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ARTICLES

Behavioral and Other Human Ecologies: Critique, Response and Progress through Criticism

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Abstract

This paper has three goals: (1) to define the anthropological subfield of human behavioral ecology (HBE) and characterize recent progress in this research tradition; (2) to address Joseph's (2000) critique of HBE from the perspective of an advocate of that field; and (3) to suggest features that make for effective criticism of research traditions. (1) HBE attempts to understand intra- and inter-societal diversity in human behavior as the product of species-wide adaptive goals which must be realized in highly diverse, socio-environmental circumstances. Theoretically, HBE draws selectively from neo-Darwinism and its cultural-evolutionary analogs, from micro-economics, and from elements of formal decision and game theory. Applications generally use simple, formal models as heuristic devices for generating testable hypotheses about resource use, reproductive and social behavior, and life history traits. (2) Using Kuhn's (1977) and McMullin's (1983) criteria for assessing progress in a research tradition, I examine Joseph's review of HBE, indicating the several points on which we agree and the greater number for which I believe her criticisms are misplaced or in error. (3) Finally, I try to describe general features of effective critique, in the sense of critical commentary that enables the advance of scientific understanding through collective scholarly effort. Such criticism will be necessary if we are to sort out the relative strengths and potential contributions of the several research traditions in human ecology (e.g., cultural ecology, historical ecology, political ecology, etc.).

Introduction

Human behavioral ecology (HBE) is a subfield of the social sciences in general and anthropology in particular. It is a sibling approach to cultural, political, historical, and other varieties of human ecology, with which, like all good sets of siblings, it shares a certain amount of likeness from disciplinary contiguity, habit and sympathy, as well as the occasional episode of misunderstanding, fractiousness and critical, inter-sibling rivalry. In its broadest manifestation, HBE represents an attempt to understand diversity in human behavior on an inter- and intra-societal basis as the product of common, species-wide adaptive goals which must be realized in diverse, socio-environmental circumstances. It employs ethnographic methods, particularly participant observation with local populations, although it brings to field work a more quantitative emphasis than found generally in anthropology. For theory and concepts, HBE draws selectively from neo-Darwinism and its cultural-evolutionary analogs, from micro-economics, and from elements of formal decision and game theory. Thus, the approach adopts aspects of methodological individualism and reductionism, drawing on such premises as rationality, optimization, and evolutionarily stable strategies, along with analytical concepts such as marginal value and opportunity costs. Scholars adopting this approach generally are committed to the use of simple, formal models as heuristic devices for generating testable hypotheses from the more general propositions found in theory.

HBE comprises several closely related strands of research. The more ecological track began in the mid-1970s, when a small group of anthropology graduate students and faculty began adapting opti-

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mal foraging theory models for the study of hunter-gatherer ecology (Winterhalder and Smith 1981). A second track developed through the application of evolutionary biology models to the analysis of human reproductive and social behavior (Chagnon and Irons 1979, Low 2000). These two strands, and a third, that of dual inheritance or cultural evolutionary models (Boyd and Richerson 1985, Durham 1990) began more-or-less independently, are yet incompletely merged (Smith 2000) and sometimes are reciprocally critical of one another and of the related studies that comprise the field of evolutionary psychology (Smith et al. 2001).

The more ecological literature of HBE, on which I shall focus, was developed as an alternative to the cybernetic, ecosystem and energy-flow schools of ecological anthropology dominant in the 1970s (Richerson 1977, Smith 1984). From this base, HBE has since expanded to encompass questions of resource distribution, life history characteristics, social hierarchy, the origins of agriculture, and multiple other topics (Winterhalder and Smith 2000). The methods employed have likewise proliferated, and now include game theory (Ruttan and Borgerhoff Mulder 1999), risk-sensitive techniques (Winterhalder et al. 1999), dynamic programming (Luttbeg et al. 2000, Mace 1996), and field assessment of utility (Kuznar 2000), among others. HBE field research no longer is confined to hunter-gatherer populations, but includes case studies undertaken with pastoralists (e.g., Mace 1993), horticulturalists (e.g., Hames 2000), fishers and harvesters of sea turtles (e.g., Bird and Bliege Bird 1997, Sosis 2000), agriculturalists (e.g., Voland 1998) and wage workers (e.g., Lancaster and Kaplan 2000). Archaeologists are among those making important contributions (Bettinger et al. 1997, Broughton and O'Connell 1999, Grayson 1998, Madsen and Schmitt 1998), HBE has ventured creatively into the interpretation of hominid (O'Connell et al. 1999, Kaplan et al. 2000) and agricultural (Piperno and Pearsall 1998) origins, and there is a long-standing feminist strain set within the field (review in Liesen 1998, see also Hrdy 1999).

Ecological researchers in HBE generally consider themselves to be advancing a project much like that envisioned by Steward (1955), though informed by contemporary evolutionary ecology theory and more rigorous in its formalization of hypotheses and field methodologies (Turner et al. 1997:34, Winterhalder and Smith 1992:20-21). The reader interested in HBE generally is recommended to Smith and Winterhalder (1992, Winterhalder and Smith 2000). For the compendia representing the broader field of evolutionary social science in anthropology, see Weingart et al. (1997), a recent collection of case studies (Cronk et al. 2000) and an edited collection of classic papers, with critical commentaries by outsiders and the original authors (Betzig, 1997).

Joseph's Critique of HBE

This description of HBE will be sufficient for the reader to appreciate that it is something of an anomaly in contemporary anthropology, which has tended in recent years toward methodological collectivism, qualitative analyses, and political, cultural or interpretive emphases which sometimes take a critical or even militantly hostile stance toward overtly scientific, materialist and evolutionary or economic studies of humans. Wide ranging critiques of HBE have been published by social (Ingold 1996) and ecological (Vayda 1995a, 1995b) anthropologists. Among the more recent is an article by Joseph (2000), the subject of this response, invited by the co-editor of the *Journal of Ecological Anthropology*, David Casagrande. I am an appropriate though not disinterested individual for this task since Joseph turns frequently to my work in HBE to illustrate her points.

Joseph frames her critique of HBE, or what she appropriately calls anthropological evolutionary ecology (AEE), in terms of a "components of theory" (p. 7, Box 1) perspective, borrowed from the ecologists Pickett, Kolasa and Jones (1994; hereafter PK&J). While PK&J develop their ideas about "the nature of theory and the theory of nature,"[2] the subtitle of their book, in order to promote integration and synthesis among the many sub fields of bio-ecology, Joseph puts

[2] To the extent that they deal with the philosophy of science, PK&J have the mainly emancipatory goal of freeing ecology from lingering influences of positivism and understandings of theory based too strictly on history and practice in the physical sciences. They are not as systematic about pursuing formulation of a coherent substitute, which would require that they sort out and take a consistent position on various of the contemporary possibilities (Laudan 1990).

their framework to the somewhat different task of criticizing a particular sub-field of anthropology, HBE.

