On Recent Trends in the Anthropology of Foragers

Kalahari Revisionism and Its Archaeological Implications

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1992

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Archaeology studies change in forager societies over time, yet relies on ethnology for much of its understanding of how those societies are organized. The prevailing model of forager societies in ethnology emphasizes their affluence, flexibility and equality, but these properties are now questioned; moreover doubts have also been raised about the archaeological uses of ethnographic data. The resulting debates have implications for the validity of certain ethnological inferences and for our understanding of the nature of ethnographically documented forager societies. Significantly, the issues can only be settled with the aid of archaeological data.

Introduction

Primitive society has been a central concern of anthropology throughout the discipline's history. Its characterization, however, has varied to fit changing doctrinal fashions in the field (Kuper 1988). Foragers[1] are a natural candidate for exemplars of primitive society (Bettinger 1991: 2), and by now it has almost become a cliche to observe that most of our history has been spent in that condition. To some, foragers represent the 'basic human adaptation' (Leacock & Lee 1982: 5), and their study is held to be 'vital to our understanding of ourselves as human beings' (Leacock & Lee 1982: 1).

Western views of foragers as exemplars of primitive society have long vacillated between the poles of abject misery and noble splendour, between the contending images of Hobbes and Rousseau. Even comparatively recent history illustrates the way in which these particular visions of primitive society have continued to inform our models of what such a society might be like. Evolutionary stage models of the mid-twentieth century, at least in North American anthropology, viewed forager societies as consisting of sets of patrilineal clans, in which hunting and male solidarity were pre-eminent (Radcliffe-Brown 1930; Service 1962). Since the mid-1960s a different vision has pre-

[1] For several reasons, 'forager' is used in preference to 'hunter-gatherer'. First, 'forager' is a more economical expression. This alone is insufficient justification, but it is no mean consideration for a frequently used term. Secondly, 'hunter-gatherer' as traditionally used carries an obvious gender bias (Dahlberg 1981; Lee 1979). The gathering of plant foods, as anthropologists are fond of noting, often makes greater contributions to the diet in such societies than does hunting. The gradual recognition of this fact has given rise to the alternative term formed by reversing 'hunter' and 'gatherer'. Following such a policy of dietary pre-eminence, some groups must be called 'hunter-gatherers' and others 'gatherer-hunters'. Confusion only increases when there exists rough parity between the two modes of food-getting, which can lead to needless agonizing over the appropriate label to apply. Clearly 'hunter-gatherer' is a contentious term. As Bishop remarks, 'the term "gatherer-hunters" is no more appropriate than "hunter-gatherers", ... The term "forager" is preferable to either' (1985: 65). Finally, it is as 'foragers' rather than 'hunter-gatherers' that the people have come to be known, both in anthropology generally and in the most recent controversy over the status of their societies (e.g., Bird-David 1988; Kent 1992; Leacock and Lee 1982; Solway and Lee 1990). With Lee (1992: 32), this article addresses — but does not retain — the traditional view of forager societies: a foraging mode of subsistence in an egalitarian collective context.

vailed, of forager societies as loosely structured egalitarian collectives characterized by gender equality and a more balanced economy of both hunting and gathering.

Their apparently privileged historical position has made foragers an especially apt subject for archaeology, the branch of anthropology devoted to the study of long-term cultural processes. How were forager societies constituted in the past, and how and why did they change over time? What are the systematic properties of forager societies and how are they expressed across the vast spans of space and time in which these societies are found? Such questions are rightly the domain of archaeology, though satisfactory understandings continue to elude us. Gaining them requires a general theory of forager societies and their transformations, a theory certainly influenced by, but not over-dependent upon, ethnographic data.

Yet anthropological theorising about forager societies has largely remained within the province of ethnology, and has been little influenced by archaeological thought. This is ironic, since today 'archaeological interest in foragers exceeds by a wide margin interest by social and cultural anthropologists' (Lee 1992: 33). From its inception, anthropology has identified forager society as a particular form of primitive society. Early models invested it with intrinsic but latent qualities of self-transformation that required considerable time to emerge (Kuper 1988). More recent models use history to account for ethnographically documented transformations: in this way, Service (1962) explained composite bands as the products of contact. Birdsell (1970) applied a similar argument to Aboriginal Australia with respect to the effects of European occupation on territorial organization and the composition of the co-resident group: 'Where anthropologists have failed to recognize the impact of dislocation and depopulation, their conclusions about local group composition and landownership as a primary spacing mechanism are blurry and overly flexible' (1970: 131). Today's prevailing model, broadly marked by the appearance of *Man the hunter* (Lee and DeVore 1968), adopts a somewhat static or equilibrium view of forager societies that nevertheless attributes to them at least a modest historical dimension.

Archaeology is the subdiscipline that studies cultural change over long periods, yet an archaeological perspective was conspicuously absent from these models. Archaeologists may regard the subject of foragers as their 'common ground' (Bettinger 1991: vi) with ethnology, but ethnologists apparently do not share this view. Archaeologists tend to countenance this arrangement by applying ethnological models to the material record. Although not necessarily entailed, this practice encourages archaeologists to reproduce ethnological knowledge in that record (Wobst 1978). This state of affairs poses an obvious dilemma, since the models applied by archaeologists are only as good as the quality of the ethnographic data on which they are based.

Archaeological study of forager societies will always be informed by ethnographic data; only the nature and degree of reliance on them is in question. My point of departure in this article is archaeology's heavy reliance on ethnographic data, exempli-

fied in particular by the renowned case of Kalahari[2] foragers in southern Africa. The dangers of such reliance are briefly surveyed, albeit covering ground familiar to most archaeologists. Some limitations of the ethnographic data, however, may not be well known or widely appreciated. I go on to discuss an ongoing debate in forager studies, as best exemplified in Kalahari ethnography. My discussion, however, requires a somewhat broader geographical coverage; accordingly, ethnographic controversies in other regions are briefly described. My purpose is emphatically not to take sides in the debate but, in the final section, to identify its significant implications for the archaeological study of foragers.

Foragers in ethnography and archaeology

The prevailing general model of forager societies crystallized in the mid-1960s, dislodging an earlier, more rigidly structured view identified with Radcliffe-Brown (1930), Service (1962) and others. That new and now prevailing model was based to a significant degree on ethnographic work conducted among Kalahari foragers, although other cases contributed to the synthesis. Now widely recognized in anthropology, the model is based on the premisses that foragers enjoy a high standard of living with relatively litde effort and that a flexible egalitarian[3] collective closely adapted to ecological constraints is the cornerstone of their social formations. Indeed its enthusiastic reception has earned this model the status of a prevailing orthodoxy (Bird-David 1988: 17).

[2] The proper designation for the indigenous peoples of the Kalahari is clearly a vexing issue. Lee favours 'San' — 'a comparatively neutral term' (1979: 30) — as a general label and '!Kung' as a label for 'a clearly bounded ethnic and linguistic grouping' (1979: 31), in effect a subset of San. He acknowledges, however, that even 'San' is unsatisfactory because it is vaguely pejorative and is not an autonym. Apparently there exists no autonym above the level of local groups (Hitchcock 1987: 245), a limitation that forces anthropologists sometimes to apply unsatisfactory appellations. Wilmsen (1989a: 31) justifies at some length his rejection of all general terms. He argues, in fact, that the familiar panoply — San, Bushman, Sonqua or Soqua, Masarwa or Basarwa — 'all should be relegated to archives' (1989a: 32) in favour of autonyms. Lee and Wilmsen agree that their subjects' autonym is rendered as something like 'Zhu/twasi' or 'Zhu'. An outsider to southern African studies is well advised to tread lightly, and the question of naming is avoided here in favour of more substantial issues. Thus, although Wilmsen and Denbow (1990: 490) call it an 'ethnographic reification', 'Kalahari foragers' is applied here as a deliberately general term. In his own work, Wilmsen (1989a: xii) justifies a similar approach by drawing attention to the foragers' highly 'ethnogracized' status within the larger social formation that he discusses.

