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ARCHAEOLOGICAL APPROACHES TO HUNTER-GATHERERS

Robert L. Bettinger

Department of Anthropology, University of California, Davis, California 95616

Archaeology has probably contributed more to our knowledge of hunting and gathering peoples than any other anthropological subdiscipline, yet it is difficult to define an approach to the study of hunter-gatherers that can legitimately be termed archaeological. Apart from a battery of specialized techniques and tactics (none exclusive to hunter-gatherer research), there is little that separates archaeologist from cultural anthropologist where hunter-gatherers are concerned and many scholars move back and forth between the study of living and extinct hunter-gatherers without perceptible difficulty. If the concept of an archaeological approach to hunter-gatherers has merit, perhaps it is in the archaeologist's recognition that most of the world's hunter-gatherers are dead, that they must be studied by excavation rather than interview, and that all this has some bearing on the kinds of theories in which students of hunter-gatherers ought to be interested. The archaeologist believes that theories about hunter-gatherers ought to have some chance of being tested in the ground since that is where most of the material they purport to explain currently resides.

Because limits on space preclude a comprehensive survey of the field below, other contributions (e.g. 2) better convey a sense of the breadth of recent hunter-gatherer research in archaeology. My purpose here is more specific: to examine recent contributions to archaeological research explicitly derived from *theories of limited sets;* by this I mean theories that speak to limited sets of behaviors. In contrast to theories of general sets, i.e. general theories, which are by nature highly abstract and sometimes difficult to grasp, theories of limited sets are by design practical and intended for application in the real world: they are theories that have, in archaeological parlance, direct test implications. A detailed treatment of the relationship between general and

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limited theories in anthropology and a critical review of the various conceptual schemes that might serve as a general theory of anthropology are presented elsewhere (R. Bettinger, in preparation).

Further to narrow my present topic, I am particularly concerned with two limited theories that have dominated hunter-gatherer archaeology over the last decade: (*a*) middle-range theory (10, 16, 111) and (*b*) microeconomic (or optimal foraging) theory (102, 121). I introduce my review of these two limited theories, and of their broader relationship to general theory, with a short history of hunter-gatherer research. This lays the groundwork for subjects subsequently addressed and establishes a fundamental point: Anthropology has always portrayed hunter-gatherers in ways that serve its theories, and in this sense hunter-gatherer research has always had an important theoretical component.

HUNTER-GATHERER THEORY 1900–1975

When they have engaged in theory, students of hunter-gatherers have historically favored theories that are comparative, evolutionary, and materialist. Within this traditional perspective, however, one can distinguish two very different models of hunter-gatherers. The first, which held sway in anthropology from the turn of the century to the mid-1960s, depicted hunter-gatherers as primitives. Describing them in a string of negatives (cf 49), it emphasized the things they lacked that more advanced cultures would later have. This portrayal was materialist in its suggestion that uncertainties of subsistence explained primitive development in other aspects of culture; it was evolutionary in its reliance upon the concept of developmental progress—the Social Darwinist model of cultural evolution anthropology has embraced from its beginings (e.g. 70, 113) to the present (e.g. 37, 40, 97, 117; cf 33). In this model, of course, hunter-gatherers represent the least-developed stage of culture.

The model of "hunter-gatherers as primitives" collapsed during the 1960s following a spate of ethnographic studies among a variety of groups, mostly in Africa and Australia (cf 63). This research exposed what seemed basic flaws in the existing model of "hunter-gatherers as primitives." Contrary to expectation, hunter-gatherers did not die young nor did they constantly wage war against Nature and starvation. Instead, they ate well, lived long lives, and seemed to have plenty of spare time (61, 122).

With the collapse of the model also collapsed the basis for explaining hunter-gatherer behavior: Backwardness and ignorance of helpful innovations could no longer account for what had previously been interpreted as primitive traits among hunter-gatherers. To fill this gap, hunter-gatherer specialists turned to contemporary anthropological theory and especially to the schools of "new ecology" and neofunctionalism (115; cf 72), which, in turn, bor-

rowed heavily from contemporary biological ecology (e.g. 60, 75, 123). Not coincidentally, neofunctionalism shared key themes with previous hunter-gatherer theory—most importantly a stated interest in cultural evolution (114) and an unshakable bias in favor of materialist explanations—that only a few (e.g. 90) were willing to abandon. To this neofunctionalist matrix was added a preference for research strategies ultimately derived from traditional cultural ecology (cf 104–106), which emphasized the importance of understanding local environment (cf 30, 63).

In the model that emerged, hunter-gatherer behavior was firstly rational and adaptive (e.g. 36). It was secondly group oriented; most versions of this "hunter-gatherer as lay ecologist" model relied in part on group selection or what my colleague William Davis terms "methodological collectivism"—the idea that human behavior originates and is justified at the level of groups rather than at the level of individuals. Hunter-gatherer behavior was thirdly homeostatic: Consciously or unconsciously all human behaviors had an underlying systemic logic—that of sustaining long-term internal group equilibrium.