I follow her outline for the first part of this essay. I describe and comment on some of Joseph's criticisms (and note some shortcomings of HBE not mentioned or emphasized by her). However, I have a larger goal than that of identifying where I think her observations about HBE are, or are not, apt. I am more broadly interested in the uses and usefulness of criticism in scholarly writing, particularly in a heterogeneous field like ecological anthropology. In the second part of this paper I try to identify some practices which I believe enhance or detract from the scientific value of critique. What makes criticism effective or ineffective—something quite apart from what makes it usually satisfying to write and sometimes painful to read—is seldom if ever a subject of explicit discussion in anthropology. Nonetheless, I think we owe ourselves some attention to it. The topics we address as human ecologists, the origins and history, the structure and functioning, and the sustainability of human-environment relations, along with their capacity to support healthy and satisfying lives across diverse societies, are of great practical and humane significance. Improving the basic analytical tools by which we advance understanding of these topics is a responsibility we owe to scholarship as well as to the people we study and about whom we write. In this, good criticism is as important as good research design and field methodologies.

Specifics of the Joseph Critique

(1) In her introduction, Joseph frames her critical evaluation of HBE in terms of a two-by-two array of sixty cells determined by twelve components (measuring degrees of theory completeness, and extending from "notions" to "translation modes") and five statuses (measuring degree of theory development, from "pre-theoretic" to "confirmed or rejected"), adopted from PK&J.[3] This scheme may be a useful organizing device at a very general level, in that it highlights the diverse elements of science. However it is unnecessarily complex and unrealistically precise as an evaluative tool. I had little success, for example, in trying to classify the parts of HBE I know well enough to understand their flaws and guess at their incompleteness. By contrast, in a recent review of HBE, Winterhalder and Smith (2000) use a much simpler scheme of six criteria derived from the philosophers of science Kuhn (1977) and McMullin (1983), hereafter, K&M. The K&M criteria are: predictive accuracy, internal coherence, external consistency, fertility, unifying power and simplicity. Some comparison in terms of these two schemes is instructive, and will illustrate problems of assessing a young field like HBE at a very high level of generality like that applied by Joseph.

According to Joseph, the PK&J scheme values the addition and refinement of components and empirical content. Thus greater complexity signals progress. On this, HBE (AEE) gets low marks: "in the case of AEE, more complex or highly derived components have not yet accompanied simpler ones" (p. 8). It is tricky to assess relative degrees of simplicity and complexity. Nonetheless, I think Joseph is mistaken about the failure of HBE to produce complex or advanced components, and cite the list of methodological developments given earlier as an example. Perhaps the more interesting issue here is which of the desiderata—simplicity or complexity—we should value most as scientists.

I begin with appraisal of HBE in terms of its simplicity or complexity, using several additional examples. Joseph overlooks or under-values significant HBE developments in part because her review does not examine the history of the field. For instance, early ecological use of HBE took the form of optimal foraging theory models. Among other elements, these models require that one specify a *currency* by which the relative costs and benefits of behavioral alternatives such as different selections of resource options can be assessed. Applications

[3] The full list of Joseph's components is: notions, assumptions, definitions, concepts, facts, confirmed generalizations, hypotheses, models, theorems, framework, domain and transaction modes. The full list of statuses: pre-theoretic, intuitive, consolidating, empirical-interactive, and confirmed or rejected (Joseph 2000: 7, Figure 1). The like Figure 4.1 in PK&J (1994: 90) includes one component, "laws," missing in Joseph; Joseph adds two components, "theorems" and "domain," not in PK&J, for reasons that are not explained.

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like those in Winterhalder and Smith (1981) typically used a very basic currency: the edible kilocalories of energy that humans could derive from the resource. Subsequent developments over the last two decades have elaborated on this element by demonstrating the importance of marginal evaluations of currency (Winterhalder 1996), by incorporating additional nutrients (Hill 1988), and by allowing for social differentiation in the ways that individuals (e.g., males and females; youth and adults) might value currencies (Hawkes 1991; Hawkes et al. 1995, 1997). Each of these has been a fruitful elaboration of a simple, initial assumption. Likewise, early and purely deterministic foraging models now have stochastic versions (review in Winterhalder et al. 1999); early non-dynamic or state-independent mathematical techniques are now matched by dynamic ones (Luttbeg et al. 2000); and, graphical or algebraic model solutions have been extended into simulation (Winterhalder et al. 1988). Agent-based variants (Kohler 2000) of basic optimal foraging theory (OFT) models are a coming development. Further, as noted in the introduction, the field has expanded into new topic areas, and to analyses of non-foraging modes of production (Winterhalder and Smith 2000). Similar developments have taken place in parental investment (e.g., Lancaster and Kaplan 2000) and life history (Hill and Hurtado 1996, Hill and Kaplan 1999) studies.

In each of these instances, a simple element (e.g., currency, model, analytical framework, realm of application) has been retained while more complex or highly derived components are introduced by stepwise refinement or elaboration of its possibilities. Winterhalder (1986:370-371) has described this process of beginning with basic elements and then—as these are better understood—of cautiously adding back complexity in terms of "simple models, progressively extended."[4] It is an imporant feature of the history of HBE ignored in Joseph's evaluation.

Though elaborations and increasing complexity are clearly evident in the development of HBE, I wish to stress the parallel intent to retain elements of simplicity. HBE cannot be located at one point on a simple-to-complex axis, nor has it moved unidirectionally along such a continuum. I would characterize its history as one of mosaic development, seeking just enough complexity as is suited to a particular analysis or development and guarding simplicity whenever it is possible to do so. I suspect that most HBE researchers would cite Occam's Razor and agree with K&M that simplicity is a virtue in scientific theories (see Boyer 1995). On this basis they likely would evaluate their field more favorably than does Joseph using the PK&J criterion.

In addition, Joseph criticizes HBE for what she terms transfer problems: "Drawing on other theories for components has also resulted in transfer problems, where those components have acquired different meanings and interpretations problematic in their new context" (p. 8). Joseph develops no specific examples of such problems, but it appears that she finds troubling the analysis of anthropological materials using theory that overlap with evolutionary biology and economics. By contrast, the external consistency criterion of K&M gives high marks to scientific theories that share key elements with related theories in other fields. This allows Winterhalder and Smith (2000) to evaluate HBE favorably for the same feature that is troublesome to Joseph: its willingness to seek interdisciplinary consistency with neo-Darwinism, micro-economics and other, relatively mature schools of theory. This kind of interdisciplinarity of course is not new to or unusual for anthropology, although these particular linkages currently are out of favor with many anthropologists.

