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CLIVE GAMBLE



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PULLING ASIDE THE PALAEOOLITHIC CURTAIN

Society represents a claim, advanced and enacted in order to construct a state of affairs that previously was not.

Eric Wolf
Inventing society

Introduction

The Palaeolithic of Europe is a record of observations and a register of ideas. Using this combination, archaeologists have recognized many patterns in the data and proposed several solutions, at a continental scale, to their recurrence in time and space. These explanations have emphasized the variable biological capacities of early hominids, their adaptations to ecological circumstances and their expression, in the form of stone tools, of persistent regional identities. The transforming powers of human migration, combined with the selective diffusion of cultural elements, have aided both the writing of culture history and the investigation of the same Palaeolithic records as long-term adaptive systems involving continuity and change.

During these examinations of data and the development of analytical concepts the subject of this book, the varied societies of Palaeolithic Europe, has received some attention. But investigating society in the Palaeolithic has never achieved the same research prominence as studies of the subsistence economy, the spatial analysis of settlements, cave art or lithic typology and technology. While great strides forward have been made by archaeologists studying the societies of later prehistory (Hodder 1990a, Renfrew 1973), their insights and approaches have generally only touched upon interpretations of the Upper Palaeolithic (Clark and Lindly 1991, Price and Brown 1985a, Soffer 1987). The half million years of occupation in Europe prior to the Upper Palaeolithic remains peopled by ecological creatures rather than social actors. We have created these half-men, half-beasts, through the unequal juxtaposition of their simple stone technologies against the gigantic forces of Quaternary geology. According to our assessments, they wait patiently for the arrival of modern humans from Africa to put them out of their long servitude to grinding nature.

Gordon Childe was particularly gloomy about the prospects of social reconstruction in this period. Writing in *Social evolution*, he expressed a widespread view:

The archaeological record is found to be regrettably but not surprisingly deficient in indications of the social organisation or lack of it in lower palaeolithic hordes. From the scraps available no generalizations are permissible. (Childe 1951:85)

Neither was he particularly encouraging about the Upper Palaeolithic and Mesolithic where, even though the record was fuller, it was very one sided.

It offers a clear picture of nothing more than the economy and material culture adapted to particular environments of ice age Europe and of the northern forest zone in early post glacial times. (*ibid.*)

These Mesolithic folk were also apparently waiting to be replaced, albeit by agriculturalists. Writing thirty years after Childe, Richard Bradley could memorably bemoan that archaeologists still stressed how

successful farmers have social relations with one another, while hunter-gatherers have ecological relations with hazelnuts. (Bradley 1984:11)

This picture has now begun to change. A degree of self-determination is currently accorded Mesolithic people, while the complexity of their regional archaeological records (Bonsall 1991) is proving a rich source for social reconstruction, as it is in the Upper Palaeolithic.

The image of the earlier Palaeolithic

Any progress that might be made towards a unified social archaeology generally founders on the rocks of the earlier Palaeolithic. What has happened since Childe's remarks in 1951 is that the Lower and Middle Palaeolithic have now achieved the research status that he gave then to the Upper Palaeolithic and Mesolithic. In the past forty years large-scale, multi-disciplinary projects have recovered a wealth of archaeological and environmental data from sites throughout Europe. Moreover, the development of a continuous Pleistocene chronology based on the deep-sea isotopic record (Imbrie and Imbrie 1979), and science-based dating beyond the period covered by radiocarbon (Aitken 1990), has produced frameworks which encourage interpretation rather than mere description.

These interpretations are frequently presented visually. Many of the projects provide artists' reconstructions of the landscapes these European hominids inhabited (Lumley 1979, Mania and Dietzel 1980). As Moser has argued, these images 'are more than just illustrations which summarize information presented in the text; rather they are reinforcements of . . . theories, and as such constitute arguments in themselves' (1992:833). By presenting the excavated facts as visual summaries, it is also implicit as to how we should interpret them. We see hominids attacking bison in the lake at Bilzingsleben (Mania and Dietzel 1980), living in their tent at Lazaret (Lumley 1969a) or planning, executing and then celebrating the elephant hunt at Torralba/Ambrona (Howell 1965). Through these devices, the unfamiliar deep-past, where the archaeolog-

ical 'scraps' are indeed meagre when compared to other periods, becomes known and knowable.

These images control our theoretical imaginations rather more than we care to acknowledge (Stoczkowski 1994, Gamble 1992a, Moser 1998, Moser and Gamble 1997, Gifford-Gonzalez 1993). They make us feel comfortable with a natural-history approach to reconstructing the past. The pictures compare with the way geologists created scenes from deep-time during the last century (Rudwick 1976) in order to establish their claims to be able to reconstruct the extinct worlds of the Eocene, the Carboniferous or any geological epoch they chose.

Such reconstructions are valuable in expressing 'natural' relationships between the multifarious strands of data that are routinely collected and analysed on any Palaeolithic site. Nowadays an image of a hominid in a landscape can combine evidence from the analysis of ostracods, mammalian micro- and macrofaunas, herpetofaunas, calcareous nannoplanktons, soil micromorphology, palynology, geology and sedimentology, to name a few of the possibilities now in the Quaternary scientist's armoury. We frequently use these images of the deep-time past, as attendance at any Palaeolithic conference reveals, when it comes to showing what we think all those data mean. Using these images allows us to reach consensus about what regularly took place – hunting, gathering, cooking, eating, child care, tool manufacture, hide scraping, sleeping, burying the dead – and which sexes and age groups were largely responsible (Gifford-Gonzalez 1993, Moser 1989).