Both more satisfying and plausible than the one it replaced, the ecological model of hunter-gatherers was hardly much better when it came to matching theory with field observation; it was, in fact, something of an operational disaster (cf 38). Its emphasis on "systems" resisted efforts to specify what its basic units were, and hence what were the loci of relevant cultural behaviors or levels of cultural selection and evolution (72); nor did it indicate how one might go about identifying "adaptive" behaviors. In short, it was not always clear who or what was acting or to what purpose (cf 79). This was made all the more confusing by the tendency of neofunctionalists to seize on celebrated oddities of culture (e.g. 80) and through long, weakly verified causal chains construe these as in some sense adaptive (see 76 for a general discussion of the problem of neofunctionalist explanation).

It is arguable, in retrospect, that the major advantage of this "hunter-gatherers as ecologists" model over the "hunter-gatherers as primitives" model it replaced was vagueness. As a theory it was more abstract and remote and therefore considerably more difficult to refute. It was simply easier (and more compatible with mid-1960s social thought) to reject the notion that hunter-gatherers were backward and economically deprived than it was to show that a particular cultural behavior was utterly without adaptive value. Moreover, with its basic concepts of homeostasis and system maintenance, neofunctionalism was easily accommodated within the broader and increasingly more popular theoretical framework of the "New Archaeology" (22, 35), which stressed transformational systemic change and progressive evolution by ideal stages (33; cf 118, Fig. 120). This explains the speed and ease with which neofunctionalism took hold in hunter-gatherer studies as "new" archaeology replaced "old" archaeology.

Discussion

If by the middle 1970s hunter-gatherer archaeology had become somewhat more attentive to theory than previously, its theory was no more materialist, evolutionary, or comparative than before. The field had grown noticeably more sophisticated in its consideration of the environment and of man-land relationships, however. In this respect research was demonstrably different (and from current perspectives, more advanced) than under the earlier "hunter-gatherers as primitives" model. Unhappily, hunter-gatherer archaeology was only slightly better at explicitly matching theory and fact than in earlier times; indeed, as archaeology generally had moved away from culture chronology, the match between theory and fact had seemed to grow worse. Confusions and contradictions within neofunctional theory further compounded this problem.

Therefore among all the possible areas into which hunter-gatherer archaeologists might have channeled their efforts during the late 1970s and early 1980s, the enterprise deemed most worthy of attention by many (e.g. 18, 51; cf. 2) was that of developing better links between theory and data through theories of limited sets. This responded to a growing awareness that the testing of general theory required more informed arguments regarding just what archaeological data were all about (26, 86, 92, 94). And given the theoretical disarray within neofunctionalism, which had driven some to abandon materialist theory completely (e.g. 90, 91), hunter-gatherer archaeologists seemed only too willing to take this path and forsake the study of higher level processes—and even to distance themselves consciously from the study of these processes. As we shall see, this has fostered some unfortunate misconceptions about the need for general theory in hunter-gatherer research.

Theories of Limited Sets

It is the job of theories of limited sets to reconcile general principles to particular cases by showing how such cases result from the general principle in the presence of special conditions. The presence of such conditions is both necessary and sufficient to identify a case as belonging to the set for which the theory is intended, such classifications being, thus, theory based. It is, further, the interaction between the general principle and the salient properties of the special, set-defining conditions that accounts for what is observed. Theories of limited sets can serve as either interim steps in the construction of general theories or means of articulating extant general theory (59). Either way, theories of limited sets are no less "theoretical" than theories of general sets—they are simply less general.

As I have said, the current preoccupation of hunter-gatherer archaeology with theories of limited sets is partly traceable to revelations rising from ethnography (e.g. 63) and to the rise of the New Archaeology. In both

instances it was all too clear that little was known about hunter-gatherer lifeways and that even less was known about how such lifeways might reflect themselves in the archaeological record (e.g. 8). The field of "ethnoarchaeology" (23, 42, 58) resulted directly from this recognition. The information that has been generated in this field over the last few years is staggering—certainly far too extensive to discuss here. The general area that has come to be known as middle-range theory and research (10, 16, 108, 111), however, has gained special importance in contemporary hunter-gatherer archaeology.

MIDDLE-RANGE THEORY AND RESEARCH

At risk of getting off on the wrong foot, I must note in the recent literature a good deal of confusion over just what "middle-range theory and research" is. According to Binford it addresses "a) how we get from contemporary facts to statements about the past, and b) how we convert the observationally static facts of the archaeological record to statements of dynamics" (10, p. 6). For Thomas (108, p. 396) its job "is to bridge the gap between the known, observable archaeological contexts and the unknown, unobservable systemic context. . . . mid-range theory is necessary to provide relevance and meaning to archaeological objects." Likewise, for Thomas it "seeks invariant linkages between the archaeological record and the behavior that produced it" (111, p. 245). For Grayson (44, p. 77) it is research that emphasizes the study of extant systems as a means of understanding process relevant to the formation of the archaeological record. For Raab & Goodyear, who introduced the term (cf 93) as a borrowing from sociology and the works of Merton (e.g. 68), "middle-range theory is seen as providing a logical link between relatively low-order empirical generalizations and comparatively high-order theories" (84, p. 257).