[3] Lee (1990, 1992) has recently argued that a degree of inequality along lines of age and sex is to be found in all societies and that none is purely egalitarian. This is neither unreasonable nor disputed; status indeed varies by degree. Lee criticizes others, however, for insisting on 'an impossibly high, absolute definition of egalitarianism' (1990: 236) as the only way in which inequality may be established for Kalahari and other forager societies. Yet the sorts of inequalities in status and access to resources that Wilmsen (1989: 197–271) suggests for the Kalahari far exceed what could be considered ordinary for groups characterized by the modest age and sex inequalities that Lee describes. Consequently, it is difficult to follow Lee's argument that opponents confuse the absence of 'absolute' egalitarianism with class structure.

The model has a special appeal for archaeologists, perhaps more than its creators intended. Although it does not contain an intrinsic historical dimension and does not necessarily entail the inference that ethnographic forager cultures are simple Palaeolithic relics (cf. Murdock 1959: 61), it is not unfair to suggest that the model's chief advocates have encouraged archaeologists in this view. Wilmsen (1983; 1989d: 8–10) cites Tanaka, Marshall and Silberbauer in this vein, and Bettinger (1991: 48) discusses the implications of Kalahari ethnography for the evolutionary study of foragers. In fact, 'an evolutionary definition' of foragers 'would have been ideal' to Lee and DeVore (1968: 4), a statement which can be reasonably construed as an invitation to regard ethnographic foragers as Palaeolithic exemplars, although Lee and DeVore emphasize at the same time the problematic nature of positing such equivalence between past and present foragers. Lee decided to study foragers in Africa, and not elsewhere, because he thought he could find there the 'actual ... environment occupied by early man' (1976: 10), implying at least a broad identification of the forager model with Palaeolithic cultures. Moreover, Lee's ultimate goal in his Kalahari work was the use of ethnographic data to 'illuminate human evolution' (1979: 2). The 'basic human adaptation' (1979: 2) which he claimed to discover can be construed as a Palaeolithic one. In all cases, Lee was careful to stress the ways in which his Kalahari subjects differ from their own and others' ancestors; where he wrote of 'illuminating' human evolution, for instance, he added that Kalahari foragers articulate with a world of agropastoralists and state societies and that 'effects of contact' (1979: 2) must be taken into account. They must be taken into account, though, precisely in order to reveal those properties that may have characterized foragers in the past. Finally, Leacock and Lee, while properly careful not to view ethnographic foragers as 'living fossils', still consider them in a timeless sense (and hence one that embraces both modem and Palaeolithic eras), to 'represent the original condition of humankind' (1982: 5).

Nevertheless, Lee and other advocates of the prevailing model nowhere demonstrated the intrinsic link between present and past foragers. It remained for archaeologists to accept the invitation and to incorporate the model into their own views of the past. Thus 'the decade following the *Man the hunter* symposium was one in which anthropologists came to see the world's foragers largely through Bushman eyes' (Speth 1991: ix), and even today, 'when anthropologists write about hunter-gatherers, more often than not they write about the IKung' (Hawkes 1987: 342). Among the many ironies of current debates on the subject of foragers is that Lee and other ethnographers are assailed for providing the data that archaeologists eagerly sought. For archaeologists, at least, the argument should be less with ethnologists than among themselves.

Most archaeologists recognize the perils of simple analogy, but the prevailing ethnographic model proved extremely popular as a framework for archaeological synthesis and interpretation. No-one seriously proposes its facile extension to ancient foragers, but its general properties, as described by Lee (1968; 1969; 1976; 1979; 1984; 1988; 1992) and others from Kalahari experience (Silberbauer 1981; Tanaka 1980), have been widely accepted. Wilmsen and Denbow (1990: 503; see also Isaac 1990d) can legit-

imately argue that such acceptance gives Kalahari groups a 'unique exemplary status' in forager studies. The prevailing model has proved indispensable — not to mention remarkably versatile — to archaeologists, who, as Root (1984: 29) notes, have applied it to everything from East African fossil hominids to late prehistoric groups in the northeastern United States. To others, it has exerted not merely a strong but a 'tyrannical' (Isaac 1990d: 323) effect upon archaeologists. Empirical dimensions of Kalahari ethnography also influence current archaeological practice and models. Yellen's (1977) data and his model of the spatial organization of forager camps, for instance, have been widely cited and applied in archaeological analyses, including my own (Shott 1989d; 19896).

The empirical basis of forager ethnography

With respect to forager societies, received ethnographic knowledge is skewed to certain latitudes and types of habitats (Foley 1982; Freeman 1968; Price & Brown 1985). Few if any foragers in midlatitude, temperate forest habitats, for instance, have been ethnographically documented. Many are found in what archaeologists are fond of calling 'marginal' habitats, certainly marginal by traditional Western standards. But even in these regions, ethnographically documented groups may not be representative of prehistoric cultures in similar habitats (Binford 1990). These limitations not only affect the archaeological study of mid- and high-latitude foragers, but also reduce the total range of cultural and adaptive variation that can be gauged from cross-cultural study.

In attempting to synthesize available ethnographic data, therefore, we may be inclined to reify gaps in our knowledge and to reach unwarranted conclusions from negative evidence. For instance, Lee's (1968) celebrated cross-cultural analysis has been questioned in its conclusion concerning the dominance of plant foods in the diet (Altman 1984; Ember 1978). Many ethnographic groups in Lee's study occupy habitats that are comparatively poor in game; richer habitats tend to be occupied by pastoralists and agriculturalists (Foley 1982; Price & Brown 1985). Moreover, the subsistence autonomy of many tropical-forest foragers, their ability to persist without inputs from agricultural neighbours, has been repeatedly questioned (Bailey 1991; Bailey *et al.* 1989; Hart & Hart 1986; Headland & Reid 1989), yet such groups are among the best-documented in the ethnographic record. Perhaps inevitably, given its broad scope and influence, Lee's (1968) study has generated specific criticisms as well. Ambrose (1986: 13), for instance, questions Lee's dietary figures for the Dorobo. Any survey of Lee's (1968) scope, however, might be challenged in some particulars, and these criticisms by no means reduce the value of his study, which is amply demonstrated by the enormous body of work it has inspired.

The general adequacy of forager diets and the low work effort required to obtain them are also important components of the prevailing model. Sahlins's (1972) well-known work relies heavily on Australian data (McCarthy & McArthur 1960) to con-

clude that foragers secure an ample and balanced diet with relatively little effort. Altman (1984: 185; 1987: 89–95), however, raises serious questions about the validity and reliability of the data that Sahlins used. He notes that McCarthy and McArthur's (1960) study group included no dependent children, who ordinarily must be provisioned by others, and argues that the study's brief duration — 13 days at Fish Creek and 7 to 14 days with varying levels of observation at Hempie Bay (McCarthy & McArthur 1960: 150, 180) — is insufficiently representative of required subsistence effort over a complete annual round. Altman (1984: 185) also calls the study 'artificial' because its participants were asked to refrain from consuming available market foods. Indeed on at least one occasion, a foray to obtain such foods was 'intercepted' (McCarthy & McArthur 1960: 147), although this could be seen as a perfectly valid control measure rather than a design flaw. To Sahlins's (1972) claim of original affluence based on these data, Altman (1984: 185) responds that 'in Arnhem Land at any rate, affluence is more a modem than an original phenomenon'. (Bird-David [1992: 25–26] and Birdsell [1970: 118] also raise questions about the quality of the data that Sahlins used to advance his thesis).

More specifically, the adequacy of the Kalahari forager diet, a key element of the prevailing model (Lee 1979; Sahlins 1972), has been questioned on energetic and, to a lesser extent, on nutritional grounds (Howell 1986; Isaac 1990d). And Lee's (1969) justly famous econometric analysis of Kalahari subsistence and foodgetting effort has drawn criticism from several quarters (Bollig 1988: 117; Hawkes & O'Connell 1981; Isaac 1990d: 330; Wilmsen 1989d). Critics point out that the mere killing of animals and gathering of plants is an insufficient description of subsistence effort, since neither animal nor vegetable resources can be obtained without some sorts of tools and neither become food until they are processed. Food processing and tool maintenance should therefore be included in calculations of subsistence work effort. Silberbauer (1981: 242–3), who adopts this principle in his own calculations, comes out with results that substantially exceed Lee's original estimates. For men in his study, in fact, direct food-getting activities take less time than food processing and tool maintenance. To his credit, Lee's own recalculations (1984: 51–53) indicate that the Kalahari work-week slightly exceeds 40 hours.[4] Other studies of forager work effort, however, suggest that even this figure may be unusually low (Eder 1987: table 6; Altman 1987). Furthermore, Wilmsen made work-effort calculations at approximately the same time of year as Lee, obtaining similar figures. But his estimates for other seasons are considerably higher, yielding an annual mean substantially above Lee's original value (Wilmsen 1989^: 235–7).