But while these images appear to open a window into the Palaeolithic, they also close curtains. The most important curtain they draw across our investigations is the subject of this book, Palaeolithic society. The reason for this is quite straightforward. The Palaeolithic has never been a seed-bed for the discussion of social theory within archaeology. Instead there have been extremely important debates about how the Palaeolithic record was formed and the causes of subsequent variability in lithic and faunal remains (Binford 1973, 1981a, 1981b, Binford and Binford 1966, Bordes and de Sonneville-Bordes 1970, Dibble 1987, Mellars 1996, Mellars 1970, Stiner and Kuhn 1992). Such middle-range theory (Binford 1983a) has been promoted in the fields of taphonomy and the study of site formation processes such that we now have a much clearer idea of what is known and is knowable in the Palaeolithic. New data have been generated, and with them the opportunity to recognize fresh patterns at both a site and a regional scale. However, these patterns have not been explained as the result of social factors. Indeed, the Binfords' critique of the five Mousterian tribes of southwest France (Bordes 1968a, 1972) defined by their different lithic assemblages was a deliberate move away from culture history and social explanation.

While the record of observations has grown enormously, the register of ideas has also kept pace, albeit on a predictable track. The track is indicated by those artists' images which capture the ecological relations between hominids and resources in the landscapes of the Palaeolithic. Elsewhere, these relationships

have been usefully expanded through the insights of evolutionary ecology to include discussion of optimality (Bettinger 1991), decision rules (Mithen 1990) or risk avoidance (Torrence 1989) among prehistoric foragers, but the emphasis is firmly on the side of a natural rather than a social prehistory.

Social relationships are not easily portrayed except as symbolic diagrams, but see the cover illustration for this book by M. Antón. Hence an anthropologist will provide a kinship chart rather than a photograph. These charts are an essential aid to their arguments about the structure of social life. For archaeologists, the naturalistic reconstructions of bison hunting at Bilzingsleben are more 'believable', because they are at least refutable through taphonomic enquiry, than the imaginative depiction of the same hominids worshipping the dawn by a lake in eastern Germany 300,000 years ago. An artist's reconstruction of a complex concept such as society would meet similar difficulties, not least because the artist would start without any guidance from scientific convention about how to depict social life in the past. Those geological scenes from deep-time provided a convention for reconstructing the natural landscape to make it accessible. Societies, animal or human, were never shown. Only representative taxa appeared as though in some taxidermist's workshop.

Palaeolithic oppositions, convictions and solutions

A commitment to a social archaeology has existed for some time in later prehistory (Flannery 1976, Friedman and Rowlands 1977, Redman et al. 1978, Renfrew 1973, Renfrew and Shennan 1982) and from this largely processual and materialist base various alternative social theories have recently been explored (Barrett 1994, Barrett, Bradley and Green 1991, Bradley 1984, Gosden 1994, Hodder 1990a, Moore 1986, Shanks and Tilley 1987a and 1987b, Thomas 1991). In particular, these later works have stressed the archaeological record as text, replete with meanings and inscribed ambiguity, which constitutes a landscape where social actors are involved in deliberate action. The involvement of social agents in the creation of their varied and dynamic social worlds stands in stark contrast to the earlier texts in social archaeology. Here the investigation of the social system and its various regulatory states formed the focus. The social creativity of individuals was crushed by an overbearing system.

The eons of the Palaeolithic when little seems to change, combined with those 'scraps' mentioned by Childe, have never suggested any other interpretation than a natural, or at best a systemic one. But even with the latter it seems that for long periods of time certain key sub-systems, such as religion and craft specialization, were entirely lacking. This makes it even more difficult to argue that change was internally produced by friction between sub-systems and where homeostasis was upset. Dynamic change therefore had to be supplied from outside. This can mean either the replacement of one population by another or the climatic cycles of the Pleistocene forcing changes in subsistence, settlement and associated technologies.

As a result, explanation in the Palaeolithic has stressed oppositions between ancient and modern humans, environment and culture, foraging and agriculture. This approach has established what many archaeologists still see as the truly important purpose of studying earliest prehistory: to discover when and where the foundations of the civilizing process, from which the western world ultimately benefited most, were laid (Gamble 1993a:20–3). For many years the Palaeolithic has been divided from the rest of archaeology by an interpretative curtain founded on these oppositions. For this reason as much as the ‘scraps’ of data argument, the Palaeolithic has not been part of the later developments in social archaeology, and hence has been largely ignored by post-processualists (but see Hodder 1990a).

Many would regard this neglect as positively beneficial to Palaeolithic studies, and for a long time I would have agreed with them. I have changed my mind not because I am yet convinced by all of the post-processualist agenda, but rather because their attacks have highlighted the poverty of theory in my area of expertise. This poverty touches most on the units of analysis that the Palaeolithic uses and the goals it has set itself. These are explored more fully below and in Chapters 2 and 3. My intention is to tweak that interpretative curtain aside and seek solutions that do not rely upon traditional divisions.

Of course I am not working alone at this project. The tweaking has already begun. The interpretative curtain, which seems to divide an *active* human past involving trade, agriculture, ritual, monuments, towns and power from a *passive* one of adaptation to the conditions of existence, is, in the case of the Mesolithic, already being pulled aside (Bender 1985:21). Neither does there seem to be any problem with drawing it further to include the rich record of the Upper Palaeolithic (Price and Brown 1985a, Soffer 1987). However, while these gains in approaching the past as a continuous stream are not negligible, it does appear that the curtain which separates Us from the Other, Civilized from Uncivilized, now falls across the Middle to Upper, Neanderthal to Modern, transition (Gamble 1991, Stringer and Gamble 1993). All we have apparently done is to shift its position by some 30,000 years.

My contention is that we should scrap this curtain altogether. The deep-time past of the Palaeolithic does not need to be investigated through such oppositions. As long as we maintain these contrasts we can only pursue a divided archaeology, where much of the Palaeolithic will remain remote to the interests of most archaeologists. The study of human origins then becomes nothing more than a story from a faraway land, as remote from people as the epochs of geology.

My solution is to take society as a unifying focus. This may seem a strange choice given the complications of defining, using and interpreting such a concept. I have adopted it due to several convictions explored in a number of previous publications.

Firstly, I can still see no justification why archaeology should have different procedures and standards on either side of an interpretative curtain which

divides modern from ancient humanity. There is no reason to switch explanations from predominantly ecological to mainly social just because the nature of the archaeological record changes with the advent of agriculture and sedentism (Barker and Gamble 1985, Gamble 1986a, 1986b, 1986c). Neither is there any reason to change explanations during the Palaeolithic as soon as a more varied record appears with the Upper Palaeolithic. My interest is in the differences among Palaeolithic societies and not the divisions between them. My concern in this book is with analysis rather than definitions.