Whether these interpretations differ significantly depends on how one views recent archaeological research dealing with hunter-gatherers, particularly the part most heavily influenced by the writing and ethnographic research of Lewis Binford, whose efforts in this area inspired its current popularity. The underlying framework for much of this work is contained in his proposals regarding hunter-gatherer systems (15). This is an elegant theoretical construction in which variability in natural environment dictates differing kinds of social, economic, and settlement organization.

Foragers and Collectors

Basic to the forager-collector model is the proposition that where access to resources is limited temporally or spatially a variety of measures will be taken to extend the economic utility of those resources (9, 13–15); absent such limitations, these utility-extending behaviors are unlikely. More particularly,

in environments that are spatially and temporally homogeneous (e.g. the tropics), there are no seasons of relative shortage and hence little intrinsic need to store or save resources beyond those needed for immediate consumption (see also 57). Under these conditions, hunter-gatherers follow the pattern first described by Lee (62; see also 41) in which a group exploits the resources within a two-hour walk (site exploitation area) around a residential base and then moves its residential base to a new unexploited area. As there is little seasonal and spatial variability in resources, residential bases contain essentially the same kinds of things; they differ principally in size, this depending on the number of occupants and length of occupation, which in turn depend on the productivity of the local environments within which they are located. In such a system, a *foraging* system, the population is said to "map" onto resources, moving its residential base to accommodate easy access for individuals. There is, in short, no incongruity (mismatch) between the distribution of natural resources and the distribution of the population.

It is different where resources are spatially and seasonally heterogeneous. In particular, periods of resource shortfall (e.g. winter on the North American Plains) cause temporal incongruities between distribution of population and resources that require some form of compensation—minimally, storage. Storage, in turn, means that some resources, often those available in abundance for short periods, must be obtained in bulk (i.e. beyond current needs) and preserved for later use. Both responses demand advance planning and sophisticated organizational tactics.

Note further that while storage solves temporal incongruities in the distribution of resources relative to population it creates spatial incongruities between the two since places where resources are stored act increasingly to anchor populations and thus inhibit their ability to shift residence to match the distribution of resources; storage freezes populations at spots of accumulation (e.g. 106, pp. 82, 232, 233). Spatial heterogeneity, or patchiness, in the natural distribution of resources (e.g. across discrete biotic communities) causes further incongruities between these resources and the populations that would exploit them. Such distributions defy any easy resolution of spatial mismatches between populations and resources by means of residential movement because travel toward one resource is often travel away from another of equal importance available at the same time. That resources must be obtained in large quantities to offset seasonal shortfalls worsens these spatial incongruities.

Collector systems respond directly to these circumstances. Storage of resources is, of course, the most obvious response. Strategies of exploitation are then geared toward the procurement of specific resources in bulk during short periods, often by groups rather than individuals. Technology is organized so that, like food, equipment is *cached* (15, 109, 110), since much of it

is used only for limited periods and only in specific places. This equipment is said to be *curated* (i.e. saved in anticipation of future use) since it is made and subsequently saved with reference to future needs of very specific kinds and, having been put to use, is again saved for use in the future (14). Further, that collectors must obtain staple foods in bulk during short periods demands greater technological efficiency. This is often reflected in the development of sophisticated and efficient gear that can be difficult to make and therefore once made is carefully saved (112; but see 1).

The organization of procurement in collector systems is likewise distinctive in that the relationship between population and resources becomes more structured: Task groups move from centrally located residential bases to obtain specific kinds of resources and return with their take. The forager responds to temporal and spatial resource shortage by residential movement; the collector responds to temporal shortage by storage and to spatial mismatches between population and resources by logistical mobilization—the moving of specialized task groups to given locations. In combination, the temporal and spatial variability that typify collector systems are reflected by greater variability within given settlements (reflecting the diversity of activity) and greater variability between different settlement categories (reflecting seasonal variation and the logistical organization of procurement). Binford sees these differences as having direct consequences for interpreting the archaeological record (15).

The Myth of Middle-Range Theory

Apparently everyone agrees that the forager-collector continuum is theory and, moreover, that it is middle-range theory in the sense of Merton (cf 84). It begins from the general assumption that, ceteris paribus, environment is a—perhaps *the*—strong force in the shaping of hunter-gatherer adaptation: Where access to resources is temporally or spatially restricted, hunter-gatherers extend the utility of these resources by storing them (gaining temporal utility) and by caching both resources and gear (gaining spatial utility) (9, 13–15). It explains the organization of technology in terms of these circumstances and examines the archaeological consequences thereof. One gathers from the literature that whether this model of hunter-gatherer strategies is middle-range theory in the sense generally attributed to Binford (10, 16; cf 84, 111) is quite another matter. This is wherein lies the aforementioned confusion regarding the "true" character of middle-range research.