Besides time spent on food processing and tool maintenance, subsistence effort should also include the time that foragers may invest in negotiations with neighbouring groups for access to their resources, effort that is as considerable as it is vital in the

[4] Even more recently, Lee (1992: 38) has been admirably forthcoming in the admission not of errors but of revisions in his thought.

Kalahari (Bollig 1988: 117; Cashdan 1983: 50). Ritual activities and the substantial energy sometimes invested in them are also considered as work in some forager groups. Certainly Gunwinggu foragers of Arnhem Land classify their significant ritual labour as work that is just as necessary to subsistence as is actual food-getting activity (Altman 1987: 217–19). Data on work of this kind are difficult to collect, however (Altman 1987: tables 46–8), and, in fairness to Lee, their importance was not clearly appreciated until relatively recently. In sum, Lee's (1969) original study, despite its value and deserved influence, may not faithfully reflect conditions in the Kalahari, nor can its conclusions be projected elsewhere, an intention that Lee would probably disavow in any case.

Other 'traps' (Moore 1981) of empirical generalization exist. For example, the incidence of bilateral descent may be overstated, and that of intergroup conflict understated, in the ethnographic record. Certainly, the incidence of violent disputes in a range of forager societies is higher than is often supposed (Ember 1978). In Kalahari societies, the rate of *internal* dispute resembles that in our own (Konner & Shostak 1986: 73), even if conflicts with encompassing Bantu agropastoralists are ignored (Bollig 1988: 119). Similarly, the fabled 'magic number' of 25 for local-group size, once accepted with little question (e.g., Lee & DeVore 1968: 11; Wobst 1974), does not withstand critical scrutiny. From a cross-cultural analysis of data on local-group size, Moore (1981: 47–74) found that they fit a variety of statistical models with the *exception* of the uniform-size model on which magic-number assumptions rest. He (1981: 79) attributes this result to the poor statistical properties of available empirical material — comparatively few data whose distributions cannot be specified — and to the contingent influence of many factors on local-group size. One of these, quite understandably given the circumstances under which many foragers Eve today, is group defence (Bollig 1988: 110; Endicott 1984). An interesting sidelight of this study is provided by the only cases, the Birhor and Kutchin, that fit the uniform-size model. Of all the peoples surveyed, these groups are the most closely articulated with international market relations, and Moore (1981: 47; see also Fox [1969: 142]) suggests that this very condition may explain their group size by the constraints it places on their behaviour.

History and theory in forager ethnography

Questionable empirical generalizations, however, are not the only problems that the uses of ethnographic data pose for archaeologists. As suggested by Moore's (1981) study, the very nature of Western contact may have transformed indigenous societies before ethnographers could study them in detail. Wolf (1982) is generally credited with applying the worldsystem thesis to ethnography, but others were forming similar views concurrently (Leacock 1982; Schrire 1980; 1984) and even earlier (Fox 1969; Gardner 1989). More recently, Woodburn (1988) has proposed that 'encapsulation' — which, he stresses, is neither tantamount to simple contact nor exclusively the product of involvement with Western agents — transformed not only individual forager societies but also the aggregate character of their record, either by coercing foragers to

adopt more intensive economic practices and more complex organization, or by ensuring their persistence only through a shift from 'delayed-return' to 'immediate-return' organization. Leaving aside questions about the validity of Woodburn's model (Pedersen & Waehle 1988), his reasoning suggests that the ethnographic universe changed dramatically with the advent of a world system.

Hitchcock has shown that in eastern Botswana, dependency was imposed by agropastoralists in order that they might 'benefit from the goods and services of the subordinate population' (Hitchcock 1987: 244). Two decades previously, even as the prevailing model was being formulated, Fox (1969) had argued that certain South Asian forager societies functioned essentially as commodity specialists in broader social and economic systems, an argument that foreshadowed much of the recent debate to be discussed below. Headland (1986: 404) echoes and expands upon this view, on the basis of his survey of a number of forager societies: 'Such foragers as we find today are being kept in their marginal niches by their more powerful agricultural neighbours, for whom they supply indispensable raw materials' (see also Headland & Reid 1989). By implication, the organization of some forager societies may be in part a product of articulation, or of what Woodburn (1982) calls encapsulation, rather than a faithful reflection of aboriginal conditions.[5]

Unique historical conditions may have changed not only forager subsistence practices and social organization but the nature and scale of their regional interactions as well. Wobst (1978) believes that such regional processes often collapsed as foragers became involved with Western and other more complex societies, whilst local-level subsistence behaviour may have remained unaffected. Those dimensions of regional process that did not collapse, moreover, may have been glossed over by ethnologists

[5] Bird-David (1988) has issued a stimulating challenge to the question of the effects of external contact upon foragers. By implication, she questions not only the revisionist critique but the entire controversy it has spawned. In brief, Bird-David argues that contact with other societies is not merely a condition but an integral component, virtually a requirement (1988: 17), of forager societies. To her, it is not a question of whether or when foragers lost their isolation, but of how they manage their contact with non-foragers (1988: 20). It is no criticism of this view to note that its relevance is limited, from an archaeological perspective, to the time since non-forager societies have existed. Contact, however, is a multifaceted phenomenon whose influence on forager societies varies widely in kind and degree. Bird-David's model embraces only one meaning of the term and is not necessarily relevant elsewhere; it is difficult, for instance, to construe the virtual feudalism under which some African and South Indian foragers are reported to five as a product of the way in which they design or manage their contact with outsiders. If such accounts, to be summarized below, are accurate — a matter yet to be settled — then shrewd, negotiated autonomy is a poor understanding of the terms of articulation. Furthermore, Bird-David argues that foragers need outsiders — therefore 'contact' or some kind of articulation — precisely because of their apparently conflicting values of self-sufficiency and egalitarianism. By her reasoning, egalitarianism *requires* individual autonomy: 'Social cohesion based on relations of equality and likeness calls for a minimization of economic interdependence' (1988: 26). When assistance is needed, therefore, foragers must secure it from outsiders. However, that egalitarianism militates *against* co-operation — a logical foundation of Bird-David's model — is a questionable proposition that is contrary to the orthodox view, in which egalitarianism and extensive co-operation are linked. Bird-David's most recent account (1992) places great emphasis on sharing *within* forager groups.

in their desire to separate out the presumed disruptive effects of contact from the 'true' ethnographic baseline (Isaac 19906: 11). This tendency reflects, in part, a legitimate desire to filter out extraneous and potentially misleading evidence deriving from circumstances of contact, but it may also reflect the inherent nature of ethnographic practice. Ethnologists, like foragers, can be territorial, and the staking out of mutually exclusive research territories along with the consequent tendency to emphasize local behaviour (Isaac 19906; Wobst 1978: 304) may lead to systematic neglect of the wider regional interactions of forager populations.

The revisionist critique

Ethnographic data must be read with some care, a statement of no great surprise to ethnologists but one that archaeologists are now increasingly coming to appreciate. Ethnological interpretations, just as much as archaeological ones (Wilk 1985), shift with changes in contemporary social values. Isaac (1990a: 324–5) sees Western agendas of gender and class relations transcribed to the ethnographic record, and Howell (1986: 10, cited in Wilmsen 1989a: 37) recognizes a 'message of liberation, peace and social justice' in traditional Kalahari ethnography. The extent to which Kalahari foragers fit these images is now in doubt (Wilmsen 1983: 1989a; Wilmsen & Denbow 1990), although even this recent questioning is situated in its own postmodern context (Lee 1992). The postmodern challenge in ethnography is a matter for ethnologists to settle, and this section is concerned entirely with revisionism's[6] implications for the prevailing model in forager studies, not for ethnography in general. If that prevailing model demands revision, much of the interpretive framework of forager archaeology must also be reconsidered.