(2) Domain offers another example in which the evaluative criteria of the two frameworks, PK&J and K&M, will lead us to divergent interpretations. It is defined by Joseph as "the scope in space, time and phenomena addressed by a theory" (p. 7). Low marks again for HBE:

> The domain is formulated early in theory development, but is only fully articulated at later stages of theory maturation (p. 8). . . Domains

[4] An investigative procedure that recalls Vayda's (1983) notion of "progressive contextualization," though at a micro-foundational scale of analysis and realized over the cumulative record of a research tradition.

> typically become more restricted as theory develops because refinement shows that the theory is not as grandly applicable as originally presumed (p. 10) . . . There is no attempt within AEE to delineate the boundaries or domain of investigation and specify that various predictions derived from AEE models are only potentially useful for understanding human behavior in highly circumscribed circumstances or at very limited levels of analysis (p. 10) . . . The failure to delimit the domain of AEE creates confusion and debate as to its applicability. (p. 12)

Several elements of this criticism are incorrect; others invite disagreement with respect to their significance as evaluative criteria.

First, it is simply wrong to claim that HBE writers never acknowledge that the domain of their approach is limited ("There is no attempt . . ."). Such attempts are explicit and they are frequent in the HBE literature. Here is an extended example, the second paragraph of the introduction to Winterhalder (1983:201-202, see also Winterhalder 1987:311-313 and Winterhalder 1986:370):

> T[his] chapter has a narrow focus—the attempt to understand systematically how a Cree makes foraging decisions when harvesting food-producing resources found within the boreal forest. These resources are treated as valuable only in that they provide calories. Nutrients and nonconsumable products are, for the moment, ignored. This goal has determined the topics covered. Equally important, it excludes several obvious and relevant subjects: the acquisition and use of Euro-Canadian food stuffs, gardening, and, except peripherally, the effects of the fur trade on foraging decisions. This is a severe restriction, but one with advantages. Foraging behavior is complex. Hunting and gathering as ecological processes are qualitatively and especially quantitatively little understood; thus, rather specific issues must be resolved before the more inclusive ones can receive reliable analysis. I hope to show that specific questions, systematically studied, can generate fresh appreciation of the more general issues that tend to engage anthropologists, and build a base necessary for expanded coverage.

Compare this to Joseph's claim, "Thus, missing are well-developed qualifying statements that various empirical findings derived from optimality models only predict foraging behavior in highly contingent circumstances or at very limited time scales and at specific levels of socio-political organization or complexity" (p. 18).

Attention to domain also is evident in the careful placing of HBE relative to the broader roster of questions that can be asked in evolutionary analyses (Winterhalder and Smith 1992:9-11). It is unmistakable in the detailed acknowledgement of the difficult analytical trade-offs that accompany using simple models (Winterhalder and Smith 1992:12-14). It is apparent in care given to describing proper and improper forms of reductionism (Winterhalder and Smith 1992:14-16). It should be evident in willingness to follow a social theorist like Elster (1982, 1983, 1985) on key issues related to methodological individualism and collectivism, types of social science explanation, and rationality (Smith and Winterhalder 1992:38-50).

Most anthropologists using HBE have been reluctant to make early, programmatic pronouncements on the *topical* boundaries of the field, especially grand ones. To take a personal example, I started HBE research focused on analyzing the resource and patch selection decisions of hunter-gatherers (Winterhalder 1977), a quite limited domain. I did not imagine that twenty-five years later software engineers would be citing and experimentally extending foraging theory models to analyze how postmodern "informavores" use the internet (Pirolli and Card 1999) or that library scientists would be using them to better understand and help their information-foraging patrons (Sandstrom 1994). Likewise, when I began trying to incorporate risk (stochasticity) into resource selection models (Winterhalder 1986), I had no prescience that a decade later a colleague and I would find an application of the models to human fertility decisions that may be relevant to the explanation of demographic transitions, agricultural intensification, and socio-cultural variation in fertility (Leslie and Winterhalder 2001).

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Although it would have been imposible to predict such an outcome, HBE models, methods and topical applications generally have proliferated creatively and often successfully for 25 years. HBE continues to surprise its practitioners in positive ways, giving no signs yet of exhausting its creative impulse as a research tradition. For the most part, those in the field have followed a tactic opposite that described in Joseph and the PK&J framework, one of beginning modestly and expanding their domain as experience warrants. Ecological anthropologists in HBE have not generally made early, pre-emptive or expansive claims for topical domain, but have followed ideas where they seemed to lead, letting the domain grow with the promise of insights and the success of applications.[5] This is untidy and perhaps confusing, but it is more honest and pragmatic than augury.

If predicting domain is impractical, it also may be philosophically suspect. K&M argue that the progress of a science can be evaluated positively if its domain grows in ways *not* foreseen by its initiators. Here again, a phenomenon that the PK&J framework might depreciate is cast favorably by the K&M criteria for scientific progress.[6]

We have then, three instances of divergent appraisal, depending on which of two general evaluative frameworks is employed. I happen to believe the approach taken by K&M represents a superior historical and philosophical understanding of scientific progress (e.g., Laudan 1977). It is also a more practical approach to assessing it than the scheme of PK&J.

If the element of evaluative indeterminacy here is cause for dismay, we might have better results and more agreement with K&M's category of predictive accuracy, which I take to be similar to PK&J's highest developmental statuses: "empirical-interactive" and "confirmed or rejected." Here Winterhalder and Smith (2000:65-66) are themselves critical of their field: ". . . the number of compelling, data-rich HBE studies is still quite small . . . On this most important of desiderata—superior agreement with observation—the HBE record is positive but altogether too thin." Joseph may agree with this, although my remedy—more HBE empirical studies of ever better methodology—may not be hers.

(3) Basic Concepts: In this section Joseph describes "four general features" (p. 13) of HBE, cogently for the most part. Because the points she makes also are widely summarized in the general HBE literature, I will not repeat them. I do wish to call attention to four areas in which I believe her characterization to be inaccurate, misleading or incomplete.

(a) First, HBE does not apply natural selection theory at the macro-evolutionary scale (p. 13). Rather it focuses on the evolutionary micro-scale almost exclusively. It is either agnostic or quite tentative about its potential for contributing to the explanation of community- or ecosystem-level phenomena, or phenomena that are evident over long (macro-evolutionary) time scales, such as phylogenetic patterns of speciation (Winterhalder and Smith 1992:22, Kacelnik and Krebs 1997:27). Based on the success of neo-Darwinism and the absence of viable theoretical alternatives of comparable scope, we have good reasons to think that neo-Darwinism eventually will provide the *necessary* micro-foundations for understanding natural processes at all scales. But, whether or to what degree micro-evolutionary processes will be *sufficient* to characterize the emergent properties of communities, ecosystems and long-term phylogenies is at present an open question. Given this self-imposed caution, Joseph's discussion of punctuated equilibria, Milankovitch cycles, co-evolution and the Red Queen Hypothesis as challenges to behavioral ecology's domain (p. 10-12) have no purchase.

