BOOK REVIEW 1-;SSAY

SUPPOSING HUNTER-GATHERER VARIABILITY

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Constructing Frames of Reference: An Analytical Method for Archaeological Theory Building Using Hunter-Gatherer and Environmental Data Sets. LEWIS R. BINFORD. 2001. University of California Press, Berkeley. 583 pp. $75.00 (cloth) ISBN 0-5202-2393-4.

Cultural Diversity among Twentieth-Century Foragers: An African Perspective. SUSAN KENT, editor. 1996. Cambridge University Press, Cambridge. 344 pp. $100.00 (cloth), ISBN 0-5214-8237-2.

Hunter-Gatherers: An Interdisciplinary Perspective. CATHERINE PANTER-BRICK, ROBERT H. LAYTON, AND PETER ROWLER-CONWY, editors. 2001. Cambridge University Press, Cambridge. 341 pp. $90.00 (cloth), ISBN 0521772109, $30.00 (paper), ISBN 0-5217-7672-4.

J B. S. Haldane, the British geneticist, once mused, "my suspicion is that the universe is not only queerer than we suppose ,but queerer than we can suppose" (Haldane 1927:286). This comment can be paraphrased to summarize the fundamental problem presently facing hunter-gatherer studies: there is more variation among recent and especially past hunter-gatherer societies than we have supposed. The central theoretical problem is whether there is and was more variability than our current theories can suppose. These three books represent very different approaches to supposing variability.

Hunter-gatherer studies matter. They are at the core of the anthropological and archaeological enterprise (Bettinger 1991), which is explaining the tremendous diversity among human cultures. Hunter-gatherers are, as Bettinger (1991) says, the ultimate testing ground for general anthropological and archaeological theories. Additionally, hunter-gatherer studies are one of the few remaining places where sociocultural anthropology, archaeology, bioanthropology, and even linguistics intersect. Hunting and gathering also matters because it was the effective environment within which some portion of human biological evolution occurred, shaping our cognition and physiology. Beyond these disciplinary concerns, hunting and gathering people today face urgent social, economic, and legal issues that need to be addressed.

However, "hunter-gatherer" is an unruly class of human society, with ambiguous boundaries and even members. Hunter-gatherer studies are fractious with sometimes bitter disputes (e.g., Hallpike and Wilmsen 2002; Woodburn et al. 2001) mirroring broad controversies in anthropology and archaeology. These three books reflect the significant intellectual differences existing among hunter-gatherer scholars, without exhausting them. Some important issues are not well covered, including symbolic systems, gender, sovereignty, and resource conservation and land-managing practices (e.g., Smith and Whishnie 2000). They are touched on explicitly and implicitly, but are not the foci of any of these works. They are about supposing variability, in sometimes markedly different ways.

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The methodological and theoretical issues they raise, particularly Binford's *Frames of Reference,* are far too vast to be treated in a single essay. I focus on those I think are central: the use of ethnographic data on recent hunter-gatherers to suppose hunter-gatherers in the past; our difficulty in even defining hunter-gatherers as a phenomenological class; unruly variation in subsistence and social organization (including social complexity); explaining the sources of hunter-gatherer variability; and lastly, what is the appropriate theoretical framework with which to explain that variability. I first briefly introduce the books themselves.

The Books

Cultural Diversity among Twentieth-Century Foragers: An African Perspective (hereafter *CDTCF)* has several goals. It is a rebuttal of "historicists" (Myers 1988; Shott 2001) such as Wilmsen (1989) and Schrire (1980) who maintain modem hunter-gatherers, particularly in Africa, and most particularly in the Kalahari, are a rural proletariat who have actively shifted their social and economic organization over the centuries in response to invasions, colonization, inclusion in regional and world systems, etc. They are not "pristine hunter-gatherers" if they are hunter-gatherers at all. A second purpose of the book is to negate what Kent saw as a strong tendency to homogenize and stereotype the cultural and organizational diversity among modem hunter-gatherers by showing the great cultural diversity among hunter-gatherers at the heart of the historicist debate. Finally, the book is intended to counter strongly ecological and materialist explanations of hunter-gatherer variability. The level of analysis is generally fine scale, at the level of individuals (e.g., Kent *CDTFC)* or groups family-sized or slightly larger (e.g., Guenther *CDTFC,* Silberbauer *CDTFC).* Most authors are cautious about generalizations or comparisons or eschew them (e.g., Bird-David *CDTFC).* Kent, in her abstract, claims generalizations about hunter-gatherers are not possible but later makes some (see below). The book's chapters cover groups in sub-Saharan Africa written by a relatively diverse array of authors and viewpoints from evolutionary ecology to the mildly postmodernist. There are no archaeological or bioanthropological papers.