There is little doubt that the series of expeditions to the Kalahari — from the Marshalls' travels (Marshall 1976) through Lee's extensive fieldwork (Lee 1979) to later, more specialized studies[7] — was motivated at least in part by a desire to identify and study relatively isolated foragers. As the participants suggested, isolation might allow modem foragers to be regarded as exemplars of Palaeolithic conditions as well as valid ethnographic subjects in their own right. While the Marshalls were engaged in their fieldwork, however, Gusinde (1957; cited in Gordon 1986: 360) could write that 'the culture and racial type of [Bushmen] ... can, on the whole, be regarded

[6] Wilmsen dismisses the 'revisionist' label as 'polemic' (Wilmsen & Denbow 1990: 493fh), but the term is already established in the literature in a sense consistent with archaeological usage (e.g., Kehoe 1981).

[7] Although the Marshalls' expeditions of the 1950s and the Harvard Kalahari Expedition launched in 1963 were distinct, there are strong reasons to associate them. Both were supported by, and to some degree identified with Harvard University, and their chief research sites are separated by less than 100 km. More importantly both were impelled, for reasons discussed in the text, by a desire to identify and study remote foragers. Finally, both were marked by a far more sympathetic appraisal of those forager cultures than was typical of most earlier ethnography (Gordon 1986).

today as sufficiently investigated'. Gordon himself, viewing the current debate in the light of earlier German sources, regards Kalahari foragers as the most 'scientifically commoditized' (1986: 359; see also Bollig 1988) of colonial and anthropological history. Wilmsen and Denbow (1990) note the earlier German debate over the nature and degree of articulation experienced by Kalahari foragers, a debate based in part on field observations that began nearly a century before the Harvard expeditions. In fact, Wilmsen and Denbow draw a close analogy between this earlier controversy and the recent debate, suggesting thereby that the earlier case constitutes a *prima facie* argument against the Harvard position. That is, if German scholars were documenting extensive culture change nearly a century ago, Kalahari foragers in the 1960s could not be regarded as having remained isolated.

In fact, such isolation was a common theme of ethnographic accounts for decades before the Harvard expeditions began. But Lee's well-deserved preeminence in advocating the paradigmatic status of Kalahari foragers places him at the centre of the current debate. Similarly, Wilmsen's contrary view was foreshadowed by others (Elphick 1977; Gardner 1989; Schrire 1980). Nevertheless, the debate has largely crystallized around the positions of these figures. Such disputes are nothing new, but their examination 'can help us understand ethnography in general' (Heider 1988: 74) and perhaps identify its archaeological impheations as well.

Revision in the Kalahari

It is no surprise that the revisionist critique as a general phenomenon has aroused the ire of many Kalahari ethnographers. Wilmsen's *Land filled with flies (1989 a),* however, because of its postmodern foundation and its intensely polemical cast, has generated sharp controversy. The book's reception has ranged from the harshly critical (Harpending 1991; Silberbauer 1991) to a more balanced but still negative treatment (Kent 1992), to critical but generally positive (Barnard 1991; Headland 1990; Peters 1990) to, finally, one of high praise (Middleton 1991). It has also spawned a bitter exchange in the pages of a major anthropological journal (Solway & Lee 1990; Wilmsen & Denbow 1990), as well as serious charges of translation error and other linguistic flaws (Harpending 1991; Kent 1992; Lee & Guenther 1991).

Wilmsen's views, Eke Lee's, are expounded at length (Denbow & Wilmsen 1986; Wilmsen 1983; 1989#; 19896; Wilmsen & Durham 1988). They first place traditional Kalahari studies within the familiar tradition of materialist and evolutionary thought that has sought to reconstruct the trajectory of human social evolution as a passage through a series of stages. Like many others today, Wilmsen rejects this approach on account of its attribution of primacy to external, material factors, thereby treating social organization and institutions as derivative epiphenomena. Though Lee is at some pains to stress the social context of foodgetting, especially in his major work (1979), it is nevertheless fair to say, with Bird-David (1988: 19), that Lee regards 'the

hunter-gatherer social system [a]s ... geared toward, if not determined by, ecological constraints relating to a foraging economy.'

Moreover, Wilmsen claims that region-wide patterns of interaction have been neglected in the Kalahari ethnographic record, by virtue of their having been regarded as symptoms of contact and debasement, and hence as extraneous processes that must be filtered out in order to estabfish a pre-contact aboriginal baseline. Both Howell (1986) and Konner and Shostak (1986) are admirably self-critical in acknowledging the influence of this view on their previous research. That it has had such a strong influence supports Wobst's (1978) claim that ethnography emphasizes local, usually utilitarian, practices at the expense of regional interactions.

To this extent Wilmsen's objections to traditional Kalahari ethnography are largely philosophical. The controversy they have spawned would be of limited interest had Wilmsen not also charged Lee and other advocates of the prevailing model with serious errors of interpretation. Specifically, Lee (1976; 1979: 33360) claimed to find little structure in local group affiliation, little stability in local group composition, shallow genealogical depth to claims of legitimate tenure and, consequently, weak property relations. In fact, Lee questions the existence of property relations as strictly conceived, and posits instead a purely consensual basis for residence and affiliation. This view leads him to regard Kalahari forager society as conforming to the egalitarian collective of the prevailing model. Lee's latest thoughts in this connection are substantially unchanged; a cornerstone of forager societies such as the !Kung is their 'ability to reproduce themselves while limiting the accumulation of wealth and power' (1992: 39).

According to Wilmsen, however, Lee's analysis is flawed by its neglect of a historical factor, the nineteenth-century colonial displacement of native populations, which alone would reduce the genealogical depth of tenure claims by disrupting population distributions and thereby altering group composition and patterns of affiliation (Wilmsen 1983: 13; 1989a: 198). The historical question remains hotly disputed (Lee & Guenther 1991; Solway & Lee 1990 and comments therein). More importantly, Wilmsen charges Lee with misunderstanding critical elements of Kalahari kinship terminology and the rules of descent and relationship on which they rest. Lee, in this view, fails to appreciate the fact that 'affines are simply recategorized kin' (Wilmsen 1989a: 180). This may seem an esoteric point, but Lee's description of Kalahari kinship and land tenure follows from his terminological understanding, and it accords priority to flux over structure, collective access over privilege. Wilmsen's reading of kinship, by contrast, emphasizes structure, exclusion and the maintenance of unequal economic conditions and status. He identifies the land tenure system of foragers with those of their manifestly inegalitarian pastoral and agricultural neighbours, the Herero and Tswana (1989fe: 65). Indeed, he regards these as constituting a single system. In the face of such charges and counter-charges (Harpending 1991; Lee & Guenther 1991), the veracity of both Lee's and Wilmsen's accounts must remain in question. As regards its immediate implications for the Kalahari, the controversy can only be settled by Kalahari specialists.

The issue, however, carries broader implications. To Wilmsen, Kalahari foragers comprise part of a larger, stratified society. Even if viewed in isolation — a viewpoint which Wilmsen would consider artificial — their society would retain its quality of stratification, such that status is acquired by virtue of long tenure in particular waterholes, privileged access to local resources, and the unequal accumulation that results. Moreover, status is seen as jealously guarded by marriage and other links between members of the privileged class, and perceived inequalities in wealth are preserved and even increased by investment in capital goods such as firearms and horses. As Wilmsen shows, such goods increase the net acquisition efficiency of foraged resources to their owners (1989a: 252, table 6.14, fig. 6.12; Wilmsen & Durham 1988), and thus might further intensify wealth disparities within the society. Evidence is even adduced to show that surplus product is extracted from poorer members of society (1989a: 250). Other elements of the prevailing model had already fallen by the mid-1980s, when Konner and Shostak's autocritique could cite egalitarian social relations as its still-unchallenged cornerstone (1986: 73). Even this tenet — the egalitarian collective of traditional ethnography — is questioned in revision. All Kalahari societies, including forager ones, are stratified in Wilmsen's view, because all Kalahari societies are components of one, stratified society. To him, the debate is not simply about how isolated Kalahari foragers are or were, or how recently and how much they have experienced change. Instead, it goes to the heart of the prevailing model: egalitarian social relations.