[5] I personally find this more appealing than an approach like that of cybernetic/homeostatic ecosystem theory (e.g., Odum 1969), which began with undue certainty and expansive statements and then went through the lengthy ordeal of a messy retreat from its claims. False confidence about domain (e.g., sociobiology's early claim that it would subsume the social sciences, anthropology's repeated claim that rationality theory applies only to capitalist societies; relativism's claim to trump all other philosophies of science) is a scourge in our field.

[6] Although PK&J do state: "A question that causes a theory to encompass some phenomenon well outside its accepted domain is fundamental" [to increasing the scope of a theory] (PK&J 1994: 119).

There are good examples of the ways in which behavioral ecology research can play this microfoundational role. One is Sutherland's (1996) monograph-length exploration of the ways in which a very simple behavioral ecology model (the ideal free distribution) can inform issues of population distribution and ecology. Others are found in the volume on behavioral ecology and conservation biology edited by Caro (1998).

(b) Second, the manner in which methodological individualism (MI) actually is employed by HBE is more limited than the statement made by Joseph (p. 13). It is a research tactic, not a claim about social reality (Smith and Winterhalder 1992), a perspective not so different from schools of socio-cultural anthropology placing emphasis on actor-based approaches and human agency. MI is "the doctrine that all social phenomena—their structure and their change—are in principle explicable in ways that only involve individuals—their properties, their goals, their beliefs and their actions" (Elster 1985:5). This strict definition entails some supra-individual elements, as the properties, goals, beliefs and actions of individuals are social and not themselves solely individual phenomena. Elster cites further qualifications (e.g., Elster 1985:5-8, 359) and HBE statements echo his caution. Thus, "[t]he primary goal of MI [methodological individualism] is to provide 'microfoundations' or 'actor-based accounts' for social phenomena by analyzing the extent to which they are the aggregate outcomes of individual beliefs, preferences, and actions" (Smith and Winterhalder 1992:39). Note the open-ended phrase, "the extent to which." This statement does not say, "by showing that they are entirely the aggregate outcome of individual . . ." Likewise, "The general point, for either [the] MI social scientist or evolutionary ecologist, is that explanation of social phenomena, including group-level benefits, should pay attention to individual-level mechanisms" (Smith and Winterhalder 1992:41). Again, note the qualified status of this claim. It says "should pay attention to," not "need only pay attention to," individual-level mechanisms. This parsing of quotations would be unnecessary except that mention of MI and certain other conceptual elements of HBE (optimization, rationality, etc.) appears to blind anthropologists to qualifying phrases set around them.[7]

(c) Third, HBE does assume differential fitness (reproductive success) among the members of a population, thus a key neo-Darwinian component of natural selection, but this is not the same as assuming "high reproductive success and stability" (p. 14, see Smith and Winterhalder 1992:50-53). Differential reproductive success can occur whether fertility is high or low, and irrespective of its stability over time.

(d) Finally, Gould and Lewontin (1979) are cited favorably by Joseph for their high profile critique of the "adaptationist program," of which HBE is a part. The reader will get a more balanced perspective on the adaptation debate in biology by also consulting rebuttals of that paper (e.g., Maynard Smith 1978, Mayr 1983), along with the sociology of science and rhetorical analyses of it found in Selzer (1993).

(4) Empirical Content. In this section Joseph briefly describes some instances of foraging and life history analyses. She criticizes them not so much for what they do but for what she believes that they omit. Her general claims are two-fold: first, the empirical analyses and case studies generated by optimality models "have a much narrower meaning and significance than recognized within AEE" (p. 14). This is a question of domain, and a reasonable position only if the often repeated cautionary statements in the HBE literature are ignored (see above). Second, Joseph appears to wish that every analysis be comprehensively holistic and encompass multiple scales:

> Even simple outcomes may be due to very complex processes and these processes involve more than just simple individuals acting to maximize economic or reproductive returns.

[7] Incidentally, this tactical focus on individuals has its counterpart in bio-ecology: "It is quite significant that all the traditional ecological hierarchies intersect at least at the level of *individual organism*, suggesting that this node may be the place to begin to explore commonality and integration in ecology" (PK&J 1994: 21; italics added).

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> Rather, such historical processes involve the presence of complex state structures, sociopolitical relationships, and cultural and ideological processes of negotiation. At the very least, empirical findings should clarify (or involve reference to) pattern, process, cause and mechanism. . . and locate individual behavior[s] in their complex multiple environments, which include physical, biological, social and cultural at a minimum. . . . (pp. 15-16)

The ontological claim here ("Even simple outcomes may be due . . .") I assume to be true in its contingent form.[8] The methodological claim ("At the very least . . .), however, is not acceptable. There certainly is a place in ecological anthropology for comprehensive, multi-scale analyses; in fact, there are instances of this approach in HBE. Hrdy's (1999) study of mothering is a marvelous example. But, broad-stroke holism isn't the only analytical option, nor always an effective place to begin. Undertaken prematurely, it can impede understanding or produce superficial results (e.g., Elster 1985 on Marx's group-level functionalism). For this reason, HBE is explicit about the importance of isolating and pursuing more limited analyses:

> Foraging behavior variability is complex and multicausal, but unless we can predict the effects of its causes taken separately, we have no hope of disentangling their respective effects when taken together. (Winterhalder 1986:371)

And, as befits this approach, HBE also is clear about acknowledging the need eventually to move beyond a focus on the particulars:

> Explicitly reductionist in its methods, as HBE matures it will have to demonstrate that it can successfully reintegrate topically isolated models into a compelling and more holistic understanding of human adaptive behavior. (Winterhalder and Smith 2000:67)

Many of Joseph's examples are interesting for features that she overlooks or for the way in which her criticisms miss their mark. For instance, the work of archaeologist Jack Broughton (1994a, 1994b, 1997)[9] is an especially compelling example of HBE analysis because he makes an active effort to exclude alternative explanations (e.g., climate change) of empirical observations he believes to be consistent with foraging theory hypotheses. Joseph claims that Broughton neglects environmental and historical context, but in fact, they are the very heart of his work on resource intensification in the late prehistory of California. Similarly, Joseph describes[10] my extensions of basic foraging models into analyses of the population dynamics of foragers and their resources (Winterhalder et al. 1988), and to social questions of resource distribution within groups (e.g., sharing, Winterhalder 1986), as if they vitiated simple foraging models. In fact, these papers are refinements and extensions of basic foraging models that, to the extent they are successful, validate what K&M criteria would term the empirical fruitfulness of foraging theory.