Panter-Brick, Layton, and Rowley-Conwy edited *Hunter-Gatherers, An Interdisciplinary Perspective (HGIP)* to correct what they saw as several problems in hunter-gatherer studies generally and the *Cambridge Encyclopedia of Hunters and Gatherers* (Lee and Daly 1999a) specifically. These include increased specialization among hunter-gatherer researchers with a resulting breakdown in cross-disciplinary communication, coupled with a drift away from ecological and materialist approaches. The book therefore includes chapters by bioanthropologists, archaeologists, a linguist, and a social anthropologist. The papers are generally synthesizing, attempting to pull together a range of topical data on hunter-gatherers and drawing conclusions within the topic's sphere, although some are rather narrow. Each chapter stands on its own.

Constructing Frames of Reference: An Analytical Method for Archaeological Theory Building Using Ethnographic and Environmental Data Sets (hereafter *CFR)* is Binford's magnum opus. In it he has three basic goals: exemplifying how a scientific archaeology should proceed in developing a firm inferential base; using ethnographic data rather than analogies to build theories about the past (not to interpret the past); and explaining variability among hunter-gatherers. He develops data sets based on evidence for 390 recent hunter-gatherer groups and modem environmental attributes (rainfall, effective temperature, mean annual temperature, elevation, biotic class, etc.) to construct what he calls frames of reference that can be projected against each other to discover broad patterning in an inductive process termed "pattern recognition research." In addition to pattern recognition (e.g., at what degree latitude does reliance on storage increase), Binford builds several models. Key models include the "basic terrestrial model" that predicts aspects of the behavior of highly mobile terrestrial hunters (e.g., could a particular environment support such a group) and a "group size model" that predicts group sizes given certain parameters. Using the data sets and models, he works his way inductively through a tremendous range of issues. The book is demanding, dense with x/y scatter plots, tables, formulae, neologisms, and difficult prose. It calls for at least two readings, one rapid to establish what is going on and a second, slower reading to think about it. Close and careful reading is also required because it contains significant copy-editing and computational problems (misla-

beled and mispositioned graphs, undefined variables, etc.). It is also extremely thick and rich in ideas that will take years to work through and test, both using Binford's data and formulae, and archaeological data sets. In fact, most of his generalizations can only be tested archaeologically. Binford continues to build his version of middle-range theory: the construction of generalizations that can be used as bridges between observations of the archaeological record and theory. The theoretical framework is strongly rooted in Julian Stewart's cultural ecology and multilineal evolution (Stewart 1955), merged with some evolutionary ecology and complex adaptive systems theory (e.g., Bentley and Maschner 2003; Kaufman 1993; Kohler and Gummerman 2000; Lansing 2003), particularly the concepts of self-organized criticality and emergence. The appropriate analytical and phenomenological scales are therefore important. The scope is global, the coverage coarse-grained, and there is no reluctance to draw generalizations.