Reaction to the revisionist critique has been varied, to say the least (Kent 1992; Lee & Guenther 1991; Silberbauer 1991; Solway & Lee 1990). In responding to Wilmsen, Lee has rightly emphasized the complex and variable history of contact in the Kalahari, and he sharply challenges what he considers to be the revisionist identification of contact with domination (Solway & Lee 1990; see also Kent 1992). Contact by itself does not imply domination, to revisionists or anyone else, but the crux of the dispute lies in whether such domination has in fact overtaken Kalahari foragers. In their own argument, Solway and Lee see no more than 'distant and intermittent contact' (1989: 118) between their subject population and more complex societies. Lee and Guenther (1991) expand on this point, and at the same time they question the factual basis of some of Wilmsen's interpretations. Similarly, Kent (1992) rightly celebrates the diversity exhibited by Kalahari foragers, especially in their response to articulation. She thus provides a useful counterbalance to what could be viewed as overgeneralization in the revisionist critique. Diversity or variation is a property of endogenous social relations as well, and it is as mistaken to ignore such diversity as it is to accept uncritically the revisionist argument concerning the origin, nature and degree of status inequality within Kalahari forager groups. Finally, Lee (1992; see also Grinker 1990) justifiably criticizes a tendency in revisionism to grant history to foragers primarily in the context of Western expansion rather than in their own terms.

The revisionist critique can only be evaluated after careful scrutiny; it is raised here not in order to stake out a particular position but to underscore the doubts being cast on the quality of Kalahari ethnography, which has been a particularly influential body

of knowledge to archaeologists. In fact, the middle ground is probably the best place to stand, whilst acknowledging the considerable merits of the arguments on both sides (Barnard 1991: 150; Trigger 1990).

Revisionism elsewhere

The revisionist critique is not confined to the ethnography of the Kalahari nor, for that matter, to studies of our own species. Similar critiques of primate studies (Haraway 1989; Konner & Shostak 1986: 74–5; Strum 1987) have challenged long-standing notions about, for instance, the ubiquity of gender inequality and the relative importance of social acumen and aggression. Studies of forager societies in other ethnographic regions have also been subjected to revisionist critique, and a brief review of selected studies is warranted for the sake of comparison with the most celebrated case.

Equatorial West Africa

Long a subject of anthropological inquiry, Pygmy groups of equatorial Africa are perhaps best known through the work of Schebesta (1936) and Turnbull (1965; 1968). Without denying the extent and importance of the Pygmies' interactions with agriculturalists, Turnbull sees in them only a short history and one of little consequence to the foragers. In fact, he regards the agriculturalists, not the Pygmies, as the 'enclaved' party, and the exchange relations the Pygmies have with them as of strategic rather than economic significance (Turnbull 1986: 104–5). This conclusion is directly at odds with the claims of Bailey (1991), Bailey *et al.* (1989) and Hart & Hart (1986), who consider Pygmy foragers to be virtual dependants of neighbouring farmers. Grinker (1990) also links Pygmies with farmers, but in a way that emphasizes their joint independence from larger social formations while still according superior status to the farmers.

Whatever the current economic status of Pygmy foragers, Vansina's (1986; 1990) reading paints a picture of their history that differs substantially from Turnbull's. Like Wilmsen in the context of southern Africa, Vansina considers the existence of Pygmies to have been heavily romanticized (1990: 29), and argues that their relations with other groups are of long standing (1986: 432). But it is not merely a question of exactly when the Pygmies lost their historical purity. To Vansina they, like Kalahari foragers, are simply the marginal class in a single, broader social system (1990: 29, 65; see also Waehle [1986: 190], who describes what amounts to a feudal system of obligatory labour performed for tribal chiefs). In traditional accounts, Pygmies are not only romanticized, they are also isolated from the larger context in which they occupy a subordinate position. Here too, according to Vansina, foragers are in reality marginal, not pristine. Moreover, the boundaries separating them from higher classes are as permeable (Vansina 1986: 431) as Wilmsen suggests they are in southern Africa.

Anthropologists who ignore the history of Pygmy interactions with others may, by Vansina's reasoning, misinterpret the origin, function and adaptive value of Pygmy subsistence practices (Harako 1976; Terashima 1983).

Another interesting parallel can be drawn between the Pygmy and Kalahari cases: kinship analysis figures prominently in the terms of the debates. As Lee and Wilmsen argue about the implications of the Kalahari kinship system for residence and access to resources, Pygmy scholars argue about the basis for what Turnbull (1968), in an influential paper, called 'residential flux'. Just as Turnbull considers kinship to have little or no bearing on patterns of residence and affiliation, others adduce strong kinship effects of that behaviour (Ichikawa 1978; Pedersen & Waehle 1988; Terashima 1985).

Social relations even within Pygmy societies may not be as purely egalitarian as portrayed in past accounts. Although their authority is limited and informal, certain Aka and Mbuti men occupy positions of apparently inherited status. These men tend to enjoy better diet, health and reproductive success than do others (Hewlett 1988: 271; Walker & Hewlett 1990). Although Kent (1991) suggests that men might rise to such positions by virtue of their hunting ability, access is apparently governed by affiliation, not accomplishment (Hewlett & Walker 1991). Evidently, there exists here at least the slight degree of status inequality that Lee (1990; 1992: 40) proposed as being characteristic of some forager cultures.

East Africa

One well known forager society in East Africa is the Okiek (often called Dorobo). Just as in the southern African context, anthropologists disagree about the history and transformations experienced by Okiek foragers. If they are indeed an autonomous group (Blackbum 1986: 79), then they represent an apparently rare example of an indigenous forager society in a 'non-marginal' environment (Ambrose 1986). Thus, the Okiek debate merits at least a brief review.

One school of thought traces its origins to Huntingford (1929), and regards the Okiek as culturally, socially and economically autonomous (Ambrose 1986; Blackbum 1982; 1986; Kratz 1981; Woodbum 1988). Blackbum (1986: 62–4), in particular, describes a finely tuned system of social relations, land use practices and subsistence behaviour that facilitates the harmonious distribution of population and promotes economic efficiency, a characterization that invokes the equilibrating properties of the prevailing model. Opponents of this view portray the Okiek as displaced and marginalized foragers whose relations and practices, far from constituting a distinct cultural system, merely comprise a set of coping strategies for the dominated and marginalized in a larger agropastoralist society (Chang 1982; Sutton 1987; van Zwanenberg & Press 1976). Galaty (1986) seeks to reconcile these views, but his discussion (1986: 12) underscores the primacy of pastoralist values shared by foragers and pastoralists alike. In the revisionist view, East African foraging is an economic condition, not a culture: 'It is the factor of poverty which distinguishes hunters and gatherers from the rest of

the community' (van Zwanenberg & Press 1976: 13). As economic fortunes change, individuals can and do pass from one class to another in this system, transformations that may appear as changes in cultural affiliation if the wider, all-encompassing social system is overlooked.

Both sides in this debate agree that Okiek foragers share the language and kinship systems of neighbouring agropastoralists. By themselves, therefore, these kinds of data may be insufficient to settle the issue in East Africa.

South India. South Indian foragers were the archetype for Fox's (1969) thesis of the 'professional primitive'. Morris, in his detailed study of the Hill Pandaram, agrees to some extent. He considers these foragers to be 'in no sense pristine' (1982: 3) and refers to political and economic relations of long standing with a broader Hindu society. In Morris's view, the Hill Pandaram have negotiated a semi-autonomous status (1982: 18) that nevertheless exposes them to 'a social situation that was decidedly exploitative' (1982: 23). This system is the product, evidently, of an expanding Hindu caste system that integrated its geographically and socially marginal components through outright serfdom or, at best, fitted them to a role as suppliers of specialized commodities (1982: 12–15). Even in the latter case, the Hill Pandaram can be described as 'vassals' (1982: 23) of the local raj. In Morris's view the various marginal groups, including the Hill Pandaram and other forager as well as agricultural populations, are the virtual property of the dominant caste. As in other cases, he (1982: 16) detects a continuum of statuses between the extremes of outright serfdom and negotiated semi-autonomy along which, presumably, individuals and groups may move according to their shifting fortunes. Once again, the boundaries between farmer and forager in a broader encompassing society are evidently permeable.

Morris (1982: 2) thus seeks a middle ground between the extreme views of the Hill Pandaram on the one hand as isolated primitives, and on the other as pure caste specialists. This is a wise policy that avoids the perils of the extremes, but the essence of his account is that the Hill Pandaram function essentially as a caste, while retaining a measure of cultural autonomy.

