take such things as power negotiation, conflict and resistance, and ideology as their 'entrees' into prehistoric social life, it is perhaps not surprising-- given the persistence of idealized notions on the egalitarianism of hunter-gatherers-

- that Paleolithic (and Mesolithic) hunter-gatherers have only infrequently been considered in these terms.

It is interesting that, in fact, one trend in hunter-gatherer studies over the past eight-plus years has been to understand "complexity" and the ways, or contexts , in which hunter-gatherers are NOT so egalitarian, especially those that existed after 20,000 years ago (e.g. Soffer 1985, Price and Brown 1985, Zvelebil 1986, Gamble 1982, Bender 1978). One could be led to believe that unless hunter-gatherers are "complex" and show signs of social inequality, they are not likely to have a "social" life, unless its one that has to do with such functions as mate-exchange or alliance-formation.

Most of the research on Paleolithic hunter-gatherers of Europe is still being done by Europeans, especially those on the continent itself; but unlike the researchers of later prehistory who have been dominating the theoretical scene, these Paleolithic researchers come primarily from a tradition of geology, rather than the intellectual background of history and geography. With few exceptions (e.g., Soffer 1985; Conkey 1985; Gamble 1982, Bender 1978) these and most anthropologically-trained American archaeologists doing European (especially Paleolithic) hunter-gatherers remain untouched by the spread of "grand theory", and have had little to say about the social lives of their archaeological subjects; the intellectual alliances have been more with Binford than with Bourdieu. With a few exceptions, research projects are not designed specifically to answer questions of social life. The latter, if they are tackled at all, are as a by-product or as a luxurious extra to the real meat of archaeology: palaeoeconomy. They have remained strongly entrenched in the need to carry out careful testing of empirical hypotheses, and have rarely indulged in what are rudely called flights of the imagination.

Many archaeologists dealing with the "more complex" societies of later prehistory (bronze Age-Iron Age) of Europe (Renfrew, Kristianssen, Rowlands) whether neo-marxist or not, also use a systems approach in the way that they postulate linkages between socio-economic processes. The work of the social archaeologists such as Renfrew and Shennan has been characterized in detail by Hodder and Shanks and Tilley as functionalist and mechanistic in their treatment of social relations as epiphenomena of the relationship of humans to the material world, and material culture as an epiphenomenon of social behavior. The aim of Renfrew at first in the beginning of the 1970s was to draw attention away from the Near East as the source of inspiration for transformation of European society, and to create models or scenarios of changing society in which inspiration was strictly European. His later aim and those of his students and colleagues: Shennan, Randsborg, Sherratt, Bintliff and others has been to create plausible scenarios of the evolution of hierarchical social systems within prehistoric Europe focusing on the causal factor

of the interaction of social groups or systems. Their implicit belief is that by a natural progression of population growth and competition for power and control of production and movement of material resources more complex social forms of organization have developed. Much of their theoretical basis is similar to that described for the social archaeologists of US anthropological archaeology. Their use of the data may be described as much looser - and has been criticised as such by Binford and Whallon - and their purpose is to look at the historical trajectory in Europe and the Mediterranean basin, rather than use European examples as case-studies to demonstrate a proposition of generalized human behavior pattern.

The data base of "Social Archaeology" comprises three major sources: burial data, improved methods of settlement survey on a regional basis and increasingly sophisticated data on materials analysis providing information on the sourcing to reconstruct the exchange networks (especially of exotic items) linking the surveyed and excavated settlements and graves. The focus on these sources of data reflects the research priority of Social Archaeologists in inter-settlement relations, especially in terms of the interaction of political leaders. Intra-settlement relations and subsistence economics are clearly less important for them. re critique of data use.

 Exactly the same data base of European prehistory has been used by archaeologists with a quite different set of beliefs about the working of society. These include neo-marxists, symbolic structuralists, and other post-moderns. A quick glance at the authorship of monographs put out in the New Directions series will show that the archaeologists writing theoretical based works are by no located in exclusive camps; there is a great deal of overlap and open dialogue in writing and conferences.

The models of many of the neo-marxists (Krisitiansen) are firmly rooted into a systemic way of ordering and manipulating their data. Thus they have not overcome a fear of re-conceptualizing their categories. What this adherence to a systemic model creates is an inability (paralysis) of being able to envisage the actual mechanism of a change from one network of interacting systems (i.e. at one slice of time) to a subsequent time-period. Although Rowlands may have explicitly rejected the opposition between the diachronic and the synchronic, in practice in most neo-marxist modelling is a sophisticated description of how society works at different periods, but little on actual transformation, which should be the essential part of a historical materialist analysis. Thus they also - like the more traditional social archaeologists -have tended to monitor change rather than seek out dialectic of change. This a criticism that has been pointed out of Kristiansen's, Rowland's and Bradley's work at least by other theoriests such as Tilley and Shennan.