The Ethnographic Record

It was once standard to use modem hunt r-gatherers as analogies for the behavior and organ ation of ancient *Homo sapiens sapiens as* well as earlier hominids. The historicists, among others, vehemently critiqued this practice. They argued that hunter-gatherers of the Kalahari had shifted from hunting and gathering to herding and farming and back over the past several centuries. As a consequence, they are not "pristine," i.e., have not continuously been hunter-gatherers since the Pleistocene, and so cannot be used as analogies for ancient hunter-gatherers. They faulted any idea that these or other similar cultures were economically independent and isolated from the social and economic systems around them, and further insisted that modern hunter-gatherers could only be understood through study of their history and broader socioeconomic and political context. Any generalizations and extrapolations into the past based on them were misleading and wrong. The response has been strong (e.g., Kent 1992; Lee 1992; Lee and Daly 1999b). Among the issues this debate produced are questions of the reality, genuineness, or pristiness of modem hunter-gatherers, questions reviewed by Layton *(HGIP),* Kent (1992, *CDTCF),* and other authors in *CDTCF.*

While this is an important issue, the debate tells us more about ourselves and our preconceptions and assumptions then it does about hunter-gatherers. With all due respect to many fine scholars, it is difficult to imagine a similar fiery debate over whether modern or ancient farmers are real, pristine, or genuine. We are not much troubled when we learn the farmers we are studying have shifted between farming and pastoralism for millennia. In fact, we are likely to think it is interesting, not a problem.

Americanist archaeologists will see strong parallels between the broader controversy and longstanding debates about the use of ethnographic analogies drawn from ethnographies of Native Americans to interpret or model the North American past. One trenchant issue is that the diversity among ancient societies is far greater than that in the ethnographic sample.

However, there is no alternative to using our knowledge of modem peoples to help us penetrate the past. Abandoning the ethnographic record makes archaeology like a paleontology cut off from the biology of living organisms. The real issue is not whether we do it, but how we do it. There are several alternatives. One, followed in the *Cambridge Encyclopedia* (Lee and Daly 1999a), examines ethnographic case studies to epitomize certain characteristics that hunter-gatherer societies are taken to possess or the dimensions along which they vary. Another approach is that of ethnoarchaeology and many evolutionary ecologists, who conduct fieldwork among living hunter-gatherers to test models that in their simplicity and universal theoretical underpinnings are thought to be time and space free, applicable equally to the past and the present, methodological issues aside (e.g., Blurton Jones et al. *CDTCF).* The third approach examines the extant ethnographic record. Kelly (1995) did this in his hunter-gatherer book, using that record to test families of hypotheses and models to demonstrate the value of evolutionary ecology in explaining hunter-gather diversity. The bioanthropology papers in *HGIP* do this to a limited degree.

Binford's approach differs from these. His explicit intent is not to test hypotheses, but to inductively explore variation in the extant ethnographic record, seeking second-level patterning not explainable by particular histories and circumstances. He finds several such patterns, most importantly the

"packing threshold," a population of 9.028 people/100 km^2. This threshold is a Rubicon for major social and economic changes. Below it, hunter-gatherer groups respond to environmental and social stress by mobility; above it, mobility becomes increasingly constrained, and other organizational and technological changes necessary. Chief among these is intensification of food production.

Recognizing some of the inherent biases in the ethnographic data sets (e.g., places such as Europe where hunter-gatherers are mainly known only archaeologically), Binford uses his data sets and models to predict the terrestrial hunter-gatherers who might have been there before they went extinct. He also maintains, and this is the key point, that he is not using these data to construct analogies or to "interpret" the past, but to identify dimensions of variability and to explain variation along those dimensions, regardless of whether the variation is among extant or extinct hunter-gatherers. On the other hand, he does not recognize that his sample, as large as it is *in toto,* is biased by history. For example, while he has 390 societies in his sample, 26 are stratified. Of those, 22 are from the Northwest Coast and immediately adjacent areas. Two more are from southern California. That subsample is numerically and geographically small enough that history may have played a significant role in its formation.

Hunter-Gatherer Variability

Who's a Hunter-Gatherer

One significant source of apparent hunter-gatherer variability is that we cannot define the class "hunter-gatherer" rigorously or consistently. Part of the difficulty is a lack of agreement over what are the key dimensions of variability in which we are interested. Definitions emphasize either subsistence or social organization and worldvew. The former position maintains there is too much organizational variation to permit a consistent definition based on anything but subsistence, while the latter position argues that foraging is as much or more a matter of social organization and ethos as it is of subsistence.