Secondly, understanding social life in all its different forms should be an archaeological goal since, as anthropologists of the past, we have the data to enlarge and enrich such experience (Gamble 1991a, 1991b, 1993a, 1993b, 1993c). We would be poor anthropologists if we made no contribution to the variety of societies available for systematic study (Gamble and Soffer 1990, Soffer and Gamble 1990, Stringer and Gamble 1993). The investigation of Palaeolithic societies is, in particular, an exploration of alternatives to ethnographic experience. As put by Wobst,

Archaeologists are the only anthropologists whose data contain information about behavioural variance in all its dimensions: in personnel from single individuals in private to the largest structural poses; in space from the smallest catchment area to the largest continent-wide population matrix, and in time from single events to millennia. (Wobst 1978:307)

Thirdly, the explanation of stasis and change lies at the centre of archaeological enquiry. Social life was the heart which kept the varied systems beating, whether or not change occurred (Gamble 1993a, 1993d, 1995a, 1995b). In most cases the investigation of stasis and change proceeds little beyond opting for a variable in the social system, such as population pressure on resources. Despite a number of telling critiques of such simplistic, asocial, explanations of change and intensification (Bender 1978, 1981, Lourandos 1977, 1985), there remain unreconstructed social evolutionists who trace all human developments to a single cause such as population numbers and environmental constraints (e.g. Johnson and Earle 1987). I will side with the critics of these old-fashioned texts by proposing a different view of the relations between humans and their environments. I will stress the active engagement and mutual involvement of the individual in the construction and negotiation of his/her environment.

Finally, it is time that archaeology claimed its place within the social as well as the historical sciences. To do this we need to set our own comparative agenda, not only to provide reflections on the uses of the past to construct contemporary societies (Gamble 1993b, 1993c), but to provide the prehistoric perspective on those modern interactions (Gamble 1992b). This position is well expressed by Binford:

we have a chance to understand humankind in a way that no participant, or no social scientist addressing the quick-time events of direct social experience, could ever imagine. To fail to recognise this potential, to fail to grasp a new understanding of humankind from this different perspective – the perspective of the macroforces that con-

dition and modify lifeways in contexts unappreciated by the participants within complex thermodynamic systems – is quite literally to ‘abandon our birthright’. (1989a:52)

It can fairly be stated that the social sciences have hardly noticed the absence of prehistoric archaeology, let alone the Palaeolithic, from their syntheses of social life. This situation is unlikely to change unless the case for an historical dimension, and one which involves an understanding of the character of the long-term prehistoric record (Murray 1987), is strongly put. The insights of social theory, and in particular the vocabulary of network analysis, have been adopted in this book in order to effect a translation between the very different evidence for human social action at varied timescales and degrees of resolution. However, I will not be arguing for a palaeosociology of the Palaeolithic any more than for a palaeopsychology or palaeoanthropology of the period. The historical and material basis of archaeology conditions the accounts of past societies and individuals that we produce. There will be no obvious support in such accounts for the claims of one social theory over another that aims to understand individuals and societies in the modern world. The methods and concerns of the present and the past separate these various projects in social analysis.

However, analysing prehistoric societies would ultimately serve little purpose if it only had relevance for archaeologists. Our contribution must therefore be directed towards influencing those future syntheses of social theory. I imagine these syntheses will continue to recognize, in standard post-modern fashion, that industrialization and civilization, while dominant social forms, do not include all human social experience let alone represent its highest achievements. It is here that archaeology has a contribution to make. Synthesis, by the very nature of the process which produces it and the understanding which comes from undertaking it, must continually seek to broaden our experience. Prehistory provides one route for such continuing awareness.

The narratives of theoretical archaeology

A major factor that has muted our archaeological voice has been the absence of a common structure for discussion and comparison. A reason for this has been Palaeolithic archaeologists’ adherence to the scientific method. In its crudest form this subscribes to an objective view of facts as neutral, and a clear belief in a structure that exists ‘out there’ and which will be revealed through greater analytical precision and yet more facts. For example, Flannery and Marcus have recently expressed their belief in this objectivity as follows:

Whenever science is combined with a social or political agenda – no matter how noble that agenda may be – *it is inevitably science that suffers*. (Flannery and Marcus 1994:441, my emphasis)

On the contrary, I find it difficult to see how a branch of historical science, like Palaeolithic archaeology, *can* be separated from a social or political agenda

(Gamble 1993b, 1993c) if it is to have any value as a form of knowledge. Studying the past, just like studying particle physics, is not a neutral activity undertaken for its own sake by individuals who, while doing it, temporarily suspend their membership of humanity.

Not surprisingly, then, many social theories have had an uncomfortable relationship with the scientific approach to knowledge. The sticking point seems to be the way that knowledge claims are verified and causation established (Clark 1993). Some of these fears lead to an almost entire rejection of the claims of science, as for example in the work of Giddens (1984). This rejection has been picked up by archaeologists who take to task the narrow scientific view which is all too common in archaeological writings. They point out, quite correctly, that it leaves human action out of the equation (Thomas 1991).

However, nearly everyone agrees that the scientific method undoubtedly works as a means to structure the investigation of the world in the physical sciences. Palaeolithic archaeology has made considerable procedural advances by adopting a scientific approach. But, this approach has led, as we shall see, to a rather limited view of what the data can tell us about society. The similarity with my earlier discussion about the restricted range of visual images is striking.

So, by choosing society as my focus I am seeking to expand the present boundaries of Palaeolithic discussion. This does not mean a break with past approaches. In writing this book I have not found it possible, or desirable, to abandon the procedures of the scientific approach in dealing with the question of Palaeolithic societies. I too am concerned with the verification of knowledge claims. Hence there is considerable discussion of ecology and environment in the chapters which follow, not because these provide an objective standard but rather a mutual one for interpreting past human action. What I have attempted to do is to broaden the scope of the scientific method by considering what a common approach between the historical and social sciences might entail. Such a realignment must involve not only the conceptual issues, such as society, which are addressed, but also the manner in which they are expressed. Here, I take my lead from Haraway, who observes in another context – the use of primate studies in contemporary science and society – that facts are only meaningful when contained within stories (1989). However, this approach still needs a caveat for those, like myself, who are interested in how we verify our narratives.