In current archaeological usage the term middle-range research roughly equates with the "analysis of site formation processes," an awkward neologism popularly defined as the systematic study of basic archaeological "structures" or (static) signatures (10, 16, 44, 111) that identify particular kinds of (dynamic) behaviors. Research commonly held to exemplify this branch of

inquiry would include the search for operational distinctions between settlement categories (e.g. 109, 110), between cultural and noncultural faunal assemblages (16), and between basically different "elementary structures" likely to be found in the archaeological record by means of understanding the organizational properties that affect hunter-gatherer systems (12–14, 19).

Raab & Goodyear fault this current emphasis on site formation on the grounds that "middle-range theory" ought be more broadly conceived. This is wholly immaterial. Processes of site formation are eminently subject to theoretical investigation. It is difficult to see in what way their study departs from the ideal Mertonian bridge between theory and fact or, indeed, from any workable definition of theory. If theory is a set of systematically connected propositions, empirically verified or potentially verifiable, about the underlying principles that govern relationships within some set of phenomena (cf. 89), then current interest in site formation certainly qualifies as a theoretical endeavor. Whether it is less general and therefore lower in level than Binford's propositions about foragers and collectors, and whether this would qualify it as "lower-level" theory or merely "lower middle-range" theory (cf 82) cannot be evaluated without invoking additional, more value-laden, stipulations about the number of phenomena governed and hierarchical relationships between them.

Of perhaps greater relevance, current studies of site formation are so intimately intertwined with more abstract notions about foragers and collectors that the two are effectively inseparable. Indeed, archaeologists may value the forager-collector dichotomy at least as much for its obvious and direct implications for interpretation of the archaeological record as for its contribution to theory. Binford, of course, is clear on this point. His disagreement with Yellen about the reality of "activity areas" in the archaeological record (12)—a classic problem of site formation—turns on his interpretations of the structured quality of collector systems in comparison to the relatively structureless quality of such forager systems as that of the !Kung San studied by Yellen (124). Likewise, his discussion of the gear carried by Nunamiut sledders and snowmobilers is not simple description (cf 9, 14) but seeks regularities along lines he anticipates would separate collectors and foragers.

Current middle-range researches in zooarchaeology (13, 16), which fall even more clearly under the heading of what Raab & Goodyear see as studies of site formation, show similar theoretical inspiration. Here, for example, patterns of Nunamiut butchering are explained in terms of the interplay between resource supply and resource demand as mediated by economic utility (see below). In this way Binford attacks the "traditional" wisdom that butchering is a cultural rather than economic activity, to show the superiority of a materialist perspective for interpreting the archaeological record. Thus,

variability in techno-economic circumstances, not culture or mental templates, dictates animal dismemberment (13). Likewise, when Binford attempts to distinguish culturally generated faunal assemblages from naturally (carnivore) generated ones (13, 16, 21), it is not merely an inductive exercise in which a series of assemblages of each kind are objectively compared and the differences duly noted. Indeed, Binford is emphatic on the point that middle-range (i.e. site formation) theory does not consist of empirical generalizations, inductive arguments, or ethnographic analogies (12). It proceeds hand in glove with some rather general assumptions about the factors that govern human butchering (i.e. what human butchers are up to) and the faunal patterns reflecting these factors; it is these patterns alone that make human faunal assemblages understandable at all and thus separable from natural faunal assemblages. That Binford's forager-and-collector paper (15) begins and closes with discussions of the processes that shape archaeological sites should dispel any lingering doubts about whether theories of site formation qualify as middle-range theories.

It will further clarify matters to dispose of the notion that middle-range research necessarily uses observations about the present to understand the past (15, 16, 18, 19, 44, 108). That at least one generally recognized contribution to middle-range research (52; cf 111) has nothing whatever to do with observation of contemporary dynamics shows that this is just not so. The confusion here evidently lies in the belief that because middle-range theory links dynamic systems (cultural and noncultural) to the static archaeological record, the development of middle-range theory requires firsthand observation of dynamic (and therefore contemporary and living) systems (18). As Binford (7) himself once noted, this would necessarily condemn us to remain ignorant of any dynamic process that for whatever reason might not be operant at observable levels among contemporary systems.

In emphasizing the need to observe dynamic systems directly, middle-range theorists seem to imply that the contemporary systems are of primary importance, which they are not. The processes and principles that govern dynamic systems (past and present) should be at the focus of concern. Firsthand observation of contemporary dynamic systems helps us to think about principles and processes, but the principles and processes transcend individual cases, however instructive these cases might be. It is, after all, this transcendent quality that gives special value to principles and processes when uniformitarian assumptions are made about the world. Put it this way: As hypotheses, would Binford's arguments regarding animal butchering be any more or less compelling were it to turn out that the Nunamiut never existed? Do analyses that apply these hypothetical principles of butchering (e.g. 110) depend on the logic of Binford's reasoning or the accuracy of his factual

observations? In short, as a source of hypotheses would we rather Binford be a good thinker or a good observer? For any logical positivist, this is no question at all: The merits of an hypothesis are necessarily independent of its origin (48); hence Binford the thinker is to be preferred. In short, we should pay more attention to Binford's logic than to the factual origin of that logic.