Southeast Asia

Philippine Negritos have often been regarded as autonomous foragers isolated from the larger societies that surround them. Headland and Reid (1989: 44) cite a number of early accounts and studies that represent this view, before going on to advance their alternative (1989: 45–6). On the basis of a combination of linguistic, ethnohistorical and archaeological data, they conclude that most Philippine Negrito groups have engaged in systematic 'symbiotic interaction' with agricultural groups and perhaps Chinese and Malay traders over at least several millennia. They emphasize the importance of rice in the Negrito diet, most of which is acquired through trade, not local production. Headland and Reid (1989: 47) conclude that Negrito foragers are forest specialists in

a larger system similar in important respects to those found in equatorial Africa and southern India.

In an interesting reversal, recent anthropological views of the Punan of Sarawak were characterized first by a lengthy revisionist exposition (Hoffman 1984; 1986) and only later by a vigorous defence of the prevailing model. Hoffman regards the Punan groups as marginalized agriculturalists, much as some in East Africa see the Okiek. Brosius (1988) and Sellato (1988), however, maintain a more traditional position that treats these groups as culturally and economically autonomous. They criticize Hoffman's methods, point to the short duration of his fieldwork, and question his command of relevant languages and non-English sources. As elsewhere, linguistic and kinship data on degrees of similarity between foragers and adjacent agriculturalists are invoked by both sides in support of their arguments.

North American Subarctic. Ethnographic accounts of high-latitude foragers have often inspired archaeological reconstructions of Palaeo-Indian and other forager cultures in North America. Some scholars celebrate the resourcefulness of these cultures, their ability to withstand the penetration of European colonial powers and the dislocations that accompanied the ensuing fur trade (Francis & Morantz 1983: 14–15, 96–7). In this perspective, the flexible collective governed by bilateral rules of affiliation and residence is an adaptation both to difficult environmental conditions and to the disruption brought by European invasion. Moreover, these scholars suggest that in their structural and organizational particulars, ethnographically documented high-latitude forager groups differ little from their ancestors. Others, by contrast, attest to radical, far-reaching transformations (Leacock 1954; 1982; Yerbury 1986). The aboriginality of documented subarctic land tenure systems is in question and still unresolved, although the ethnographic debate has gone on for some years.

Even in this case, the issue is important not only to students of North American forager cultures, but to a wider audience as well. Binford's (1980) renowned forager-collector opposition rests to an important extent on ethnographic data acquired from recent studies of high-latitude foragers. For archaeologists, this opposition has proved enormously influential, and has informed analysis of countless prehistoric cultures. But if prehistoric collectors *(sensu* Binford) differed significantly from the contemporary ethnographic exemplars of the type, the opposition itself requires revision if not re-evaluation (Binford 1990).

North American Great Basin

Forager groups in the Great Basin of the western United States have also figured prominently in models fashioned by American scholars. Great Basin Shoshone and Paiute groups served as exemplars for Steward's (1938; 1955) 'family level of sociopolitical integration'. Service (1962: 83–8) later argued, in opposition to Steward, that ethnographically documented Great Basin groups were the transformed remnants of disastrous invasion by Euroamerican society. In his view, their weak sociopoliti-

cal organization in composite bands exemplified the tragic consequences of this invasion, whereas their predecessors were considered, like virtually all foragers in the pre-industrial world, to have been organized in highly structured patrilineal bands. In the views of both Steward and Service, Great Basin forager groups were taken to represent a general stage or condition of forager cultures.

Steward's original data were limited, a fact that he freely acknowledged (1938: 3) and that others have long since realized. They may, however, have been skewed as well. Great Basin foragers with relatively dense populations, and organized into fairly cohesive social groups, were largely excluded from Steward's survey (Thomas 1983: 60–2). Though no criticism of Steward, recent archaeological work suggests that prehistoric Great Basin groups were more complex in organization than were some ethnographically documented ones; in fact, a historical trend toward *declining* complexity may be seen from this evidence (Thomas 1983: 63–4).

Summary

Viewed in revisionist perspective, what links these cases to the Kalahari debate are disputes over the historical isolation and the cultural and economic autonomy of ethnographically documented foragers. (Isolation and autonomy are of course distinct; autonomy can be maintained in contact with others.) What distinguishes them from the Kalahari case and, to some extent, from one other is the degree of class inequality revealed in each instance. In most cases, the existence of class relations is at best suggested, and in only one — the South Indian — is some measure of class structure and surplus extraction actually described.

Thus, class relations seem especially prominent in the Kalahari case. Two possibilities may account for this observation: either ethnologists who work elsewhere are not interested in class relations and so ignore them, or there exists genuine variation between regions and societies in the extent of class differentiation. In the first case, Kalahari revisionism is distinguished by its emphasis on class structure; in the second, it is Kalahari class relations themselves that are distinctive.

Implications

The revisionist critique in the Kalahari and elsewhere carries significant implications for archaeology, some of which are explored in this section.

(1) Perhaps the first and most important implication of the revisionist attack is the doubt it casts on the validity of ethnographic data. This should at least have the salutary effect of making archaeologists more critical consumers of ethnographic data in the future. They might regard more sceptically, for instance, the findings of kinship analysis, which figures importantly in the Kalahari and other debates, and ponder the necessary limits that ethnographic practice can impose on the validation of anthropological knowledge (Heider 1988; Isaac 1990b; Wobst 1978: 303). Archaeology,

of course, is open to similar criticism, but because archaeologists consume ethnographic data the ethnographic dilemma is especially relevant to them.

Obviously, more work is needed in the Kalahari, but anthropologists might also begin a programme of new research on selected forager societies, especially those whose study has contributed to the general model. These might include, for instance, the Hadza of Tanzania, tropical forest groups in Africa and Southeast Asia, and North American Subarctic groups. Ideally, each study would engage several independent analysts, continuously for a number of years, thereby avoiding the pitfalls both of short-term studies (Isaac 1990b; Wilmsen & Denbow 1990: 494; Strum [1987: 23–4] makes a similar point with respect to primate field studies) and of personal bias (Heider 1988). Lee and DeVore's Harvard Kalahari Project was conducted, and perhaps still is, over a considerable time span; moreover many anthropologists work today, with groups like the Hadza and Ache, either individually or in modest research groups. No mere arrangement for sustained or renewed study is advocated here. Instead, reasonably long periods of continuous study using standard research agendas (which, of course, would permit some individual choice in methods and topics, but only within a systematic framework) are needed. Doubts cast on the autonomy and historical integrity of some forager societies, especially those of the tropical forest, might be either strengthened or dispelled under further scrutiny (although certain tropical forest foragers are extraordinarily well documented by now), using archaeological and archival data as well as new ethnographic observations. In this way, the historical trajectories of these groups might be revealed, the viability of their present organization assessed, and their suitability as archaeological analogues determined.

No contemporary forager culture is an ideal exemplar of any prehistoric one, and the accumulated ethnographic record cannot be treated as an archive of the distant past. Nevertheless, it should not be assumed that any and all involvement with states and other groups necessarily leads to loss of cultural integrity. To the contrary, forager groups may well retain their cultural integrity and hence their value to archaeologists in revealing the systematic relationships between key variables (Bettinger 1991: 144; Ember 1978: 447; Hawkes 1987; Kent 1992: 54–5), a value demonstrated in many cross-cultural studies.[8] It may be argued that 'the same fundamental evolutionary and ecological principles' (Bailey & Aunger 1989: 495) — and for that matter structural and symbolic principles as well — may be applied to both the ethnographic and the archaeological domain. To some extent, this is an affirmation of Lee's uniformitarian premiss, although Lee largely confines his application of this premiss to the material domain and argues that it 'is not adequate to deal with problems of consciousness' (1979: 436). Uniformitarianism is implicit in a wide range of cross-cultural studies of

[8] My own use (Shott 1989a; 19896) of Lee's (1979: table 9.10) extremely valuable data on Kalahari technology and Yellen's (1977) equally valuable assemblage-composition data must be noted in this connection. Criticism of the archaeological use of ethnographic data on foragers, here or elsewhere, must not be construed as universally invalidating such data or their use.

foragers, whether by or for the use of archaeologists (e.g., Binford 1980; Ember 1978; Hayden 1981; Shott 1986; Torrence 1983).