We should resist the temptation to assume that since stories are stories they are, in some sense, unreal or untrue, for this is to suppose that the only reality or true truth, is one in which we, as living, experiencing beings, can have no part at all. (Ingold 1993:153)

The problem with the Palaeolithic is that currently there are very few stories. Those which exist deal mostly with origins (Conkey and Williams 1991, Knight 1991a, 1991b, Landau 1991) and the inception of change, rather than with the long periods of stasis between these rare moments of excitement. In adopting society as my focus I aim to turn the spotlight on the narrative framework

which is needed to examine and explain the data from very long periods of time, the 500,000 years of the European Palaeolithic. It is during this time when our register of ideas, just like the few images in the artist's portfolio, runs low.

A social approach to the Palaeolithic requires, among other things, that as well as recognizing the patterns in the data we can also reproduce them. At the outset it is important to understand that these Palaeolithic societies, however we might eventually define them, are not waiting to be revealed through the simple tabulation of their attributes. The challenge that the Palaeolithic presents, and one reason the interpretative curtain exists, is that the basic assumptions of the elements of social life which underpin so much of social archaeology cannot be taken for granted. Minimally these might concern language, material-based symbolism, reflexive thought, the metaphor of kinship, ritual and the differential exercise of power. From such a potent cocktail we are able to mix many social theories (Turner 1991) without once mentioning subsistence, ecology, natural selection and demography.

Studies of Palaeolithic society: human social origins

While Childe, in 1951, was pronouncing on the deficiencies of the Lower Palaeolithic record, research had already begun to fashion a model of social organization from those elements just mentioned above. This culminated in one of the great edited syntheses of modern anthropology, *Social life of early man* (Washburn 1961). Among the contributors to this Wenner-Gren symposium were physical anthropologists, palaeontologists, primatologists, psychologists, geneticists, zoologists, geologists and archaeologists.¹

Although I refer to it as a 'great' synthesis, anyone coming to it cold might be excused for wondering why. The papers seem to be saying very little about social organization and rather more about a timetable of events such as the appearance of cannibalism and the first use of fire (Washburn 1961:viii-ix). There are occasionally conclusions that appear to address a social perspective, even if they seemed to state the obvious:

All [physical] evidence suggests that the Palaeolithic bands were not territorial units, that they were capable of large migrations, and that sexual relations must have existed between them (Vallois 1961:229)

while developmental pathways were readily inferred:

In evolutionary perspective a necessary locus and an indispensable condition for a cultural system is an organized system of social action in which social behaviour is patterned by role differentiation . . . A social structure, therefore, can be identified as one of the characteristic features of a protocultural stage in hominid evolution. (Hallowell 1961:240)

However, many of the contributions only seem to engage in speculation about social form and origins based on a minimal ethnography of hunters and gatherers and assertions about how to interpret the archaeological record.

The importance of *Social life of early man* rests on three factors. Firstly, it demonstrated the advantages of a multi-disciplinary approach to human origins. This pattern would be taken up and pursued in early hominid research. The mix of disciplines was heavily towards the natural and physical sciences and it was from their crucibles that the agendas of ancestor research were to be poured.

Secondly, the outstanding paper in the volume was by Washburn and DeVore, 'Social behavior of baboons and early man'. They dealt with troop size, range, diet, population structure, sub-groups and play, mother-child relations, sexual behaviour, economic dependence, dominance, home base, sounds and gestures. This thematic framework has been the standard for subsequent research. Throughout the paper they contrasted the life of baboons with modern foragers, producing a summary table which listed spatial, demographic, affective and behavioural differences for the topics mentioned above. They concluded that 'because of the great behavioural gap between man and his nearest relatives, some reconstruction of [social] behaviour is possible' (Washburn and DeVore 1961:103). This gap presumably indicates extinct transitional stages between the societies of apes and humans.

The third factor is only apparent with hindsight. Washburn and DeVore encouraged Richard Lee to undertake, between 1963 and 1964, his first study of the !Kung San foragers of Botswana (Lee 1979:9–15). This marked the introduction of anthropologists to the multi-disciplinary project of human origins (*ibid.*:9) and led directly to the important *Man the hunter* symposium (Lee and DeVore 1968). This and subsequent comparative volumes on foraging societies (Bicchieri 1972, Burch and Ellanna 1994, Damas 1969, Hunn and Williams 1982, Ingold, Riches and Woodburn 1991, Leacock and Lee 1982, Lee and DeVore 1976, Miracle, Fisher and Brown 1991, Smiley et al. 1980) have provided a rich source for defining the parameters of societies where nothing more was assumed than that they lived in small groups and moved about a lot (Lee and DeVore 1968:11).²

Characterizing hunters and gatherers

This description of the nomadic style became the basic model (Mueller-Wille and Dickson 1991:26) for subsequent research into foraging societies. In a comparative synthesis of the papers in *Man the hunter*, Lee and DeVore (*ibid.*:11–12) drew a distinction between elements in the organizational base and the social system (Table 1.1).

As might be expected, this basic model expanded on the views summarized by Washburn and DeVore (1961) in their comparison between foragers and baboons. The emphasis on open systems, alliances, mobility and lack of territory (Lee 1976), while not supported by all the papers in the symposium, did offer an alternative view to the patrilocal, land-holding model of bands promoted by Service (1966, 1971) in his classic statement on social evolution.