Discussion

In the last analysis it would seem that the current confusion surrounding definition of "middle-range archaeological theory" reflects flaws basic to the concept itself. If it is worth anything at all, a scientific theory must at least pretend to address a specific body of facts—either those in hand or those potentially obtainable. The real or anticipated patterning of these facts are the "signatures" through which the principles of interest express themselves; the principles are expressed via the phenomena that constitute the limited set. On these grounds I see no epistemological difference between mid-range theories (sensu Thomas; 111), middle-range theory (sensu Merton; 68, 84), and theories of the processes governing site formation (sensu Binford; 10, 16): All are simply theories of limited sets. Moreover, beyond these, no lower order of theoretical endeavor is conceivable. In short, if we limit ourselves to scientific enterprises, middle-range theory is, if not a myth, at least a misnomer, since anything of lesser scale ceases to be theory at all.

I do not mean to deny that there is something new and innovative about the conduct of hunter-gatherer archaeology today. We are now attending to matters of verification and meaning in a far more explicit and sophisticated manner. Yet this is not our first experience with the problem of archaeological verification or with how we assign meaning to the archaeological record. As Grayson (44) has argued, it is a mistake to believe that contemporary efforts to apply Binford's insightful models of butchering and of the forager-collector continuum to the real world are something more than the latest attempt to do hunter-gatherer archaeology under the umbrella of a consistent theoretical framework. One might go further still and argue that the similarities Grayson has noted between past and present middle-range research in archaeology have less to do with the special nature of archaeological problems than with the simple fact that both rest on theories of limited sets—as does all empirical scientific research. In sum, what is truly new about contemporary middle-range research is the innovative theorizing of Binford, not the matching of theory and fact—which has always been with us (e.g. 107).

A final reminder seems in order: For all the exciting avenues of investigation it promises, the "new middle-range theory" attends to limited sets of phenomena. At more abstract levels of analysis these phenomena are governed by different principles—by those that comprise theories of general sets. This means that to be fully operational archaeology must give at least as much

consideration to the linkage between theories of general and theories of limited sets as it does to the linkage between theories of limited sets and empirical patterns. The current version of middle-range theory in archaeology not only emphasizes the latter at the expense of the former but actively perpetuates the myth that middle-range theory can and ought to be intellectually independent of general theory (16, 111).

To forget that theories of limited sets are ultimately derivatives of theories of general sets makes it all too easy to believe that the phenomena we study are true and real—i.e. that they are objectively defined. In its search for invariant signatures of behavior—archaeological truths of the "if and only if" kind (16; cf 43, 111)—seems to have middle-range theory and research seems to have fallen prey to just this trap. Binford (18) to the contrary, that several alternative explanations can account for shifts in diet during the terminal Pleistocene is most assuredly not symptomatic of faulty middle-range theory; nor would improvements in middle-range theory eliminate such situations. Both true and false theories and hypotheses can have true implications (92). To claim that good middle-range theory would eliminate such indeterminacies only underscores the depth of misunderstanding surrounding the nature of middle-range research, the intimate linkages between theories of general sets and theories of limited sets, and the relationship of both to the scientific enterprise. These relationships are more openly recognized and dealt with in another thriving area of archaeological inquiry underwritten by a theory of a limited set: optimal foraging theory.

OPTIMAL FORAGING THEORY

Optimal foraging theory came to anthropology via biology (e.g. 64, 83, 95). It might as easily have been borrowed from economics since its principles constitute a central part of microeconomics, specifically the part that attends to the rational decision-making of individuals under a set of specified conditions that include limited resources and means and unlimited needs. The anthropological version of optimal foraging theory presumes that in certain arenas human decisions are made to maximize net *rate* of energy gain. These include choices about (*a*) diet or diet breadth (64), (*b*) foraging location or patch choice (64), (*c*) allocation of foraging time (24), (*d*) foraging group size (25), and (*e*) settlement location (50, 77). Several reasonably comprehensive reviews survey the range of optimal foraging applications in hunter-gatherer research (2, 102, 120, 121). Rather than cover that ground again, I have chosen to convey the general sense of this family of models by referring to the model of diet. This is easily the best-known and most commonly applied optimal-foraging model in anthropology.

An Optimal Model of Diet

In the basic model of diet breadth, foragers confront an array of items—food or prey. Several factors with respect these items may vary, including abundance, amount of energy produced per item, amount of energy needed to acquire energy from each, and amount of time needed to acquire that energy once the item is selected. The problem for the forager is to select the combination of these items that maximizes net energy intake per unit of foraging time—i.e. the amount of energy captured less the amount of energy expended during the period spent foraging. It is assumed that (*a*) barring exceptional circumstances, the amount of time and energy needed to extract energy from a given item of a given kind, and the amount of energy it produces in return, are fixed; (*b*) decisions resolve contingencies consisting of choices between momentary rates of return (alternatives resulting in higher rates are chosen over those that result in lower rates); and (*c*) choices of food types are qualitative and complete rather than partial (an item is either in the optimal set or it is not).