For Binford:

> [I]n one respect they are all members of a single class: they do not organize themselves to control food production through strategic modifications in the organization of the ecosystems they exploit. This distinction ... does not isolate a unit, such as a species or a subspecies that has clear niche implications. As a class ... hunter-gatherers encompass a variety of niches compared to the range observable in non-human animals *[CFR, p. 116].*

In *HGIP* the definition of hunter-gatherer is similar, but on a different phenomenological scale: "In essence, hunter-gatherers exercise no deliberate control over the *gene pool* of exploited resources" *(HGIP* p. 2, emphasis in original). Panter-Brick et al. acknowledge this definition creates problems with ambiguous cases, but argue such cases are relatively rare. They follow Hunn and Williams (1982) who examined the relative dependence on agriculture of 200 forager societies, and showed that there were very few societies that had a 5-45 percent reliance of agriculture. However, Smith (2001) contends that now-extinct groups may have filled the gap and that by insisting that groups be classed as either hunter-gatherer or farmer, we mask considerable and important variation. We return to this below.

Kent and most of her contributors focus on organization and worldview as much or more than they do directly on subsistence. Their views parallel those of Lee (1981) and Lee and Daly (1999b). Hunter-gatherers as a class are defined first, but not completely, by foraging (exclusive reliance on nondomesticated organisms), second by certain organizational properties (living in bands, relative egalitarianism, mobility, mobility patterns of dispersion and aggregation, and a common property regime), and third by a common cultural ethos marked by sharing and belief in a giving environment and an animated cosmos. Kent distinguishes non-egalitarian foragers (e.g., Northwest Coast) from egalitarian ones, claiming the latter are set apart by mobility and cultural flexibility, a flexibility that could also be termed resilience (Ames 1981; Holling 1973). It is this flexibility or resilience then that produces the great variability among egalitarian hunter-gatherers in southern Africa. She suggests similarly organized people are found worldwide. They are in fact what are sometimes called "generalized hunter-gatherers" or

foragers (Kelly 1995), immediate return foragers (Woodburn 1980), generic hunter-gatherers (Binford *CFR),* or the OAS (Original Affluent Society-Rowley-Conwy *HGIP).* Kent (Introduction, *CDTCF)* thinks non-egalitarian (complex) hunter-gatherers are more appropriately classed with horticulturalists and agriculturalists based on their organization (see also Lee 1981). In effect, she restricts the class "hunter-gatherer," which is still highly variable, to those foragers who are egalitarian and organizationally flexible.

Rowley-Conwy *(HGIP)* makes a crucial point about temporal change in hunter-gatherer societies important to inject here. Changes, even those persisting for long periods of time, are not inevitably irreversible. People can become sedentary, then mobile, and then sedentary again (e.g., Habu 2002); people may plant cultigens, stop, start, stop again. For the purposes of this essay, I call this pattern the Rowley-Conwy effect *(RCE);* it is important to what follows.

Subsistence Variation

Many subsistence economies commonly labeled as "hunter-gatherer" do not fit tidily into the distinction hunter-gatherer vs. farmer. These are economies that rely heavily on wild resources but that may also maintain small gardens, or trade wild produce for cultigens, or keep goats. The peoples of the Northwest Coast are among the world's most famous hunter-gatherers. However, in some places at least Northwest Coast peoples maintained gardens (Deur 2000), manipulated perennial plants to increase productivity of their stands (e.g., Darby 1996; Peacock and Turner 1999), set fires to control seral succession (Boyd 1999), and so on. Some argue these practices are a sort of farming (e.g., Marshall 1999; Onat 1997). While I do not agree that this is farming, it is difficult not to see these practices as strategic manipulations of and changes in ecosystem organization, or as the deliberate manipulation of the phenotypes of organisms, though perhaps not genotypes. The immediate point here is that such societies may have once been far more common than they are now. Known ancient examples include at least Middle, Late, and Final Jomon; Woodland; and Early Natufian among others. They represent subsistence economies that we must understand, but which are accessible almost exclusively through archaeology.