Joseph summarizes the debate surrounding Blurton Jones' (1986, 1987) birth interval model for the !Kung San in terms that make it appear to have been a largely futile exercise: "Thus, we are still left with a case of one, with several methodological shortcomings" (p. 19). Blurton Jones' own candid retrospective (Blurton Jones 1997:83) on the research and discussion his analysis provoked is no less critical, but it does conclude with a less pessimistic and more realistic appreciation of the way in which science progresses: "In science we often learn more when things don't work out the way we expected." By choosing three examples of refuted hypotheses from Hill and Hurtado's (1996) monograph on Ache ecology and demography, Joseph is able to conclude that life history theory has "contributed few if any interesting insights" (p. 19). Smith's (1996) review essay,

[8] "The qualities of [human] behavior impose special demands on its analysis. Individual behavior and social life are complex and diverse, ephemeral in their observable manifestations, and subject to rapid change over time. They are shaped by several different kinds of causes ranging from genes to symbols" (Winterhalder and Smith 1992: 4).

[9] Joseph's account relies on a summary found in Boone and Smith (1998).

[10] Joseph relies on summaries found in Bettinger (1991), replicating Bettinger's mistaken citation of Winterhalder et al. (1988) with a date of 1989.

by contrast, describes confirmed hypotheses, and catalogs the work's many contributions to ethnography, methodology (e.g., in assessing small population age structure) and theory. Analyses by Wilson and Daly (1997) refute Joseph's claim (p. 19) that life history theory is of interest only to inter-specific or higher taxonomic comparisons.

Throughout her review Joseph tallies falsified hypotheses as if they were coffin nails for the HBE effort. However, behavioral ecologists celebrate having a framework that takes hypothesis testing seriously. They strive to make their methods sufficiently rigorous that it is possible to regularly identify the flaws in their predictions. They have well-developed procedures for advancing understanding when hypotheses fail (Kacelnik and Krebs 1997:24,32; Winterhalder n.d.), an approach similar to that of ecology generally: "Incorrect theories or components of theory can have great heuristic value . . ." (PK&J 1994:118). This willingness to state hypotheses in terms that put them at risk of falsification is rare in ecological anthropology.

(5) Two important issues are raised in Joseph's section on derived conceptual devices. They affect not just HBE, but a broader range of schools in the social sciences. Joseph makes these charges: first, its methodology commits HBE to assuming what it is trying to demonstrate—that organisms are adapted—and second, this error is compounded by assuming, not demonstrating, that these adaptations achieve a state of optimality. Neither charge will survive a careful reading of the HBE literature.

The first of these related concerns evokes the issue of functionalism. Joseph repeats Vayda's (1995a, 1995b) criticism that HBE is an instance of naïve functionalism, or in her words "hyperfunctionalist post hoc accommodative reasoning" (p. 24). She, however, does not give examples. The charge of naïveté neglects the careful methodological attention given to the issue of functionalism by advocates of HBE (Winterhalder and Smith 1992:6-7, Smith and Winterhalder 1992:42-45), who generally have followed Elster (1982) on this point. Key elements of the methodology adopted by HBE have the role of minimizing the analytical problems associated with functionalism and the analysis of adaptation. They include the requirement that models of ecological circumstances be matched by those of evolutionary mechanism (Winterhalder 1997); the focus on selective mechanisms and micro-foundations; the MI focus on agents which (who) actually have adaptive agency; an insistence on clearly specified hypothesis testing; etc. If HBE *were* simply a matter of hyperfunctionalist, posthoc accommodative reasoning, it would not have a record of occasionally falsifying one or another of its hypotheses.

In his analysis of explanation in the social sciences Elster (1983:25-95) argues that neo-Darwinism is the *only* comprehensive theory presently capable of providing a logical justification for explanation by beneficial consequences. This may be true, but it is more important to HBE that neo-Darwinism provides quite specific guidelines and constraints within which such analyses can be framed (Smith and Winterhalder 1992). The temptations of functionalism, the degree to which the social sciences wittingly and unwittingly practice functionalism in its several forms, and the possible harmfulness of that, are worthy of lament (Turner and Maryanski 1979). The theoretical safeguards sought by HBE advocates should be cause for study and analysis, and where possible improvement, but that kind of effort is not found here. If Joseph has means of helping us or her fellow social scientists understand better than we do the issues of functionalism or adaptationism, it would be more productive for her to share those insights than to repeat epithets like "naïve."

Do advocates of HBE assume optimal adaptations or states in nature? No, although this is a common misunderstanding. Optimization is a theoretical premise justified by the directional tendency of selective processes; it provides a framework for generating testable predictions. It is thus quite different from the claim that some or another phenomena in nature ever or regularly achieve a state of optimality, and in fact, analyses based on the premise are necessary to determine the degree to which evolutionary outcomes may fall short of

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optimality. Maynard Smith and Parker are the often-cited, foundational papers on optimization: "The essential point is that in testing a model we are *not* testing the general proposition that nature optimizes, but the specific hypotheses about constraints, optimization criteria, and heredity" (Maynard Smith 1978:35, italics in original). Or, "Optimization models help us to test our insight into the biological constraints that influence the outcome of evolution. They serve to improve our understanding about adaptations, rather than to demonstrate that natural selection produces optimal solutions" (Parker and Maynard Smith 1990:27).[11] These same points are regularly stated in HBE publications (e.g., Smith and Winterhalder 1992:50-53, Winterhalder 1987:313-314, see also Foley 1985).

(6) Inadequacies of theory/framework—the general critiques of this section are two-fold: HBE adopts an evolution-supplanted-by-history view of human development (p. 21-22) and it's theory of environment is deficient (p. 22-24) to the point of being "absurd" (p. 23).

The first of these critiques prompts Joseph to pose (I presume to HBE) the rhetorical question: "when does evolutionary time and natural selection-driven change end and history and phenotypic adaptation, driven by "decision-making," begin? Does evolutionary time end with *Homo sapiens sapiens*, with the rise of culture during the Upper Paleolithic, or with the rise of agriculture or the rise of the state?" (p. 21). Because the idea that there is a Rubicon between evolution-without-culture and culture without-evolution hasn't been viable since Alfred Russell Wallace (1871), I suspect Joseph alludes here to the rather complex issue of dual inheritance (Boyd and Richerson 1985, Durham 1990) and the evolution of open behavioral programs (Mayr 1974). However, her rationale for representing it as a chronological matter of sequential causation rather than an analytical matter of interacting causation is a puzzle. Joseph apparently is unaware that there are HBE discussions of conceptual and other relationships between history and evolution (e.g., Boyd and Richerson 1992, Winterhalder 1994, Boone and Smith 1998).