To solve a problem of this kind we first ask what would be rational in the best of all possible worlds. Since we have assumed that extraction costs and energetic benefits are fixed, the only possible room for variation is in the relative abundance of items of each kind. Given these assumptions, the best (and simplest) case is the one in which all kinds of all items are infinitely abundant—i.e. so abundant there is no need to search for them at all. How we choose among the various items then turns exclusively on the energy yield of each relative to its time and cost of extraction—i.e. its energetic return less its energetic cost of extraction divided by the time expended in extraction. Here, the logical choice is to select only the kind of item yielding the highest net return per unit of extraction time; this gives us the highest possible energy intake rate under these best of all possible circumstances.

Given the circumstances above, what would be our best option if we were for some reason required to exploit at least two kinds of items? Clearly we should then select the combination of the kind of item with the highest rate of return and the kind with the second highest rate of return. All other resources have lower rates of return, and thus any combination including them would result in a momentary return lower than that resulting from the combination of the first and second kinds. Resource items can thus be ranked by energy return per extraction time, and with these assumptions, no rational forager should pass over a more-rewarding to exploit a less-rewarding item. The use of a low-ranked resource thus implies that the forager uses all higher-ranked resources when available.

In the real world, of course, nothing is infinitely abundant. All resources must be sought out prior to their exploitation. Calculation of our momentary energy return must thus include time (and perhaps energy) spent searching for

items. We must reevaluate our selection of resources because the resource we would prefer to exploit (because, once found, its rate of energy return is highest) may be so rarely encountered that to seek it exclusively would result in a very low overall rate of return.

Consider, then, a forager seeking the highest-ranked resource available to him. (His rate of return can be calculated by subtracting the energy it takes him to find this resource and extract its energy from the amount of energy this generates, and then dividing by the time involved.) Suppose further that in the course of his search the forager encounters an item that ranks second to the one he seeks. Whether the forager halts his search for the first-ranked resource to extract the energy of the second-ranked one depends on whether the rate of return for the latter activity (which now includes only the time and energy that would be expended in extracting its energy) is greater than the rate of return for continuing to seek and exploit only the first-ranked resource. This captures the sense of what is meant by the term "contingency model" (96). The forager decides between two courses of action on the basis of rates of energetic return applicable at the very moment the decision is made. Likewise, this example shows how exploitation of a given resource is independent of its abundance. The forager's choice depends only on the rate of return for extracting a found item's energy (i.e. excluding the search phase) relative to the rate of return when only resources of higher rank are sought and exploited (i.e. including both search and extraction phases).

One implication of this model is particularly important: As overall resource abundance declines, search time increases, and the breadth of diet generally increases in compensation. Under no circumstance does breadth decrease as a response to scarcity. Conversely, selectivity is the appropriate response to increasing resource abundance.

Applications

Little known to anthropology in 1980 (cf 2), optimal foraging theory has gained rapid and wide acceptance and currently competes on an even footing with middle-range theory and research in terms of popularity. It fully deserves this attention as it has guided many insightful studies of contemporary hunter-gatherer behavior (45, 46, 73, 100, 101, 119, 120).

Formal attempts at archaeological application are fewer. Bettinger & Baumhoff (4, 5) have joined the diet and patch models to deduce a theoretical continuum of hunter-gatherer strategies superficially resembling Binford's forager-collector series. This model, however, has the advantage of specifying precise relationships between population and resources, on the one hand, and settlement and subsistence patterns, on the other. Related research would include proposed explanations for prehistoric colonizations in Australia and North America derived from the diet breadth model (73, 74). Experimental

work has also been performed to determine energy extraction rates for certain North American resources (98, 99; but see 5).

Criticisms of Optimal Foraging Theory

With all the attention and comment optimal foraging theory has attracted in archaeology, it is hardly surprising that some has been negative. In part, this criticism recognizes nagging ambiguities surrounding the application of optimal foraging models to animals in general and humans in particular. Three unresolved issues seem fundamentally important. First, is energy (rather than a mix of nutrients) the correct currency with which to measure foraging efficiency (53, 56, 65, 87, 102, 103, 120)? Second, is foraging properly treated as an endless string of individual responses to momentary contingencies rather than as an overarching plan or strategy (3, 56, 65, 69, 96; e.g. 54, 55, 88, 120)? Third, what kind of mechanism (e.g. genes or learned behavior) might cause humans to forage optimally (47, 56, 65, 73, 102, 103, 111, 120)?

Scientific skepticism in such matters is a predictable and healthy component of science both during and between times of "paradigm crisis" (cf 59). Doubtless it will persist as optimal foraging theorists seek anthropological credibility for their models. It is more difficult to understand the basis for certain criticisms raised by individuals figuring prominently in middle-range research, who are among the harshest critics of optimal foraging models. Binford has been particularly outspoken in the matter. His objections, however, are as curious as they are unconvincing. He protests first on the grounds that he cannot "understand how constant properties can guide us to understand and explain cultural diversity" (19, p. 219; see also 13, p. 455). There is little merit in this objection. Science is full of constant properties useful in understanding diversity. Indeed from the structural transformations of Levi-Strauss, to the forager-collector continuum, to the double helix and DNA, it is difficult to think of a single kind of scientific theory (good or bad) that does not use constant properties to explain diversity. This is, in fact, what theories are supposed to do.