The World Systems approach that has been adopted by many neo-marxists in their modelling has acted as a direct challenge to the model of European autonomy. As Shennan has pointed out, however, the world system modelling does not represent some cyclical return to the Childean diffusion models in which the impact of the Mediterranean world system on Continental Europe was regarded as creative and positive. On the contrary, Rowlands and others regard the impact as the"exploitative creation of dependence and underdevelment".

Two recent developments in neo-marxist archaeology have attempted to tackle the problem of doing more than monitoring change, but aim at imagining how and why change may have occurred and to model the contradictions which may have led to the trajectory of the social formation. Neither is perfect but they are part of this myriad of models that now proliferates European archaeology. Both of these developments suggest the need for a consideration of social life at a spatio-temporal microscale.

The microscale that I have suggested is that of a household and a generation, or a human lifetime. The part of time and space in European history that I have written my scenario for (or modelled) and struggled with in-field primary research is exactly the same as that dealt with by many other social archaeologists: SE European Neolithic-Copper Age. The recent trend in popularity of "Household archaeology" has recently enjoyed popularity in US (Wilk and Rathje 1982, Flannery etc) where it has not consciously been part of the development of any neo-marxist oriented studies in archaeology. The trend was aimed at filling in the most detailed level of settlement pattern analysis and obtaining information on population patterns, specialized production, class structure. It was suggested, moreover, that household analysis allows us to "bridge the existing 'mid-level theory gap' in archaeology" since households "are the level at which a social group articulates directly with economic and ecological processes".

My own feeling about the study of households (which is still in its infancy in European archaeology) is that it satisfies all of these purported advantages, and that in particular it is the vehicle with which one may study the social relations of production in prehistory. I though that by the inclusion of the household as the minimal unit of analysis one would be able to actually carry out a historical materialist analysis of the Southeast European Neolithic in keeping with the methodology described in the various manifestos on the subject (unveiling the contradictions which lie at the basis of change) rather than just promising this in theory. One of the requirements of such an analysis of sociohistorical formations is that it should be carried out at a variety of spatio-temporal scales, from very small to very broad. Thus in such an analysis of European prehistory, one is less interested in describing what the household did in different times and places than

in explaining its changing functions (actions) through time, and the effect of this on other patterns at both the household (minimal scale), village and regional (macroscale) level. It is obviously of crucial importance to be able to envisage this changing role through time.

It has recently been pointed out to me that envisaging the household and household co-operative action is still not a small or sensitive enough unit to be able to use it to unveil contradictions and "explain" change. What is needed is to make the household into what Childe has called a "plurality of human individuals or persons who cooperate" - observable persons : households with faces, household with gender, age etc.

The other recent trend of neo-marxist archaeology has also laid emphasis on the microscale, but this time that of the indivdual (Tilley). This has gone along with a growth in interest in ideology "as part of what makes up reality, in the sense that people act, interact and produce effects in terms of their forms of consciousness, deluded or not". In Shennan's view, two strands of post-processual archaeology have (neo-marxist and structural-symbolic [Hodder) have come together in the concept of ideology and the work of such archaeologists as Tilley. Central to Tilley's work is an idea of material culture as having a much more active creative role in social change than was conceived in the more traditional social archaeology in which material culture is an epiphenomenon of social behavior. This role of material culture is understood within the context of individual agents and their actions.

That Tilley should suggest such a strategy of analyzing the archaeological data is not surprising in view of the fact that he is a student of Ian Hodder's, whose more recent work has suggested that material culture is created within the ideological context of groups of people, and is patterned as part of socio-economy so that its structure can be tackled by archaeologists and will reflect the social formation and its contradictions at the level of the individual agents of change.

Recently Stephen Shennan has suggested that such a treatment of material culture can give strength to neo-marxist explanations, such as those of Kristiansen, Rowlands and Bradley, which previously did not have a view of the nature of archaeological record or material culture in general BUT did contain ideas about how societies operate and change.