On her second point, Joseph objects first to distinguishing behavior from the environment in which it occurs, and secondly to the practice in HBE of attending selectively only to certain elements of the environment. With respect to separation: "If the human social sciences have contributed anything to our understanding of human behavior over the last several hundred years, it is that human behavior is never context free and without structure" (p. 22). This is of course true. But as a truism, it also provides us no analytical guidance for analyzing behavior in context. We can acknowledge that in many cases behavior and context are reciprocally causal (as in the evolutionary and economic concept of a strategic environment; see Elster 1986:7), but this does not mean that we are logically precluded from distinguishing them for analytical purposes.

A clue as to why Joseph would think the previously quoted truism is damaging to HBE may reside in her second criticism. "In AEE, when the environment is alluded to, it is so artificially restricted as to be useless . . . Analyzing individual foraging behavior or any other behavior without embedding them in broader social, cultural, and biophysical contexts (graphically depicted in Figure 3) is absurd" (p. 23). Joseph appears to be objecting to delimiting a subset of variables from among the possible ones in order to pursue a focused analysis. As a general proposition, her point is a weak one. This kind of restriction is more-or-less synonymous with identifying a problem; most social science analyses extract from the array of many possibilities a subset to investigate. If, like HBE, they have a suitable framework for constructing and

[11] Optimization is assumed in HBE in the same special and limited way that most research traditions make high level assumptions not themselves directly subject to verification. For instance, "*research traditions are neither explanatory, nor predictive, nor directly testable*" (Laudan 1977: 81-82, italics in original) though their constituent theories are.

testing hypotheses, and for interpreting the results, it will become apparent if the selected restrictions rob the analysis of any ability to contribute to understanding.[12]

(7) Conclusion: Joseph ends her essay with this statement: ". . . what I am calling for is a new evolutionary ecology, one that goes beyond the apologetic capitalist world-view of biology" (p. 24). In as much as there is no explicit discussion in Joseph's essay of apologism, capitalism or the world-view of biology, it is difficult to read this statement except as a kind of code, one that calls up ideological disapproval based on undocumented allegations about the politics of a field and those who practice it. It accuses, but offers no content to which there can be a response.

Critique as Practice

We ought to encourage and prize critique, promote it as a craft and eagerly seek out instances that touch on our preferred academic haunts.[13] Most of us, I would guess, probably do not see criticism so favorably or expose ourselves to it so avidly. Surely in part this is because it can be hard on our egos. But I think it also is because facile or bad critique is far too common and perhaps even the norm in anthropology.[14] This situation prompts me to try to describe what I believe are the features of scientifically *effective* critique. By effective I mean something beyond rhetorical power—capacity to sway opinion or confirm prejudices—though that always is an element in writing. I mean effective in the sense of capacity to advance understanding through collective effort, effort that engages diverse experiences and viewpoints, kinds of knowledge, and skills in a dialog that encourages scientific progress. If my preliminary set of such features stimulates broader attention to this subject, it will serve its purpose.

I focus on the situation of criticism between differing scientific schools of thought, paradigms (Kuhn 1962), or the term I think best suits different fields in human ecology, "research traditions" (Laudan 1977:70-120). Nonetheless, some of my points bear on other forms of critique, such as short commentaries and book reviews. I will not rehash the more obviously objectionable critical techniques such as *ad hominem* attacks, or blatantly political or rhetorical exercises. These generally signal themselves in their choice of language.

(1) Effective critique has a well-defined and openly acknowledged perspective. In effect, it proceeds from a framework that is explicitly stated and that offers a platform for evaluation. If a review is framed in terms of philosophical questions, then it will be more effective if the author has a consistent, identified philosophical perspective. If a review is framed as a critique of a research tradition, then the author's research tradition allegiances should be stated. This need not be an onerous requirement, bane of page limitations, but it is an essential one.

On this feature, for instance, Joseph's critique of HBE gets a mixed review: the PK&J framework is a plus, but is not matched by willingness to adopt an explicitly stated alternative research tradition in human ecology.[15]

[12] Incidentally, the biologists Kacelnik and Krebs are badly misrepresented. Here is Joseph (p. 23-24): "given the position of some practitioners that "I personally find 'culture' unnecessary" (Betzig 1997: 17) or that "the latter [culture] is very accommodating: it does not get in the way of fitness maximization" (Kacelnik and Krebs 1997: 28), then there may be no attempt in the near future to seriously consider the role of culture." This comment makes it appear as if Kacelnik and Krebs (1997) are echoing Betzig's dismissal of culture. However, a careful reading of the text will show that in the extract quoted here they are making a charitable restatement of Betzig *in order to disagree with her*. They are not subtle about their own views; the boldface heading which appears just several lines above the section Joseph quotes is: "We, personally, find culture necessary" (1997: 27).

[13] The volume edited by Betzig (1997) is an interesting experiment in self-criticism.

[14] This is not an oblique comment on Joseph's review of HBE. I already have identified specific points on which we disagree about HBE; I should add that her critique avoids many of the pitfalls I think most compromise anthropological commentaries in general.

[15] ". . .the purpose of this paper is not to develop an alternative framework for the study of human ecology and evolution . . ." (p. 8; cf. Figure 2).

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(2) Effective critique is motivated by constructive objectives, explicitly revealed. An author's objectives in writing a critique are multiple, rarely stated, not always obvious, and not, I believe, of equal merit. Among other motivations, they include one or more of the wishes to: (a) inform a broad audience about an approach by establishing its current content and reach; (b) advance meta-understanding of a broad area of inquiry by placing an approach relative to others; (c) use the approach as a 'foil' to examine comparatively an alternative research tradition that shares one or more features; (d) instruct practitioners about shortcomings or unrealized possibilities and thus improve their practice; (e) dissuade a broader audience from attending to or pursuing work with a particular literature; (f) promote an alternative agenda; or, (g) confirm shared though unexamined preconceptions about an approach or its results. I would classify motivations "a" through "d" as more, and "e" through "g" as less, constructive.

(3) Effective critique begins by engaging the subject work on its own terms. This means taking seriously what the author is trying to accomplish. An essential and commonly neglected element of critique is the question: What are the author's self-identified objectives, and how well does the work succeed by them? Writing criticism by this admonition helps to insure that the reviewer strives for some internal understanding of the work before setting about identifying its errors or limitations.[16] This does not mean that criticism has to ignore whatever alternative terms or views the critic feels are worthwhile (see #1). It certainly is fair to complain about incomplete or inadequate treatment of the author's chosen topic. Shortcomings deserve exposure; unrecognized opportunities should be illuminated. But good critique, even devastatingly good critique, begins with a charitable and a careful reading.