Smith (2001) tries to deal with this ambiguity by arguing there is a long and complex continuum between "pure" hunting-and-gathering and agriculture. He divides up this continuum by distinguishing first between food-procurement societies and food-producing ones, the former corresponding to "classic" or generalized hunter-gatherers. He subdivides food production into three categories: low-level food production without domesticates, low-level food production with domesticates, and agriculture (presumably high-level food production). One implication of the *RCE* is that some subsistence systems will shift between or even among these categories through time. These fluctuations may compound the confusion if they happen more quickly than archaeology can measure.

While I am not enamored with conceptualizing this variation as a continuum, or with the label "low-level food production," I agree with Smith that there is or was a great range of ancient economies not represented in the modem and recent sample of hunter-gatherer economies, posing a significant problem for our understanding of the evolution of subsistence and social systems, and that we lack the theoretical frameworks with which to conceptualize these extinct systems. *CFR* is Binford's attempt to build those frameworks. I am not sure he is successful in terms of these extinct economies. A further implication of both the existence of subsistence systems outside the ken of the modem ethnographic sample and the RCE is that the subsistence variation at the heart of the historicists' critique in southern Africa may actually be the norm for hunter-gatherer subsistence economies during the entire Holocene.

Organizational Variation and Evolution

During the past 20 or more years, the evolution of social complexity among hunter-gatherers has been a major focus of hunter-gatherer studies among archaeologists. While complexity was originally defined as a set of traits that were thought to co-occur (Price 1981), "complex hunter-gatherer" was, in a sense, a residual category containing those hunter-gatherers who were not generalized hunter-gatherers. They had larger, more dense populations; they tended to be sedentary (or at least not as mobile); they practiced storage; they had intensive economies and differentials of wealth, prestige, and status. Some were stratified. At one time, the soci-

eties of the Northwest Coast, a few in California, and the Calusa of Florida were thought to be the only such societies in the past or the present. As such, they were easily explained away. More recent research shows such societies were much more widespread in the distant and not-so-distant past. Their study addresses such fundamental questions as to why humans live in societies marked by permanent inequality.

Some recent thinking (Boehm 1999; Diehl 2000), drawing on evolutionary psychology and similar ideas, challenges the deeply held notion that egalitarian societies ever existed, or, if they did, that they represent the "natural" state of human groups. Put another way, both equality and inequality require explanation. One currently widespread theory is that egalitarianism is a social response to very high-risk environments, such as those of the Pleistocene (e.g., Hayden 2001; Richerson and Boyd 2000). Inequality arises then as a consequence of the appearance of more-productive and/or less-risky environments. Binford ridicules this notion, using his data to show there is no relationship between permanent leadership and environmental productivity as he measures it. He argues instead, following Panowski (1985), that permanent leadership and strength of inequality relate positively to diet breadth.

The concept of "complex hunter-gatherer" is also undergoing some recent revision and challenge. Price (1981), who coined the phrase "complex hunter-gatherer," now questions their existence, suggesting that most known examples are the consequence of contact with agriculturalists, and, when complexity did evolve autochthonously, it did not last long (Price 1995a, 1995b). He is wrong on both counts (e.g., Ames and Maschner 1999). He also calls for a more rigorous definition of complexity, although such rigor may be difficult to achieve (Lansing 2003). Arnold (1996) has put forth one such definition, limiting "complex hunter-gatherer" to those groups with permanent ranking or stratification and proposing we call groups that possess other traits usually linked to complexity, such as sedentism, storage, large populations and the like, "affluent foragers" (e.g., Koyama and Thomas 1981).

Rowley-Conwy grapples with some of these issues in his *HGIP* paper. He classes hunter-gatherers using only two dimensions: mobility (whether they are foragers or collectors) and whether they have property or not. As do many researchers (e.g., papers in Fitzhugh and Habu 2002), he defines complexity minimally as having logistical mobility patterns. However, as noted, Rowley-Conwy is primarily interested in showing that hunter-gatherer social evolution does not follow common expectations about social evolution (i.e., a smooth and irreversible progression from simple to complex). He maintains that over the long term, change among hunter-gatherers is unpredictable, reversible, and rapid (the *RCE),* suggesting patterns of change analogous in form to punctuated equilibrium (e.g., Gould 2002) although he does not invoke the same causes of change (see Prentiss and Chatters 2003 for an explicit archaeological application of punctuated equilibrium to hunter-gatherers). His is a valuable paper, arguing that developments such as sedentism widely thought to trigger increases in complexity and to be irreversible are sometimes neither. This appears to be a point requiring regular repetition.