This is really a very different concept from the more accepted treatment of material culture as reflecting the results of actions and behaviors. Traditionally - if the methodology of the "scientific archaeology" has by now become a tradition - the reconstruction of social behavior would be achieved by building a bridge between abstract theories of behavior and observations of the empirical archaeological data

through the medium of a series of empirical hypotheses arranged in the hierarchical levels of middle range research. The abstract theories of behavioral change which can most successfully be validated to the satisfaction of one's Establishment colleagues by middle range research are those which deal with the relationship of human societies to the material world, such as, for example, the human manipulation of the material world - the means of production, subsistence economics, resource utilization.

The criticism that has been levelled at the theoretical archaeologists of Britain from the advocates of "scientific or real archaeology" in US has focused on the fact that the British are not formulating models from which to derive testable hypotheses, but that they are creating scenarios of historical events which are specifically applicable to one particular historical context only. It follows from this that the scenarios that have been written have not been demonstrably supported by a rigorous analysis of the empirical data of the archaeological record within the standards expected of the research strategy practiced and developed in American anthropological archaeology. I am enough of a materialist to believe that a middle range research strategy can do a lot to provide a more sophisticated and elaborate data base from which to formulate plausible scenarios about, for example, the dialectical process of the transformation of the social relations of production. Others have suggested such a compromise or synthesis of the "insights of evolutionary-ecological rationalism.....with those of historical materialism" (Marquardt 1985).

Thus the pluralism that we shall advocate at the end of this paper in consideration of alternative models of change could be extended to the strategies of dealing with the archaeological record. In addition to the synthesis suggested above, one could imagine (dare we say suggest) the reverse, a middle range research investigation of material culture as an epiphenomenon of social actions could be supplemented by a study of the same material culture as an active part of the ideological context of society, as advocated by Hodder and Tilley.

We can say on the basis of our preliminary and far from complete experience, either kind of syntheses are virtually unheard of in current archaeological research.

What about the apparent impasse of what constitutes a valid theory of human history? Neomarxist explanation is irreducibly specific and historical and leads towards an "elucidation of deep generative mechanisms (which naturally involve the use of terms and concepts of more general applicability) as opposed to regularities in 'surface phenomena'". The very essence of neomarxist research is to write ".. specific historical accounts in which key relationships and structures are uncovered". This seeming insoluble rift between the two ways of thinking is

expressed as violent rejection of post-modern archaeological theorizing by Binford and of dialectical materialsm by Price (1982) as a valid form of archaeology.

Among the European archaeologists themselves, the multitude of "analytical scenarios" to explain the same historical trajectories has led to less violent discourse, but nevertheless quite different views. How, if ever, are these differences to be reconciliated? Which is the "right" view, i.e. closer to the true story? Many times it appears that this debate in archaeology has been about something as basic as the formation of knowledge with its ultimate goal as the revelation of truth. But what in fact we are dealing with in most cases are probably propositions of a more metaphysical nature.

If it is knowledge that we are after, then we may follow Gordon Childe in his little read book (ha ha) of 1956 in which he stated - following a classic marxist manifesto that knowledge is a system of propositions which are true insofar as they correspond with the external world. The function of knowledge is to be a guide to public (as opposed to individual) action; success of that action is the test of the truth of proposition (knowledge) from which action is derived. Thus it follows that knowledge is to be acted on, not contemplated. Rowlands almost repeated the same sentiment in his suggestion that "academic analysis is part of the process of changing the world, not merely observing it".

Beliefs, on the other hand, are propositions of a similar nature to knowledge BUT they can be held privately; need not be endorsed by society. Beliefs are different from knowledge in that they are metaphysical propositions that claim truth BUT are exempt from an operational test. A metaphysical proposition (belief) transcends the boundaries of experience, therefore can only express beliefs not knowledge. They are not used as a guide to action.

In 1956 Childe was able to show the success of the system of knowledge embodied in Western capitalism: "Slowly but inevitably the superiority of our system of propositions both in content and structure can be demonstrated (RET by brute force etc.) in practical application". Insofar as archaeology provides a guide to the success of this action, it may be said to be knowledge. At present, there is no doubt that this has something to do with much of the ecological-evolutionary adaptationist schemes of human society's history being accepted widely as knowledge. However, many have remarked how archaeological "knowledge" is not taken seriously by society and is not used as a guide to action. It has not practical applicability or relevance (even Childe said that in 1956). Clarke et al.: broadly marxist approach but is middle class coffee table symbol of prestige and published by Thatcher govt: "indicative of how unseriously archaeology is taken. We can be

left to practice our trivial pursuit because it doesn't make any difference to anybody".