Table 1.1. *The nomadic-style model of hunters and gatherers (Lee and DeVore 1968: 11–12).*

Organizational base

- open social systems where local groups are commonly associated with a geographical range
- communication between groups takes the form of marriage alliances and visiting. This results in hunting groups consisting of several ‘bands’ which are part of a larger linguistic and breeding community
- the economic system is based on a core of common features
 - a home base
 - division of labour where the males hunt and the females gather
 - the collected resources are shared

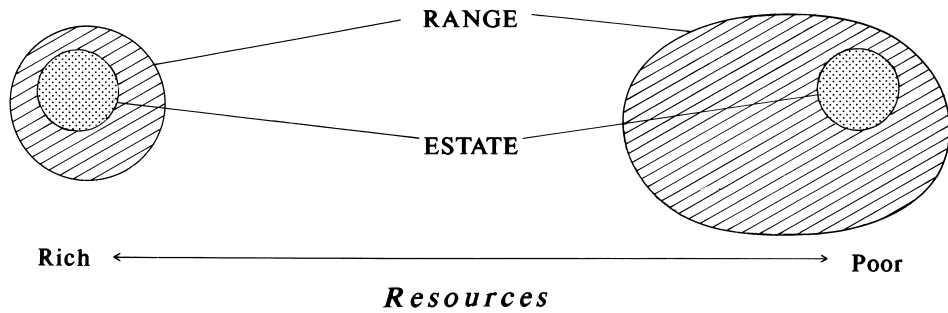
Social system

- the prime necessity of mobility restricts personal possessions and material culture. This acts to maintain the egalitarian structure
 - food supply keeps the group small, frequently fewer than fifty persons, and population is redistributed between bands, thereby maintaining the effectiveness of units
 - intergroup relations through marriage and other means of alliance are the means by which shortfalls in resources are counteracted
 - exclusive rights to resources are uncommon
 - the environment is the storehouse and food surpluses are rare. This restricts the social use of food resources
 - visiting prevents any strong attachment to one area. Conflict within the groups is most commonly resolved by fission
-
-

Lee and DeVore’s model echoed Lesser’s earlier call to adopt, as a working hypothesis,

the universality of human contact and influence – as a fundamental feature of the socio-historical process; [and] conceive of human societies – prehistoric, primitive or modern – not as closed systems, but as open systems . . . we [should] think of any social aggregate not as isolated, separated by some kind of wall, from others, but as inextricably involved with other aggregates, near and far, in weblike, netlike connections. (Lesser 1961:42)

Ethnographic examples can be found to support both poles of the argument. The reality, as Stanner (Peterson 1986, Stanner 1965) pointed out, is a continuum of differential social access to local groups and the resources they control. This variable association was enshrined in what he termed *estate* (religious core) and *range* (foraging area) relations (Gamble 1986a:33–4). The position of any group on the continuum was strongly influenced by variation in ecological conditions. The closer in size that a group’s estate and range become, the more likely it is to find a patrilocal, restricted access system. On the contrary, when resources are poorer, range size will far exceed the area covered by a group’s



1.1 The transformation under variable ecological conditions of hunter-gatherer spatial behaviour, as predicted by Stanner (1965).

estate. This generally results in unrestricted access, sharing and an open rather than closed system (Figure 1.1).

Peterson (1986) has shown how this social continuum works for several Australian foraging societies (Table 1.2). Ranged along an ecological gradient³ of good to poor resources it is possible to see systematic changes in the principles of social integration, association to land and marriage rules. The social principles of inclusion and exclusion which change along this gradient are examined in Chapter 3.

I mention this debate concerning the open or closed nature of foraging groups not only because it relates directly to my main concern, societies, but also because it illustrates a recurrent feature in the way hunters and gatherers are dichotomized (Table 1.3).

Several of these authors do discuss the continuum which Stanner explored so profitably. However, this gloss is often forgotten, and the extremes are used as definitions that structure archaeological analysis and characterize the pre-historic hunter-gatherer way of life (e.g. Johnson and Earle 1987, Keeley 1988).

A multi-disciplinary success story

The polar oppositions in Table 1.3 were introduced from the 1960s when both archaeologists and anthropologists discovered, unlike Childe, that they could investigate the social life of early hominids. The key to this discovery was to adopt a multi-disciplinary approach to the problem of social and biological origins.

This strategy is best exemplified by the success of the Leakeys at Olduvai Gorge, Tanzania, where a cornucopia of fossils and archaeological sites was now dated by scientific techniques (Leakey 1951, Leakey 1971). At the same time they encouraged investigations into chimpanzees, with startling results concerning tool use and group behaviour (Goodall 1986). Key elements in Washburn and DeVore's comparison between baboons and foragers, which had suggested a space for the investigation of social life in the Palaeolithic, could now be investigated for our closest primate relative.

Table 1.2. *Australian social organization along an ecological gradient (after Peterson 1986:Figs. 6:5 and 151–3).*

Resource gradient	Persons per km ²	Integration	Ties to land	Marriage	Social principle
Poor	1:200 (Pintupi)	Generational moieties	Conception	Monogamy	INCLUSIVE
Medium	1:86 (Walpiri, Pitjantjatjara)	Patrilineal generational moieties	Section/ sub-section managers		
Rich	1:20 (Yolngu) 1:4 (Kurnai)	None	Descent groups	Polygyny	EXCLUSIVE

Faced with such success, the emphasis, not surprisingly, for such multi-disciplinary research became heavily African (Clark 1976). This research was geared to the *origins* of social life⁴ as later expounded in the work of Reynolds (1966), Howell (1965), Isaac (1972, 1978), Leakey and Lewin (1977), Zihlman (1978), Wilson (1980), Tanner (1981), Lovejoy (1981) and Humphrey (1983), and more recently by Parker (1987) Wilson (1988), Foley (1989, 1989), Knight (1991a, 1991b), Maryanski and Turner (1992), Rodseth (et al. 1991), Potts (1993) and Quiatt and Reynolds (1993). Several of these models are reviewed by Richards (1987:206–33) and by Knight (1991a:Chapter 5). A valuable set of feminist critiques is supplied by Fedigan (1986), Conkey (1991) and Sperling (1991).⁵

What emerges from these accounts is that the *social*, in the context of research into human origins, was a way to link disparate, multi-disciplinary data together to form a coherent narrative. It was the paradigm which justified the enterprise, by supporting two different narratives: stories about change and adaptation.