Binford addresses the issue of complexity very differently from almost all current discussions. He distinguishes between social complexity (ranking, etc.) and systemic complexity. His discussion draws on Johnson's (1982) distinction between sequential and simultaneous (vertical) hierarchies, using the distinction to explore how groups of foragers can be organized in small, egalitarian groups yet integrated into larger, complex economic and social groups operating at regional scales through institutions such as sodalities, secret societies, production specialization, trade, and the like (this is his rebuttal of the historicist critique, although that is not actually very clear in *CFR).* This kind of complexity, he argues, arises when groups are initially heavily reliant on plants. Ranking (internal complexity) occurs in groups that become heavily dependent on aquatic resources.

His discussion of multiple forms of complexity is a useful corrective to the almost obsessive focus on permanent inequality in the complex hunter-gatherer literature, including some of my own papers, in that it highlights sequential hierarchies and important issues in the development of regional systems of interaction that may be social and economic mosaics. Hunter-gatherer landscapes are generally treated as containing only foragers or collectors, for example. Some landscapes might have

both simultaneously as well as different subsistence systems. Kent's sketch of the southern African cultural landscape (Introduction, *CDTCF) gives a* wonderful portrait of just such a mosaic.

Binford's use of complex adaptive systems in his discussion of cultural complexity is less successful. He invokes the concepts of emergence and self-organized criticality in his analyses of intensification and complexity, but in a seemingly *post hoc* manner. He also does not develop the notion of social complexity itself as possibly the result of self-organization as a process. The idea seems implicit to much of his thinking and is congenial to his strong preference for endogenous causal processes at the system level. He uses these concepts to make predictions about the tempo and form of culture change.

The Causes of Variability

Kent and most of her contributors suggest that variation among forager societies arises from behavioral flexibility that is a consequence of hunter-gatherer social organization and worldview in specific environmental and historical contexts. To my mind, this begs a question to be addressed in the next section. However, some of the apparent variability might be an artifact of the scale at which it is analyzed in *CDTCF.* Analyses are fine-grained: behavior is often described at the level of individuals, families, or small groups. This scale of observation can maximize heterogeneity and mask similarities.

Binford approaches variability from a global macroscale and with his well-known disdain for historical explanation. Ultimately he argues the variability we see in economic and social organization derives in large measure from five factors: (1) local environmental conditions, (2) the initial conditions of evolutionary trajectories (primarily whether groups were principally reliant on terrestrial animals, collecting plants or harvesting aquatic resources), (3) available technology, (4) demographic packing, and (5) subsequent intensification of food production. He claims that organizational and economic variation increases markedly after groups cross the packing threshold and experiment with different responses to the resulting stresses. Thus, when we see strong variation, we are seeing groups above the packing threshold. He further maintains we cannot extrapolate from fine-grained analyses such as those in *CDTCF* to broad-scale patterns. These patterns are emergent phenomena and therefore are not necessarily predictable from previous local events. For example, intensification of food production as a process is marked by self-organized criticality; when changes occur locally, they happen swiftly and in unpredictable directions (his argument, at this point, would appear to parallel Rowley-Conwy's). However, this appearance of disorder is scalar. While at a local level the course of intensification is unpredictable (multilineal), at a higher level it really has only two outcomes: increased reliance on aquatic resources or agriculture.

Deeply implicit in here is the same scalar issue that has roiled paleontology for the past 30 years: is variation/change at the macrolevel of lengthy cultural sequences the result of the accumulation of microlevel variation and changes (a la *CDTFC),* or are there different macro- and micro-level processes. The bulk of the discipline would answer yes to the first question (e.g., Boyd and Richerson 1992) and no to the second. To my mind, the questions are open.