(2) Although prevailing ethnological models of forager societies differ substantially in their particulars, egalitarian social relations are a central feature of virtually all of them (e.g., Ingold 1987: 113, 223; Leacock 1982; Lee 1988; Woodburn 1988). Indeed the general consensus in this respect is remarkable. Even the most recent of archaeological surveys of forager societies generally accepts this tenet, although it recognizes complexity in forager cultures where the conditions of preservation so allow (Bettinger 1991). But revisionist (Wilmsen 1989d) and other studies (Hitchcock 1987: 239–44), foreshadowed in archaeology in the last decade (Bender 1985; Kehoe 1981; Soffer 1985), have questioned this central tenet. At the very least, egalitarian social relations in forager societies must be demonstrated, and archaeologists should not assume that prehistoric forager societies lack 'clear instances of exploitation, classlike groups, or inherent structural contradictions' (Bettinger 1991: 146). This will require us to identify reliable and unambiguous archaeological indicators of unequal social relations (Trigger 1985: 208–9). Only then can we avoid overdependence on the ethnographic record, and the danger of merely reproducing it in the archaeological one.

(3) In a related connexion, archaeologists should not rely so heavily on ethnology for the general theory that they apply. Instead, they should develop theory that takes into account not only material conditions and their influence on subsistence practices and other cultural behaviour, but also the intrinsic roles of social structure and of ideology in subsistence. This theory must consider how and to what extent subsistence practices and social organization are linked. It must determine, for instance, whether subsistence requires particular forms of social organization or whether, by contrast, social factors are primary and determine subsistence practices; or indeed, whether the two are functionally related at all. In addition, such theory must identify the causes of both subsistence and social change. Traditionally, causal factors are perceived as external (e.g., environmental change or the kind and degree of risk), or else they are considered internal but uncontrollable (e.g., endogenous population growth). But societies, of course, may possess other internal dynamics that impel change, at least under certain conditions (Bender & Morris 1988: 7–9; Trigger 1982: 6). Identifying and characterizing these dynamics is one task of a general culture theory, and specifying their material correlates is a corresponding task of an expanded archaeological theory. Yet as Comaroff (1984: 582) notes, 'it is one thing to make this observation and quite another to meet its demands'. At present, it is justified if only by the conviction that we shall never learn to read the internal dynamics of social processes if we do not try, or if we are unconvinced of their importance.

In addition, the role of interactions between cultures at the same and at different levels of organization might be considered at greater length. Interactions of forager cultures can occur over great distances, but their study has been a somewhat neglected area of ethnological research (Wobst 1978). If such interactions also took place between prehistoric forager groups, then the control provided by historical documentation in

the Kalahari and elsewhere offers enormous potential for the archaeological study of such processes and of how they are registered in the material record.

In fact, much of the existing ethnographic record the world over registers interactions between foragers and other societies — or at least other components of larger societies (Leacock 1982: 164). There surely exists here a wealth of data that archaeologists can use to explicate the material record of the past ten thousand years or so. Documented patterns of interaction in the recent past can serve as controls for the archaeological study of more distant periods. In turn, the improved archaeological understanding of how such interaction is registered in the material record should allow us to study the historical development of foragers and others further back in the past. Thus, archaeology may be uniquely suited to the task of formulating and testing appropriate models of regional interaction (Hayden 1982; Wobst 1978), using ethnographic data but not relying on them to the exclusion of other sources of knowledge.

(4) A fourth implication of revisionist critiques concerns the role that archaeology can play in the development and evaluation of ethnological theory. This prospect reverses the roles these fields have traditionally played, but it is now clear that knowledge of the paths cultures have followed prior to their ethnographic documentation may be crucial to the validation of ethnographic knowledge (Headland & Reid 1989; Hitchcock 1987;

Leacock 1982: 164; Trigger 1985; 1990). This process has begun already in Africa (Denbow 1990), but a great deal of work lies ahead.

Ethnology can help resolve the current debate concerning the viability for foragers of tropical-forest habitats prior to the advent of agriculture, but 'in the end, it will be on the basis of the archaeological evidence' (Bailey 1990: 278) that the matter will rest. Similarly, Hitchcock (1987: 225) sees great and as yet unrealized potential for archaeology to contribute to the resolution of current debates in Kalahari ethnography, such as the causes and origins of class structure. Few ethnologists have expressed such sentiments in the past; Kuper (1988: 7), for instance, declares that 'there is no way of reconstituting prehistoric social forms', thereby denying to archaeology a role in the current debate. This view is as understandable as it is common, because archaeologists have rarely demonstrated the ability or inclination to help resolve issues in ethnological theory. But if archaeology needed ethnology in the past, ethnology needs archaeology just as much today:

Whatever reservations ethnologists and ethnohistorians may have about the adequacy of archaeological data or about their compatibility with historical or ethnographic data, they must recognize archaeology as being the major source of information concerning cultural change that is required to set early ethnographic information into an adequate historical context. Reliable archaeological data are clearly preferable to unverified ethnographic speculations (Trigger 1985: 118).

Ethnologists can dispute in specific cases the nature and extent of factors like market penetration and other forms of systematic culture contact, but archaeologists alone can provide the critical knowledge to settle such disputes.

(5) Yet another implication of the revisionist critique exists for those of us who study early foragers in North America and elsewhere. We deal routinely with extremely sparse material remains and we have a strong tendency to see in them the properties of the prevailing forager model. No one seriously argues, of course, for identity in particulars, but we do tend to infer the general properties of the model: an egalitarian collective with simple, flexible sociopolitical organization. In the light of the current debate, we should consider the social context of Palaeo-Indian subsistence, the social conflicts, if any, endemic to Palaeo-Indian cultures, and the internal dynamics and historical trajectories of these cultures.

In theory, big-game hunting, still considered by some as a Palaeo-Indian hallmark, is consistent with more than one subsistence strategy. As Moore (1981) has shown, viable strategies depend not only on the acquisition efficiency of resources, but on their reliability as well. More than this, they also depend on social factors that are not determined by objective environmental conditions: information, goals, and decision-making rules. Specifically, the amount of information available to social groups — themselves constituted in size and structure in ways at least partially independent of material conditions — can be characterized on a scale ranging from none to full; the quality of that information from conditional to prescriptive; and decision-making rules from those of risk-minimization to those geared to the maximization of marginal utility. Moore (1981: 190–215) demonstrates that the combinations of these social factors create a range of possible economic strategies, and wisely counsels against 'detailed assertions of optimizing behaviour' (1981: 345). All such potential variation is masked in the traditional view, which emphasizes what was eaten at the expense of the social practices inherent in the subsistence quest itself.

Bishop (1989: 52) adopts a traditional view of Palaeo-Indians in an otherwise revisionist brief on North America, by suggesting that PalaeoIndians may have been the egalitarian exception to the continent's inegalitarian past (see also Kehoe 1981). Perhaps we can evaluate this possibility before long, in the process of developing a more detailed and sophisticated view of Palaeo-Indian society.

(6) In some respects, the history and nature of interaction between forager groups and other societies may appear insignificant. The terms under which forager groups articulate with outsiders, after all, are irrelevant to the material conditions in which they live. The diet-breadth model and other elements of optimal foraging theory, for instance, should apply regardless of the degree of cultural autonomy that foragers enjoy. Indeed, such theory should apply in carefully qualified circumstances, and it should continue to serve anthropology's attempt to understand forager societies. However, to the extent that environmental conditions have been substantially altered, the specific dimensions of subsistence behaviour observed ethnographically cannot be expected faithfully to reflect past behaviour. For instance, Belovsky's (1987: 52–3) attempt to generalize from the rates of subsistence return and other material parameters characteristic of recent Kalahari foragers does not take account of the apparently serious degradation that the southern African habitat has undergone in the recent past (Bol-

lig 1988: 114; Isaac 1990a: 329). Nor does it consider important changes in the rate at which hunting occurs — as distinct from the rates of return on hunting (Wilmsen 1989a: 232). Eder (1987: 79) makes a similar point regarding Batak econometrics.

Even more important, the social context of production in exploited, marginal groups may include demands for surplus product imposed by elites; Morris (1982: 16, 23) suggests as much for the Hill Pandaram in South India. The presence and extent of surplus extraction is unclear from most recent ethnographic accounts, but we should be alert to its possible effects on forager subsistence practices. If foragers are producing at least in part for others, certain key assumptions of materialist models — such as freedom of choice between alternative resources and the direct relationship between food-getting success and individual welfare — are clearly violated.

This is not to suggest that rigorous ecological analysis should be abandoned. Material conditions are obviously relevant to any forager society, but the nature and extent of their importance is not everywhere the same. In fact, relieved of the heavy burden they bear in cultural materialist approaches, environmental conditions and ecological data can serve a critical role in a more informed and comprehensive theory of forager social formations.