In particular, Glynn Isaac sought to characterize, using the early Pleistocene-age archaeology of East Africa, the nature of these early societies and their developmental trajectories. Hence his discussion of home bases (1978), networks for cultural transmission (1972), and the superstructure of culture (1976) which was placed, in Hallowell's (1961) terms, upon a protocultural base comparable to great-ape organization (Figure 1.2).

The worm in the bud

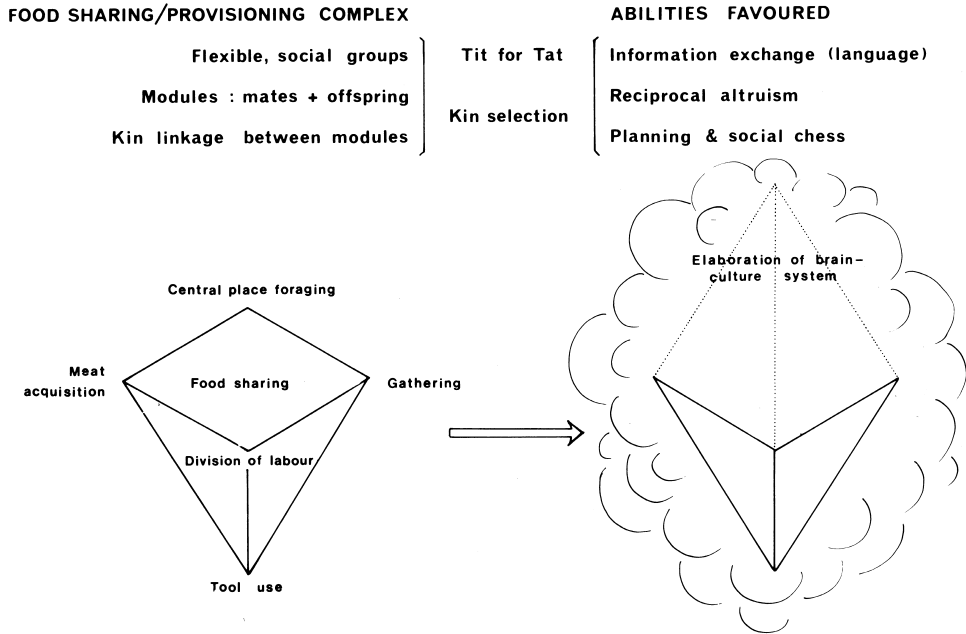
But all may not be so rosy in the garden of human-origins research. While archaeologists celebrate the achievements of multi-disciplinary research and the products of the scientific method applied to a problem which, only forty years ago, Childe pronounced impossible, we have to ask ourselves what we

Table 1.3. *Some of the dichotomies used in the characterization of hunters and gatherers, past and present.*

Local group	<i>Patrilocal</i>	<i>Composite bands</i>	(Service 1971, Steward 1936)
	<i>Unilocal</i>	<i>Bilateral</i>	(Gardner 1966, Layton 1986)
Social organization	<i>Territorial</i>	<i>Non-territorial</i>	(Lee 1976)
	<i>Tribal level</i>	<i>Band level</i>	(Constandse-Westermann and Newell 1991, Fox 1967)
Kinship system	<i>Complex</i> <i>e.g. Eskimo</i>	<i>Elementary</i> <i>e.g. Australian</i>	(Lévi-Strauss 1969) (Fox 1967)
Network	<i>Nucleate</i>	<i>Anucleate</i>	(Yellen and Harpending 1972)
Interaction pattern	<i>Closed</i>	<i>Open</i>	(Lee 1976)
	<i>exclusive</i> <i>inegalitarian</i>	<i>inclusive</i> <i>egalitarian</i>	(Gamble 1993d) (Woodburn 1982)
Settlement system	<i>Collector</i> <i>circulating</i>	<i>Forager</i> <i>radiating</i>	(Binford 1980) (Marks 1983)
Return system	<i>Delayed</i>	<i>Immediate</i>	(Woodburn 1980)
Systemic organization	<i>Complex</i>	<i>Simple</i>	(Keeley 1988)
Knowledge base	<i>Group opinion</i>	<i>Memorate</i> <i>personal experience</i>	(Gardner 1966)
Technological organization	<i>Curated</i>	<i>Expedient</i>	(Binford 1973, 1979)
	<i>Reliable</i>	<i>Maintainable</i>	(Bleed 1986)
Historical position	<i>'Hot'</i> <i>active</i>	<i>'Cold'</i> <i>passive</i>	(Bender 1985)
Social world	<i>Unencapsulated</i> <i>pristine</i>	<i>Encapsulated</i> <i>enculturated</i>	(Woodburn 1991) (Service 1971)
Evolutionary typology	<i>Local group:</i> <i>acephalous</i>	<i>Family level group:</i> <i>no domestication</i>	(Johnson and Earle 1987)

have actually learnt. Not in terms of new archaeological sites, fossil skulls or patterns, but instead in terms of those social origins which the whole multi-disciplinary enterprise was created to investigate.

Latour and Strum (1986) have examined seven accounts of human social origins ranging in age from Hobbes' *Leviathan* (1651) to the sociobiological account of Axelrod and Hamilton (1981). They included one archaeological account, Leakey and Lewin's *Origins* (1977), which differs from all the rest⁶ in adhering to a science-based chronology for our social origins and presenting many of these carefully garnered multi-disciplinary facts. However, when they compared *Origins* with Rousseau's *A discourse on the origin of inequality*



1.2 The 'food-sharing' or 'central-place foraging' model and its implications for social evolution among early hominids (after Isaac 1989:Fig. 4.14).