Binford continues in specific criticism of optimal foraging theory and its assumption of economic rationality. "If there is one principle that anthropological field studies have affirmed over and over again, it is that the intellectual contexts of behavior in different cultures renders [sic] rationality a relative phenomenon" (19, p. 220; but compare 13, p. 453). This is the standard substantivist criticism that, as a version of formal economics (microeconomics), optimal foraging theory is flawed because it ignores cultural values and arbitrarily separates economy from other parts of culture. This criticism may be just; but mark well what substantivism offers as an alternative: *Norms* guide human behavior. The substantivists, in particular Dalton

(27–29) and Polanyi (81), argued that economic behavior was the result of individuals' acting on the basis of enculturated norms; this was in contrast to the formalist view that individuals act in their own self-interest, partially constrained by cultural norms (cf 32). The substantivists, however, consistently failed to account for the source of their norms and so, in essence, their explanations of economic behavior were merely descriptive. Does Binford believe this the proper approach to the study of hunter-gatherer economic systems? If so it is surely a turnabout from his earlier work—in particular his vehement criticisms of normative models (e.g. 6). Certainly it contradicts the very premises upon which a great deal of his own middle-range research is built (see below).

Note also that unless Binford is claiming that rationality is *entirely* cultural—which would put him squarely in the camp of extreme substantivism (e.g. 91) and eliminate any active role for materialist theory in either archaeology or anthropology—his suggestion that models of objective rationality are without analytical value is groundless. Weber (116) spoke eloquently to this point. He posited that human rationality consisted of two parts, which he termed substantive and formal, respectively. The former corresponds to Binford's (substantivist) concept of culturally conditioned rationality, the second to objective rationality (as a formal economist or optimal foraging theorist would see it). In recognizing this distinction, Weber held that the utility of analyses grounded in concepts of objective rationality was not obviated by the fact that rationality was in part culturally conditioned. Quite the contrary, he argued, the former required the latter: Individuals are held in constant tension between the actions dictated by cultural rationality and actions dictated by the objective rationality of selfish individuals. Recognizing this tension required that subjective and formal rationality be distinguished analytically so that the relative effects of each could be weighed. This is precisely what some optimal foraging theorists have argued (74, 103; see also 2). Such studies provide a yardstick of objective economic rationality as a basis for the comparative study of human behavior. Without this yardstick it would be impossible to know whether human behavior was governed by cultural norms and ideas or by rational, self-interested individuals. Ironically, Binford (20) has elsewhere made essentially the same point.

Thus on these particular points, the criticisms Binford has leveled against optimal foraging theory are without substance. Likewise the distinctions some middle-range theorists seek to draw between their research and optimal foraging research are largely illusory, and the criticisms of optimal foraging theory they rely upon to emphasize these distinctions are ill-founded. The two research programs share virtually identical premises and assumptions. It seems worth taking the time to document these similarities, if only to set the record straight.

Note first that optimal foraging theory qualifies as middle-range theory in the sense of both Merton (cf 111) and Binford (10, 16). It employs a few generalities (those about utility and economizing we have reviewed above) and from these sets forth explicit and testable predictions. That prominent middle-range theorists have taken issue with the model is all the more puzzling, given that it is merely a more formal statement of the same principles upon which much recent middle-range research, including their own, is based. This is particularly clear in the case of Binford's (13) well-known and widely used (110) model of butchering strategies. In this model different skeletal elements are scaled from highest to lowest utility according to their by-weight complement of meat, marrow, and grease. This scaling is formally identical to the ranking of items of diet by energy utility in optimal foraging. Both assume that economic utility governs the value assigned to different commodities by foragers (consumers) and that this value, however justified in cultural terms, determines forager (consumer) behavior with respect to these commodities (13, pp. 38–39, 477). It is equally obvious that optimal foraging theory faithfully predicts the conditions under which one would expect the two basic butchering strategies postulated by Binford (13, p. 81): *bulk utility,* in which all but the lowest-utility items are taken; and *gourmet utility,* in which all but the highest-utility items are left behind. Indeed, Binford's suggestions that the bulk strategy is to be expected when demand is high, the gourmet strategy when demand is low (13, p. 44), does little more than restate the basic microeconomic (and therefore optimal foraging) principle that the use of various items varies according to demand. In both cases the probability that a given item is used increases as overall demand increases relative to supply. Note, too, that both butchering and optimal foraging models are models of site formation, in that for both the probability an item will be deposited as a product of use is, ceteris paribus, a direct function of its probability of use.

The forager-collector continuum (15) is grounded in precisely these same principles of economic utility. It suggests that where access to resources is temporally or spatially restricted, hunter-gatherers respond with behaviors that extract greater utility from these resources: storing them to gain temporal utility, and caching them and the gear used to procure them to gain spatial utility (9, 13–15). In short, storage and caching, and in consequence virtually the full spectrum of adaptive variability between foragers and collectors, results from rational economizing under differing conditions of resource supply and demand. Binford (18) has elsewhere found it useful to call upon microeconomic principles formally identical to those in optimal foraging (e.g. 2, 4, 44) to explain late-Pleistocene/early-Holocene dietary shifts and the origins of agriculture.