Towards new archaeological approaches

In the Kalahari and other regions of southern Africa, archaeological evidence has until recently contributed little to anthropological understanding of the origin and nature of forager societies, although archaeologists increasingly realize that foragers in these regions have a more complex and dynamic prehistory than previously recognized (Denbow 1990; Maggs & Whitelaw 1991). The revisionist critique has brought archaeology to the fore, although it is used chiefly to assert or deny claims for historical isolation, and disagreements over the interpretation and meaning of the evidence (Wilmsen & Denbow 1990; Yellen 1990) are as sharp as any that exist among ethnologists. Clearly, archaeologists as much as ethnologists need critically to reconsider their interpretive predilections.

Some evidence suggests that the process is already beginning. Dennell (1985: 113) posits a complex view of Neolithic prehistory in Europe that attributes to foragers autonomy, persistence and an active role in the process of culture change. Foraging as a survival strategy for marginalized farmers in a context of emerging economic and social inequalities is an integral component of Parks's (1992) recent model of prehistoric cultural development in the Nile basin.

Archaeologists' judgements may change, but also their interpretive conventions remain in dispute. We may suspect that the past of human foraging was more complex and diverse than evolutionary-stage theories would suggest, but how can we know that it was so? How, that is, can archaeology validate claims about complex pasts characterized perhaps by major status inequalities within forager societies? Questions of this sort are premissed on the proposition that the past is, in some ways and to some degree, knowable. Despite the well-documented excesses of what most call 'processual archaeology', a commitment to this proposition does not condemn its holders on intel-

lectual grounds. Thus, it is worth contemplating some of the ways in which we might learn about the complexities of forager prehistory.

One way, of course, is to acquire more data on that prehistory. A small but growing corpus exists for the Kalahari and other regions, but it remains modest in scale. 'More data', however, is a common archaeological plea, and its acquisition more often fuels controversies than resolves them. Without clear notions of what kinds of behaviour and organization the material record registers, more data are as likely to complicate as to settle disputes. The central issue raised by the revisionist critique concerns not the degree and duration of isolation of forager societies, but whether or not they are flexible egalitarian collectives. Archaeologists, therefore, must develop means of distinguishing flux from rigidity in residence and affiliation, and of determining the kind and degree of internal status inequalities. Fortunately, archaeology already possesses several procedures for achieving these objectives.

Status inequalities may produce differences in the quality and amount of food available to members of a society, differences that can be measured by compositional analysis of ancient human bone (Price 1989). Unequal diet might also be reflected in the kinds and distributions of food remains within and between sites, although processes of assemblage formation (Schiffer 1987) vastly complicate the reconstruction of patterns of access to resources. Mortuary evidence, subject to its own interpretive problems, is nevertheless a common source of information on the organization of forager societies. Unequal access to wealth or property may be registered in uneven archaeological distributions of their imperishable traces, once such traces can be unambiguously identified. Networks of exchange and how they might be manipulated in the interests of certain classes or groups can also be studied. Again, however, simple inference can be defeated by the complexities of formation processes (Schiffer 1987: 294–8).

The rigidity or flexibility of local-group affiliation and composition might be measured by the degree of stylistic uniformity in ceramics and other kinds of artefacts, although foragers do not always use ceramics and courageous early attempts at such inferences in other cultures ran foul of a host of complicating factors (Plog 1978). It is also possible that stable marriage patterns, a likely concomitant of rigid forms of group affiliation, might be expressed in genetic markers visible in skeletal remains.

The application of these and other techniques has already established the presence of cultural complexity in some prehistoric forager cultures (e.g., Price & Brown 1985). To date, however, and as a necessary consequence of the requirements that these techniques place on the quantity and quality of data, they have been applied primarily in contexts where the vagaries of preservation have yielded archaeological records of great abundance and diversity. When preservation conditions are less favourable and the available record correspondingly smaller and more impoverished, the evidence for possible complexity is more equivocal. Until we devise ways of reconstructing from sparse material remains the value systems that gave meaning to the production and distribution of food and wealth, a form of preservational determinism is bound to dominate the character of our inferences about the past. Studies of technological orga-

nization (Binford 1979) have expanded the basis for making archaeological inferences from material remains but, to date, more in the domain of subsistence practice than in that of social organization. Archaeologists have still to give serious thought to how sociopolitical organization is registered in sparse remains like stone tools.

At the same time, we require far more study of what archaeologists turgidly call 'assemblage formation processes' (Schiffer 1987). Nearly two decades after formation processes were identified as important determinants of archaeological assemblages, we still pay more lip service than serious regard to the need to consider and control for the myriad factors that produce the material record before simply ascribing its character to specific kinds of behaviour or types of social organization.

Conclusion

Contemporary forager societies do not manifest a single, homogeneous organizational form, nor do any stand as exemplars of primitive society. Instead, forager societies both today and in the past have been drawn into wider sets of social and economic relations ever since, and probably well before, their documentation by ethnographers. These are not original observations, but the revisionist critique brings them to the fore. By questioning the assumption that foragers are historically and geographically isolated and autonomous, revisionism may become the instrument that finally rids anthropology of the romantic notion of a primitive society which, according to Kuper (1988: 8), 'does not and never has existed' (cf. Lee 1992: 33).

If we reject the prevailing forager model, based as it has been to an important extent on Kalahari ethnography, in favour of a particularist one grounded in the revisionist critique, we shall merely continue an intellectual cycle that began over a century ago (Trigger 1990). We require instead a synthesis of the general and the particular, one that would address 'common problems and challenges' (Comaroff 1984: 573) by combining a legitimate concern for case-specific historical developments with a commitment to general theory and explication, and that would thereby break the cycle by integrating specific and general perspectives. Archaeology has an important role to play in forging this synthesis. The next decade is bound to be a stimulating one, and archaeological research will be animated by recent ethnographic controversies even as it sheds its uncritical reliance on the received ethnographic wisdom.

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A propos des courants de pensee actuels dans 1'anthropologie des societes de chasseurs-cueilleurs : le revisionnisme Kalahari et ses implications archeologiques.

Resume

Bien qu'etudiant le changement des societes de chasseurs-cueilleurs au cours de 1'histoire, I'archdologie se base sur I'ethnologie pour en comprendre les formes

d'organisation sociale. Jusqu'i present, le module ethnologique predominant pour les societes de chasseurs-cueilleurs mettait 1'accent sur l'abondance, la flexibility et legality. Toutefois, ces propri£t£s sont maintenant remises en cause, et, d'autre part, I'utilisation par les arch£ologues de ces donn£es ethnographiques a £t£ s£rieusement mise en doute. Tous ces d£bats conduisent & questionner la validity de certaines inferences ethnologiques pour la comprehension de la nature des societes de chasseurs-cueilleurs, telles qu'elles sont documentees ethnographiquement. Pourtant, comme se propose de le montrer l'auteur, ces questions ne peuvent etre tranchees qu'avec l'appui des donnees archeologiques.

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Notes

Valuable comments were received from Robert Bettinger, Lynn Fisher, Barry Isaac, an anonymous reviewer and from my colleagues Phyllis Baker, James Chadney, Mark Grey, Thomas Hill and Ann Woodrick at the University of Northern Iowa. Tim Ingold's many substantive and editorial comments greatly improved the text. In all sincerity, my greatest debt is owed jointly to Richard Lee and Edwin Wilmsen, each of whom has made great contributions to forager anthropology. The article is more critical of Lee than of Wilmsen, partly because of the extent and scope of Lee's work. Moreover, the serious problems raised here relate more to the ways in which archaeologists have used his work than they do to the intrinsic merits of his own research. This article draws heavily on one that appeared in a recent issue of *Michigan Discussions in Anthropology,* my thanks go to the issue's editors — Preston Miracle, Lynn Fisher and Jody Brown. The Interlibrary Loan staff at the University of Northern Iowa greatly assisted my efforts, and deserve a special note of thanks. Any errors or omissions are my responsibility alone.

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On Recent Trends in the Anthropology of Foragers
Kalahari Revisionism and Its Archaeological Implications
1992

Man, New Series, Vol. 27, No. 4 (Dec., 1992), pp. 843–871.
<jstor.org/stable/2804177>
Royal Anthropological Institute of Great Britain and Ireland