What's the Appropriate Paradigm, If Any?

When Sue Kent invited this essay, she asked that it somehow address the future of hunter-gatherer studies. That task is impossible, at least for me, since there are many possible futures. However, I suggest that stagnation and irrelevancy is one strong possibility. As with the rest of anthropology, hunter-gatherer studies have no single, coherent paradigm (or even common assumptions) with which to explain behavioral variability among hunter-gatherers or to set common research priorities. There are, instead, multiple sometimes partially overlapping approaches, ranging from strongly materialist to strongly postmodernist, from strongly scientific to strongly anti-science. The edited books include a range of these approaches, although lacking the strongest postmodernists. As a consequence, they, and the entire field, lack an elemental coherence and consistency. There is also incoherence because of the very strong particularism of much current work. This is not to deny the importance of particularistic studies; they are essential. However, the ultimate outcome of particularism in the absence of a framing paradigm is the accumulation

of more detail without a clear purpose or point. It is the absence of such a paradigm that will ultimately lead to stagnation and irrelevance.

I would maintain the only option is a scientific, materialist paradigm and that it has to fall within what Durham (1990) broadly defines as "evolutionary culture theory," which requires attention both to particular histories and to process. I think, in the long run, there is no alternative, despite current critiques (e.g., Bamforth 2002). I illustrate the necessity for a materialist framework by discussing what I see as a crippling irony at the heart of some of the notions about hunter-gatherers developed in Kent's book, the *Cambridge Encyclopedia,* and elsewhere.

If, as Kent and others maintain, modem foragers are distinguishable from even small-scale farmers because of their subsistence, social organization, and ethos, it must be asked how this combination of traits arose, particularly if it is shared by peoples spread widely across the globe, as is claimed. The possible explanations are limited: (1) historical coincidence, i.e., the coincidental outcome of contingent and unconnected local events; (2) innate cognitive tendencies common to all humans; (3) cultural descent or continuity from a remote common cultural ancestor, or (4) parallel or convergent evolution producing a common set of behavioral and organizational traits among humans not culturally related, but sharing some sort of common circumstances. (A fifth alternative is there are no common traits and to think otherwise is error. If that is so, we are at a dead end.) Possibility 1 seems enormously unlikely. It is also the only nonmaterialist option. Evolutionary psychology makes explicit claims for number 2 as does evolutionary ecology but less expansively. Thus, human cognition and decision-making tendencies rest on an innate base that evolved during our hunter-gatherer past in the Pleistocene and persisted to the present. Almost everyone would probably now reject number 3, which requires cultural continuity and stability spanning tens of thousands of years linking groups across equally vast spaces (this not to deny that there are extant hunter-gatherer cultural traditions with deep antiquity). To invoke it, one must explain the continuity. In both numbers 2 and 3, the shared traits are homologies, resulting from descent from either a common biological or cultural ancestor. Number 4 proposes that the similarities that Kent, Lee, and Daly (1999b) and others see among modern foragers are analogies, i.e., similarities due to common functional problems (e.g., bird wings, insect wings). These analogies (and here lies the irony) must include not only the similarities in subsistence, but in social organization and ethos as well. What might these common functional problems be?

It is generally thought that foraging evolved during the Pleistocene as a response to the high subsistence risk caused by rapid and extreme climatic and environmental changes (e.g., Richerson and Boyd 2000). Kuhn and Stiner *(HG1P)* argue that hunting and gathering as practiced by modern humans evolved in the Upper Paleolithic. Their conclusions parallel Gamble's (1999). This does not preclude that some of the kinds of cognitive abilities and decision-making rules proposed by evolutionary psychology and evolutionary ecology could not have evolved much earlier. Some aspects of human food-getting behavior extend back into the early Pleistocene at least (e.g., Smith 1999). It does suggest, however, that what we understand to be full hunting and gathering appeared with or after the evolution of anatomically and perhaps even behaviorally modern humans and evolved during the Late Pleistocene. This would be the earliest we might see the combination of traits. Gamble (1999), Dunbar (2003), and others are sufficiently convincing to suggest that the capacity for a hunter-gatherer ethos did not exist until the Upper Paleolithic. Indeed, their evidence suggests that the combination of flexible yet durable social ties and high mobility may not have been cognitively possible until then. If we argue for biological continuity (option 2) or cultural continuity (option 3), we are still explaining the core hunter-gatherer traits as the result of material conditions, although remote in time. In either case, we are faced with explaining their persistence and continuity. My own thinking is that the combination of subsistence and cultural traits is real and results jointly from options 2 and 4.