Whatever the inspiration for his recent suggestion that human rationality is relative and that optimal foraging theory is accordingly flawed, nearly the whole of Binford's work in middle-range research is predicated on the assumption that human behavior is economically rational (13, pp. 38, 44, 453, 477). Lacking this, his own cross-cultural studies of hunter-gatherer economy would be pointless (e.g. 15; see also 57). Furthermore, optimal foraging theorists anticipated many of Binford's basic notions about economic behavior ten years prior to his work in middle-range research. That Binford cites a standard work in optimal foraging theory and evolutionary ecology (13; cf 34) to defend his views regarding the relationship between utility, rationality, and the universals of behavioral economy only underscores the intellectual debt his own model owes to microeconomic and microevolutionary theory.

Granting that optimal foraging theory is grounded in the same basic principles that more informally guide most middle-range research (and this much is hardly in contention), it follows that all the objections raised against the application of optimal foraging theory to archaeological problems are equally applicable to middle-range research. Thus, the claim that optimal foraging models are difficult to operationalize (111), while admittedly true, applies every bit as well to the middle-range theories—those that deal with butchering, for example. It is no more difficult to ascertain whether prehistoric groups were foraging rationally than it is to ascertain whether prehistoric butchers were carving rationally. This is necessarily so since we have just shown the latter to be a special case of the former.

If I am correct in my arguments about the logical similarities between middle-range research and optimal foraging theory, then why do the most ardent proponents of the middle-range theory find optimal foraging theory so objectionable? It would seem to be because optimal foraging theory has sometimes been linked to an overarching general theory, sociobiology (e.g. 45, 47, 74, 102), that these individuals find objectionable (18, 19, 111). Yet as it is clear that both optimal foraging theory and middle-range theory are limited theories ultimately governed by more general theories, and as optimal foraging theorists have attempted to articulate their limited and general theory while the middle-range theorists have not, it hardly seems fair for the latter to engage in such criticism. Indeed, as noted earlier, the middle-range theorists have either been conspicuously silent on the matter or emphatically denied any need to consider general theory. One would gather from various clues that Binford still allies himself with neofunctionalism—witness his reference to homeostatic devices that function to maintain hunter-gatherer groups at optimum sizes (18), his appeal to the explanatory power of vaguely phrased "ecological relationships" (15), his preference for models that stress equilibri-

um (18), and his emphasis on systems (e.g. 13, 15, 17, 19) and adaptation (e.g. 13, 19). (Unfortunately, this affiliation is not made clear, so one cannot be sure.) In the final analysis it is this purposeful ambiguity with regard to general theory that is the core of the trouble with current middle-range research and the center of the current confusion over what middle-range research is and why optimal foraging theory is or is not acceptable.

IMPLICATIONS

The recent shift in archaeological objectives away from the development of general theory toward the more modest endeavor now termed middle-range research was beneficial in many respects. By the mid-1970s American archaeology had become so riddled with far-fetched and untestable theories that it was hardly clear what was real and what was not—or whether this mattered at all (cf 38, 39). Unfortunately, in their haste to put aside vacuous theorizing, middle-range theorists seem to have accepted the myth that their research can proceed in the absence of general theory (cf 16, 111), when clearly it cannot. Widespread confusion regarding the nature of middle-range research is one symptom of this. The misplaced criticism of optimal foraging theory is another; purportedly it centers on matters of practicability and verifiability, when it actually turns on more deeply hidden views about general theory. In this way, the rhetoric of middle-range research has caused an issue of general theory to be treated as though it were a problem of middle-range theory and operationalization.

It is time that archaeology once again owned up to its dependence upon general theory and confronted the knotty issues lurking therein. If it has been recently convenient to act as though our limited theory is independent of general theory, it is only because this permits eclectic movement between different theoretical positions without commitment to one. At our convenience, humans are rational here (13, p. 453), irrational there (19, p. 220), or somewhere in between (18, pp. 221, 241–242). This strategy has, in short, allowed us to avoid all the messy problems one confronts when attempting to apply ideal models to the real world.

Admittedly, the prospects for a general theory in anthropology are presently bleak. Neofunctionalism, the general theory in vogue when archaeologists began ignoring such matters, is hopelessly out-dated. The two strongest remaining contenders—sociobiology and Marxism—are hardly more appealing. The former seems to emphasize genes and genetics too strongly to explain adequately the full range of human endeavors. The latter, in its repeated attacks on empiricism and scientific objectivity (78), seems to reject the very basis for archaeological interpretation. Worse still, few archaeologists know enough about either to form an opinion. Most will need to do a great

deal of homework before they can make a choice. All the same, such homework, along with more innovative work in theory-building, needs doing. Archaeology—indeed, all of anthropology—is sorely in need of a viable general theory, and it will no longer do to pretend otherwise.

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