(1755), written long before any fossil hominids or Palaeolithic archaeology had been dug up, they found little difference in the structural framework and assumptions. From their brief survey they came to a challenging conclusion:

We had assumed that the knowledge and introduction of better 'facts' about the origin of socialness would produce better accounts. *Yet this seems not to be the case.* More particularly, there appears to be an inverse law at work in our examples: the more facts exist and are incorporated, the less attention is paid to the coherence of the framework within which those facts are placed. As a result, *the number of facts appears to be almost irrelevant*, since the most coherent accounts in our corpus were those, like Rousseau's, which laid aside all facts, and the least coherent were those with the most facts. (Latour and Strum 1986:185, my emphasis)

Challenging – at least to those who expect that the answer to human social origins will be revealed by joining up the dots. Challenging also to those, like Landau (1991), who regard it as sufficient to point to the repetitive nature of human-origins stories written by archaeologists, without judging their coherence against a wider guild of writers tackling the same subject.

I draw the following conclusions from this cautionary tale of social construction. Facts, as Haraway tells us, are meaningful within stories. We need to balance narrative coherence against the necessary selection of information. We do not need to include everything that has been discovered to produce coherent syntheses, but we do need to pay equal attention to the structure of our

narrative. The scientific method needs to recognize the mythic character of accounts of social origins since these are laden with values concerning our present status, roles and rights.

What Latour and Strum are basically reminding us is that we should be explicit about our starting assumptions. There is obviously always scope for tightening logical procedures and considering what exactly it is that makes a scientific account convincing in terms of verifying knowledge claims (Gardin 1980). I also take heart from their critique since although they emphasize the mythic quality of scientific research, they do not dismiss the idea of studying social origins. This is in contrast to the British anthropologist Evans-Pritchard who once declared that such studies were a waste of time; speculations about unanswerable questions (1965).⁷ Their insistence on social genealogy, where origins debates are opportunities to negotiate and renegotiate what our society is about, provides a necessary context for justifying the enterprise and one, once again, where science will not suffer by being combined with a noble social or political agenda.

Studies of Palaeolithic society: the origins of modern foraging societies

The most influential typology of social evolution in the past thirty years identified as its units bands, tribes, chiefs and states (Service 1971). Although most people claim to have outgrown this typology, I suspect that most of them still use it as a mental shorthand (Yoffee and Sherratt 1993). It allows a first approximation whereby an ethnographic or archaeological example can be slotted into an organizational scheme which, by its generality, is expected to tell us more about the subject under scrutiny.

The problem is that while in the broadest sense the divisions have some reality – bands and states *are* different social realities – there is little agreement about the criteria which would classify the conceptually much murkier transitional societies to a particular rung of the typological ladder. Moreover, the inevitable progression implied in the typology, what Johnson and Earle refer to approvingly as an upward spiral (1987:15), is nothing more than a story about the benefits of civilization. While it is a comforting myth about our western social origins, such evolutionism tells us little beyond our belief in progress.

Open and closed societies: which came first?

These problems are well shown with the first category, bands. It took many years for Europeans and North Americans to acknowledge that hunters and gatherers had a social life and for its systematic investigation to begin (Kuper 1988). Once investigation had started, the problem then became one of how to classify such a heterogeneous sample, scattered across the world, exploiting very different resources, and exhibiting a wide range of customs.

Steward (1936, 1955) and Service (1966, 1971) brought some order to this

problem by recognizing that many of these bands were the product of contact with the modern world. These were the *composite* bands such as the northern Algonkians and Athabaskans (Service 1971:48–52), whose societies had been destroyed by western contact leaving only a fluid-membership, family-level type of organization (*ibid.*:97). These were seen as remnants of former *patrilocal* bands, which Service defined by exogamy outside the local group and virilocal residence, where on marriage women move to the husband's household or campsite (1971:64). In Lee's phrase, 'the dominant impression one gets from accounts of patrilocal bands is one of semi-isolated, male-centered groups, encapsulated within territories' (Lee 1976:75). These, according to Service, would have been the widespread form of social organization during the Palaeolithic. Ethnographic examples of patrilocal bands included all Australian societies and the central desert Californians.

But this still left a great many bands outside the typological net. For example, the Eskimos and Great Basin Shoshone were regarded as anomalous (Service 1971:83–97) because although affected by European contact they still retained some patrilocal band features. The Bushmen of the Kalahari were, even in the post-*Man the hunter* edition of his work (Service 1971:50), regarded as difficult to classify but certainly not patrilocal in form. Finally, the North West Coast foraging-based societies were on many counts outside the classification of band societies and they were placed in the chiefdom or ranked societies category.

Lee, on the other hand, challenged the widespread nature of patrilocal bands, regarding them as ethnographically rare (1976:76). His model, which traces its origins to Mauss (1906), emphasizes the fission and fusion of groups in which membership is organized bilaterally rather than just through the father's line. The !Kung became the prime example of the 'relatively open, social group with overlapping shared territories [which] seems to be the prevailing form among contemporary hunter-gatherers' (Lee 1976:76). All this suggested a rethink of the status of the composite band. Maybe their form was not entirely due to their near destruction through contact with industrial civilization (Lee 1976:77)?

As we have seen, investigations of our earliest social origins in Africa are mainly interested in the acquisition of the elements of sociality (e.g. technology, territoriality, co-operation, hunting) rather than in identifying social types such as bands. The strategy, as shown by Isaac's influential food-sharing model (1976:Figure 6, 1978), was to add a bit to the primate model of organization and take a bit away from modern hunters (Figure 1.2). The dominant view was that these earliest societies were essentially open, with fluid membership, and were non-territorial. They thus approximate to the descriptions of San groups (Barnard 1992, Lee 1976, Silberbauer 1981). The fluid social structure and immediate-return systems of these modern societies have been put down to their encapsulation by herding and agricultural societies (Woodburn 1991).⁸ Hence the suggestion has resurfaced that these societies are in many ways comparable to Service's (1971) assessment of composite bands, as the product of

Table 1.4. *A division of contemporary hunting and gathering societies (Woodburn 1991:35).*

Encapsulated Immediate return systems	Unencapsulated Delayed return systems
Africa	North America
!Kung	Inuit societies
Mbuti	North West Coast societies
Hadza	Australia
India	Aborigine societies
Malapantaram	
Naiken	
Paliyan	
Malaysia	
Batek Negritos	

enculturation rather than being indicative of earlier widespread social forms (Table 1.4).