It is only in the much more stable environments of the Holocene that agriculture and complex societies could develop (Richerson and Boyd 2000; Richerson et al. 2001). If forager organization and worldview evolved multiple times in multiple places independently during the Holocene and within the last few hundred years, it follows that

there existed local circumstances structurally similar (though not necessarily climatically similar) to the Pleistocene, requiring similar functional responses. Among these would be the dynamics of the modem world system (Wolf 1982) invoked by the historicists. In this instance, high-risk environments included particular social, economic, and political ones.

I have not attempted to support my claim that evolutionary culture theory is the best way to suppose hunter-gatherer variability. For that I recommend the reader to Bettinger (1991), Durham (1991), Kelly (1995), O'Brien and Lyman (2000), Shennan (2002), and Wilson (1998).

Final Comments

The publication of *CFR* is a signal event deserving separate comment, given Binford's stature and the sheer reach and ambition of the book. *CFR* is a major achievement. Does it help us to better suppose hunter-gatherer variability, and to conceptualize variability beyond what we currently know? The answer to the first question is a definite yes, but its generalizations require rigorous testing and replication, the normal scientific process that Binford himself has advocated for four decades. His data sets (presented in tables in *CFR)* need to be available via the web or CD so his variables and formulae can be readily examined, manipulated, and used by others. Many of his generalizations are only testable archaeologically, if for no reason than the archaeological record is now the only independent data set.

The answer to the second question, does it help us to suppose variability beyond what we currently know, is less clear cut. His overarching theory remains not much more explicitly developed than I have sketched here, although much is implicit. The utility of complex adaptive systems (CAS) theory is an open question despite stimulating applications (e.g., Bentley and Maschner 2003; Kohler and Gumerman 2000). Merging it with current evolutionary theory is a work in progress (e.g., Depew and Weber 1997; Kauffman 1993; Lansing 2003) with an unclear outcome. He contributes relatively little to that effort, although it is woven implicitly throughout. On the other hand, he raises important issues about micro- and macro-phenomenological levels, processes, and emergence that have plagued anthropology since Kroeber (1917) published the concept of the superorganic.

The answer is also less clear-cut because people may not read *CFR* and test its ideas. The book is big; it is difficult, in ways irritating and frustrating. It may not fit well with a professional culture increasingly used to receiving and disseminating information electronically. The errors may lead some to dismiss it entirely. People may use it only as a source of convenient interpretations ("according to Binford 2001"), or as a strawman ("once again Binford falls prey to..."). That would be too bad because its value lies in the opportunity to productively engage with it and test it.

For example, Binford concludes that aquatic economies are the result of intensification, the path of intensification depending on whether the initial economy was primarily dependent on terrestrial plant or animal resources. There are early aquatic economies along the Pacific Coast on North America that do not seem at first blush to fit this prediction. They range from fully maritime foragers on Kodiak Island (Fitzhugh 1996) to groups exploiting a mix of terrestrial plant and aquatic resources in southern California (e.g., Erlandson 1994) all with apparently very low population densities probably well below the packing threshold. However, testing his ideas would be extraordinarily productive, even if he is wrong. One does boggle at the prospect of building an archaeological frame of reference comparable to his ethnographic ones. It will also be extremely difficult to archaeologically operationalize such things as the packing threshold. Archaeological testing will have to be done comparatively, using multiple, spatially large-scale, temporally long sequences.

Finally, I said at the beginning of the previous section that it is impossible to forecast the future of hunter-gatherer studies. However, I do know that they and we will be considerably impoverished by Sue Kent's untimely death. She was a person and scholar of the first water. She will be sorely missed.

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