(4) Effective critique represents accurately the practice of a field, no more or less. This sounds unexceptional, but much criticism eases its task by misrepresenting, overstating or exaggerating what is claimed by advocates on behalf of a field. Critics sometimes presume features that are not overt; impute goals or accomplishments that are not stated. This is usually done indirectly. Joseph provides several examples: "Thus, there is a need for AEE to bound the scope and scale of theoretical investigation. After all, no single theory can account for the entire range of variation and change in human sociocultural behaviors, structures, interactions, and flows across time and space" (p. 12). Or, "The important point is this: human behavioral outcomes (in this example foraging behaviors) are rarely if ever due to any fixed predetermined or innate characteristic of our species' decision making, but rather [are] contingent upon particular historical epochs and social structures . . ." (p. 15). There would be no need to say these things, except to imply—incorrectly—that HBE strives to be that single theory, or that it assumes human behavior is unaffected by historical epoch or social structure.

(5) Effective critique is based on first-hand familiarity with the history and substance of the work or field being reviewed. The converse is criticism of a field by citing isolated instances of exaggerations, dogmatisms or errors of logic and fact. Even solid, well defined and responsible fields of inquiry house individuals with a wide range of styles and proclivities to make bold, sometimes unguarded declarations. Every field has its strong and its weak applications. Most of us who have been writing for any length of time, and especially the more creative among us, can look back with a twinge of embarrassment at one or more incautious statements, however analytically conservative we may be otherwise. It is a relatively easy task to gather together and then condemn an intrinsically damning collection of these squibs, but they may tell us almost nothing about the substance of a field, the way in which it has developed, or its potential to add to understanding. Creationists do this to evolutionists to great rhetorical effect, but I am surprised and occasionally bewildered at how often scholars do it to their colleagues. Who can resist clobbering the

[16] Formally this corresponds to the method of rational reconstruction: "The striking feature of the method of rational reconstruction is the logical separation of the question of understanding a theory from the question of agreeing with it" (Wong 1978: 10).

easily scorned phrase, in place of a more demanding description and assessment of content and its history? Mature critique relies on knowing the history and primary literature of a field with sufficient thoroughness to winnow the careless remarks from the substance.

I would also include under this heading the benefits of avoiding critique by 'proxy.' I refer here to the practice of citing the summary statements (or epithets) of other reviewers, without the rationales. Effective critique engages primary sources. It does not rely on secondary accounts, either for critical points or for secondary summaries of content. Likewise, a more subtle form of this guideline would enjoin a critique to apprise how a concept or other element of theory or practice is applied in a field, quite apart from preconceptions about its suitability.

Another common violation of this feature of effective critique is criticism by the *possibility* of error, instead of a demonstration that the error actually occurs in practice with sufficient frequency or analytical importance to compromise a field of inquiry. Joseph provides an example: "The fundamental problem with proceeding from initial assumptions about the general consequences of evolutionary processes is that there is nothing to guard against subverting the empirical investigation of processes and mechanisms to suit that assumption" (p. 14). In general form, this statement is true of any research tradition. Given that possible mistakes are nearly infinite in variety, it would seem much more productive to focus on those that are, in fact, commonly made.

(6) Effective critique diagnoses problems in a manner that reveals their relative importance. Ideally it would also evaluate, at least in tentative terms, the hope for and likely manner of their resolution. This would contrast with critique that appears designed to smother its subject by enumerating a long and undifferentiated litany of flaws, without a systematic attempt to assess their significance. In effect, good criticism informs the reader what is blemish, what is a serious problem that might be rectified, and what may be a mortal defect. It would also indicate what are unsolved problems—common in any relatively new research tradition, offering opportunities for its further development—and what the author projects to be unsolvable problems.

A related issue is that of presenting internal debates among advocates seeking to improve a field as if they were seeking instead to critically dismiss it. For instance, someone unfamiliar with HBE would not know from reading Joseph that Mayr, Maynard Smith, and Bettinger are friendly critics and advocates, working to improve an approach they find promising. Vayda and Ingold, on the other hand, seek to convince the reader that key elements of HBE are hopelessly defective.

I think it fair to say that proponents of HBE are aware of most or all of the shortcomings mentioned by Joseph. For instance, ". . . most HBE work to date has neglected proximate analysis of the mechanisms guiding the adaptive behavior of individuals, be these rules of thumb, evolved psychological dispositions, or sociocultural inheritance. Likewise, it has neglected the actual histories by which such traits develop in populations" (Winterhalder and Smith 2000:67). But what she sees as failures—a word that implies unsuccessful attempts to solve—we would see as problems and issues awaiting the effort that may (or if one prefers to be more optimistic, likely will) lead either to their resolution or, if not that, to a better understanding of domain.

(7) Effective criticism is narrowly comparative. By this I mean that it puts one research tradition or theory up against another on topics or problems they have in common and makes a relative evaluation in terms of explicit criteria.[17] Much criticism does not offer and defend a more viable alternative (theory, concept, method) for whatever is identified as the shortcoming of the work under review. If it is not comparative, criticism can be remedial

[17] ". . . *the evaluation of theories is a comparative matter*. What is crucial in any cognitive assessment of a theory is how it fares with respect to its competitors. Absolute measures of the empirical or conceptual credentials of a theory are of no significance; decisive is the judgment as to how a theory stacks up against its known contenders" (Laudan 1977: 71, italics in original).

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but not dismissive; that is, it can identify areas where development is needed, and it may suggest ways to improve the research tradition under review, but it can not offer evidence that the tradition should be depreciated in importance or abandoned in favor of another. The astute reader will want to know, if not this, then what? And, what assurances are there that the alternative offered has better solutions to these problems, or that it does not entail serious problems of its own?[18]

In the case of HBE, this would suggest that critics are obligated to show how alternative research traditions do a better job of answering the questions specific HBE applications are attempting to answer, according to criteria like those devised by K&M. If conducted in a spirit consistent with other guidelines being suggested here, that would be an enlightening exercise, one I suspect favorable to HBE.

(8) Effective criticism exercises itself on case studies and empirical issues. This is not to say that high level elements of the research endeavor such as theory, concepts, research design, methods or interpretive issues are off limits. But, critiques that remain at a high level of generality or limit themselves to the realm of philosophical questions are less likely to engage their audience in productive ways. This may reflect the fractured state of the contemporary history and philosophy of science and the temptations of social scientists to overlay their own contentiousness onto the shifting uncertainties of that field. It may reflect the more general point that empirical content anchors debate in ways that more abstract elements do not. Exercising two research traditions on one or more case studies often is the most revealing way of locating their relative strengths and weaknesses.