An important aspect of the Kalahari research begun by Lee and his colleagues has been to oppose this assessment. They have argued that open network systems should also be regarded as original (Yellen and Harpending 1972:244). The advantage of such systems lay in adjusting population to variation in regional food supplies, using group fission to resolve conflicts by literally 'walking away from the problem' and evening out sex ratios and family sizes between local groups within the bands (Layton 1986, Lee 1979). However, the so-called revisionist critique of hunter-gatherer studies (Schrire 1984, Wilmsen 1989) now strongly supports the notion that the form of these 'open' societies can be traced to the wider economic and social forces that have shaped the recent political history of southern Africa. Individuals, it has been discovered, move between hunting and herding economies with comparative ease and with scant attention to the social niceties of typology.

As a result the pendulum has swung back to consider that those societies, largely unencapsulated by farmers or industrial nations, are in some way more representative of a generalized pre-agricultural social system. In a world consisting exclusively of hunters and gatherers, a higher proportion may have had delayed-return systems (Woodburn 1991:61). Such societies are mostly from the Arctic, the North West Coast of North America and Australia (Table 1.4). Woodburn (1980, 1982) has argued that these societies are characterized by delayed systems of return involving rights over valued assets. These assets take the following forms:

- valuable technical facilities used in production (nets, boats, fish traps)
- processed and stored food usually kept in or near houses

- wild products whose yields have been increased through intensification
- female kin to be bestowed by men in marriage (Woodburn 1991:32)

Australia, the continent of hunters and gatherers, is rather troublesome to Woodburn's suggestions concerning the original form of foraging society. Storage is rare and encapsulation by farmers did not take place until the arrival of Captain Cook. Yet, patrilocally based foraging bands did not exist in the continent (Hiatt 1962). To get round this problem Woodburn proposes that Australian marriage systems were a form of delayed return with the older, initiated men controlling the distribution of women between local groups in a connubium, or marriage universe. 'Australians [are] farmers in disguise who are concerned with farming (and farming out) their women' (Woodburn 1980:108–9).

But by insisting on the system of return, delayed or immediate, as the means to differentiate and characterize foraging societies, Woodburn ignores the importance of space and property as an asset. This is a serious omission since it is the transactions in property, as Layton (1986:30) shows, which shape the long-term processes of hunter-gatherer society and so determine its political structure. In this case property is owned by small descent groups, or clans, numbering from fifteen to fifty individuals (*ibid.*:22). Elsewhere they are referred to as the minimum band (Damas 1972) or local group (Birdsell 1968).

In Australia this property consists of sacred sites over which each clan has exclusive rights. These sites form Stanner's concept of the estate (1965). It is the association of people with the land and their active stewardship of the meanings connected with places that characterizes Australian foraging societies. Such exclusive rights to geographical sites and sacred knowledge are contrasted by Layton (1986:30) with other hunter-gatherers who exercise exclusive rights over territories and the food resources they contain. These rights are also vested in lineages and so form the basis for inegalitarian social structures.

A rather different view is provided by Meillassoux (1973). He argues that the mode of exploitation of the land sets up relations of production that resolve themselves through the physical and social reproduction of society. The relations of production between members of a hunting group do not in his view require continued membership of the group and so do not lead to long-lasting social cohesion. Moreover, where land is the subject rather than the instrument of labour, as is the case with farmers, this results in the rapid depletion of resources and hence the enforced movement of the group from one area to another in search of new food supplies. This makes it difficult, if not impossible, for him to see how hunters develop into farmers without them receiving some extra advantage from the environment.

Property, the spatial use of resources, provides the focus for social action through involvement based on rights and obligations. If we just concentrated on the system of return, the use of resources in time, as Woodburn advocates, we might be tempted into some strange evolutionary arguments. For example,

it might be argued that the !Kung would revert to a delayed system, if only they were allowed to by the larger economic and political forces which surround them. Such a view would deny them any active participation in their own history and only requires that they seek some equilibrium in organization. Hunters and gatherers are returned to that passive state where they are acted upon by external forces such as climate or civilization. The members of such societies are barely conceived as active social agents but rather as slaves to nature. The societies they inhabit are 'cold' in terms of the possibilities of self-generated change, relying instead on a push from outside. In this scenario the 'hot' societies emerge as those with agriculture and civilization. They are peopled by 'warm-blooded' participants in the creation of history (Bender 1985:21).

But putting such caricatures aside, this still leaves the problem of how and why such dramatic social change took place. This is clearly Woodburn's concern. He has concluded that

delayed-return hunter-gatherer systems are, in a sense, pre-adapted for the development of agriculture and pastoralism. They have the organization (the binding ties and the social groups) which should make the development of an economy based on agriculture or pastoralism easy when the techniques become available. (Woodburn 1991:57)

Therefore it was among the now-vanished, unencapsulated, delayed-system societies that such change occurred. In other words their success made them disappear.

Meillassoux also got out of this same problem by pointing to the North West Coast societies, with their reliance on fishing, as the likely seed-bed from which agricultural systems with their rights to tenure and property sprang (1973:201). But like Woodburn, he does not explain why, in these windfall environments, the foraging way of life is still dominant.

Simple causes

These studies point to the difficulties, if not impossibility, of establishing a prototype from ethnography for immediately pre-agricultural societies and then deducing an evolutionary trajectory. This, however, has not stopped Johnson and Earle (1987:18) from presenting the !Kung and the Inuit as examples of two extremes in hunter-gatherer organization representing, respectively, family level and local group organization. Prehistoric foraging societies from Olduvai Gorge to the Middle Palaeolithic of the Dordogne are then fitted to the demographic parameters of the !Kung and the Great Basin Shoshone. Everything that happens in the Upper Palaeolithic was 'impelled by a continued growth in population' (*ibid.*:55). In fact they claim that population growth is the primary determinant of all social change (*ibid.*:3). The intensification precipitated by such growth then produced local group-level societies, such as the coastal Inuit or the interior Nunamiut. Therefore we are left with the view that the open, composite systems are somehow ancestral and that foragers wait pas-