The suggestion of the post-modern theoretical archaeologists in Europe as well as a miniscule number in the US is that archaeological beliefs can be turned into knowledge only through the powerful medium of critical analysis.

Many, but not all, of the social archaeologies derive from "post-modern critiques that challenge any notion that there might be foundational, trans-contextual valid standards such as scientific rationality was once thought to embody. The essential thesis is an unequivocal contextualism..."(Wylie l988:11). Long ago, Ascher (l961) took the position --in his defense of analogy in archaeological reasoning-- that "archaeologists should give up the paralyzing demand for certainty ..." (as interpreted by Wylie l985:80). Social science inquiry is always underdetermined (Giddens), and the requirements for certainty have been not only self-imposed but restrictive. It is little wonder that, if one adheres to the restrictive demands, one is left to discuss primarily--if not exclusively-- the archaeological record, as materials, as statics (Binford l983).

Geuss has written of critical analysis in general in the social sciences that valid social analysis (including archaeology) must embody a critical analysis, that is, an enterprise "dedicated towards the emancipation of individuals from the coercive ideological conditions in which they not only act but in which their very beliefs and ideals are formed". If critical theory represents the most advanced form of consciousness available to us in our given historical situation, need worry whether or not something is "true". BUT still to have such an effect, critical theory must be "knowledge" i.e. must show ideology to be false .

We may well be at a place now in archaeology where the process whereby we establish our knowledge about the cultural past involves divergent methodologies and epistemic standards. Yes, a move away from standard scientific methodologies--as are apparent in many of the social archaeologies-- towards textual models of analysis does produce a very different sort of understanding, subject to distinct criteria of adequacy which are distinct from those of the "scientific method". Given that each (method) is inherently limited, the sensible strategy, suggests Wylie (l988:11), is to encourage a diversity of options. Diversity can manifest itself in one researcher or in one archaeological report. In fact, the kind of deliberate ambivalence --that Harding (l986) proposes for feminist inquiry-- "seems quite attractive when considered in the face of the sharply adversarial conflict that presently divides archaeology" (Wylie l988:). Some degree of tolerance of a methodological and theoretical pluralism is essential. The archaeology of social life has been both stimulated by, and demands!, a certain

ambiguity and ambivalence. "We should 'learn to live with [the inconsistencies and tensions]' created by the juxtaposition of (these) different approaches; they are productive tensions ..." (Wylie l988:11, following Harding l986).

Finally we can ask the question whether the scenario-building and historical materialist analyses of much post-processual archaeology with its emphasis on critical analysis will work better for post-Modern world of 1990s than for mechanistic world of 1970s and 1980s. We can answer, maybe, but only if socially accepted aims of action and values (beliefs) change from their present emphasis progress is equated with increased production. The aims that drive much of post-modern archaeological investigation (imaginings if you will) are to create a series of propositions of gender and class relations, of alternatives to centralized production and residence, alternative technologies, job classifications, work group composition which would help guide action of 1990s. What the Europeans are trying to do in this case is to re-conceptualize not only how archaeological knowledge is created, but how this knowledge is transformed into social action.

We may repeat the question posed at the beginning of this paper as to why is such a remarkable re-conceptualization of archaeological investigation so prevalent in Europe, particularly Britain and Scandinavia, at this point in time?

In a recent consideration of epistemological issues in archaeology, Wylie reminds us that "Where theory inevitably over-reaches evidence-- and evidence is, in any case, theory-laden-- appeals to the 'facts' cannot settle questions of theory-choice on their own" (Wylie l988:12). "The slack", she argues, is inevitably taken up by preconceptions specific to the scientific community or to the political or socioeconomic standpoint of individual theorists. It has, of course been well-argued that "the facts" that social scientists mean to establish and to which they appeal are "themselves constructs produced and reproduced in an on-going (irreducibly social and political) process of negotiation among the various particpants in the research" (Wylie l988: 12). As Wylie summarizes, "institutional structures and political interests of various sorts... determine the outcome of scientific debates much more extensively and directly ..." (Wylie l988:12). [Certainly much of French Paleolithic archaeology can best be understood through this perspective].

As Shennan (l986:330) points out, many of "those who have taken up such ideas (as neo-marxism, the individual and active social agent) have in general been sympathetic to varying degrees with left-wing political position". Indeed, it is not surprising that contemporary concerns with social negotiations, social strategies, and social forms--particularly in those societies (e.g., Great Britain and

Scandinavia) where academic jobs are scarce and the socioeconomic system under particular stress--are now more easily 'read into' the accounts of prehistory.

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