As an example, Kelly's (1995) monographic-length review of the literature on human foragers is an excellent effort to integrate micro-level HBE explanation with large (spatial) scale generalizations about pattern in hunter-gatherer societies derived mainly from studies undertaken within the cultural ecology research tradition.

It is daunting to envision writing criticism according to the cumulative strictures of such guidelines I have suggested. Nonetheless, any movement in the directions they endorse will improve our collective ability to advance understanding of human ecology, especially through commentary on each other's research traditions.

Conclusions

Although Joseph does not identify herself with a specific paradigm or research tradition, I think it possible to offer, at least tentatively, some more general comments on sources of difference between her perspective and that of HBE.

Joseph wants holistic analyses that are comprehensive in the sense of simultaneously engaging all of the possible physical, biological, social and cultural variables, across all scales (e.g., her Figures 2-4). By contrast, followers of HBE generally believe it is both possible and important to work 'by pieces,' that is, to study in detail parts of problems, confident that the understanding obtained through that procedure will eventually find a place in the larger picture. I imagine Joseph and me picketing each other's research tradition with signs that read, respectively: "Holism must be comprehensively engaged from the beginning," and "Holism is a laudable goal but still a distant prospect." We disagree then on the variety of scales and number of variables that constitute a worthy and manageable research problem.

Joseph shares the general anthropological distrust of theory derived from evolutionary biology and economics, especially some of their conceptual assumptions and their emphasis on mechanism and micro-foundations. HBE, by contrast, sees these fields as valuable sources of theory and analytical tools, necessary but not necessarily sufficient for

[18] Here is an instance of the comparative nature of theory evaluation, directly stated: "Neoclassical economics will be dethroned if and when satisficing theory and psychology join forces to produce a simple and robust explanation of aspiration levels, or sociological theory comes up with a simple and robust theory of the relation between social norms and instrumental rationality. Until this happens, the continued dominance of neoclassical theory is ensured by the fact that one can't beat something with nothing" (Elster 1986: 26-27).

pursuing understanding of human behavior. HBE selectively adapts ideas from these sources because they have greater safeguards against some recurrent problems in the social sciences, such as group-level functionalism; it recognizes the intrinsic merit of practicing cross-disciplinary integration with fields like biology and economics. While it is quite interdisciplinary in general, contemporary anthropology generally is not approving of linkages that happen to go in this direction.

Finally, we disagree on how to assess progress in a nascent scientific endeavor. HBE is a young and, so far as the number of persons pursuing it, a quite small research tradition with an impressive record of accomplishment. There are areas of theoretical ambiguity (Have we escaped or only partially evaded the ills of functionalism? How do models of cultural inheritance affect the analysis of adaptation?), and a long list of things yet to be done, any of which might turn out to affect the endeavor in ways we have yet to apprehend. Followers of HBE are willing under some circumstances to count the failure of theoretically substantial hypotheses as a perfectly respectable way of advancing understanding within a research tradition (Winterhalder n.d.); Joseph tallies them as signals the tradition is deficient. In these and other ways revealed in the comparison of PK&J and K&M we are applying divergent evaluative criteria and for fairly obvious reasons coming to divergent conclusions.

PK&J point out that integration of bio-ecological paradigms would require that each be highly developed, whereas some are "in a state of conceptual infancy" (p. 142). They add that "integration across paradigms may be limited if theory in one of the areas is poorly developed . . ." (p. 143), and "All paradigms must be represented by clear, complete theories before they can be integrated" (p. 147). I conclude by asking the reader to contemplate the comparable prospects for integrating, or at least for understanding the respective strengths of the various research traditions in human ecology. How well are cultural ecology (Steward 1955), ecological anthropology (Vayda and McCay 1975), political ecology (Greenberg and Park 1994), historical ecology (Balée 1998) and other approaches equipped in this regard? Despite its relatively brief history, HBE would bring to such a 'place-finding' comparison a more self-consciously delimited, formally developed and rigorously tested theoretical framework than most of its sibling traditions. But demonstrating something like that would require that we collectively have much greater experience with the kind of comparative, effective critical evaluation that I hope that this paper will promote.

References Cited

BALÉE, W.
1998 "Introduction," in *Advances in historical ecology*. Edited by W. Balée, pp. 1-10. New York: Columbia University Press.

BETTINGER, R. L.
1991 *Hunter-gatherers: Archaeological and evolutionary theory*. New York: Plenum Press.

BETTINGER, R. L., R. MALHI, AND H. MCCARTHY.
1997 Central place models of acorn and mussel processing. *Journal of Archaeological Science* 24:887-899.

BETZIG, L., EDITOR.
1997 *Human nature: A critical reader*. Oxford: Oxford University Press.

BETZIG, L.
1997 "Introduction: People are animals," in *Human nature: A critical reader*. Edited by L. Betzig, pp. 1-17. Oxford: Oxford University Press.

BIRD, D. W., AND R. L. BLIEGE BIRD.
1997 Contemporary shellfish gathering strategies among the Meriam of the Torres Strait Islands, Australia: Testing predictions of a central place foraging model. *Journal of Archaeological Science* 24:39-63.

BLURTON JONES, N.
1986 Bushman birth spacing: A test for optimal interbirth intervals. *Ethology and Sociobiology* 7:91-105.
1987 Bushman birth spacing: Direct tests of some simple predictions. *Ethology and Sociobiology* 8:183-203.

1997 "Too good to be true? Is there really a trade-off between number and care of offspring in human reproduction?" in *Human nature: A critical reader*. Edited by L. Betzig, pp. 83-86. Oxford: Oxford University Press.

BOONE, J. L., AND E. A. SMITH.
1998 Is it evolution yet? A critique of evolutionary archaeology. *Current Anthropology* 39:S141-S173.

BOYD, R., AND P. J. RICHERSON.
1985 *Culture and the evolutionary process*. Chicago: University of Chicago Press.
1992 "How microevolutionary processes give rise to history," in *History and evolution*. Edited by M. H. Nitecki and D. V. Nitecki, pp. 179-209. Albany: State University of New York Press.

BOYER, P.
1995 "Ceteris paribus (All else being equal)," in *How things are: A science tool-kit for the mind*. Edited by J. Brockman and K. Matson, pp. 169-175. New York: William Morrow and Company.

BROUGHTON, J. M.
1994a Declines in mammalian foraging efficiency during the Late Holocene, San Francisco Bay, California. *Journal of Anthropological Archaeology* 13:371-401.
1994b Late Holocene resource intensification in the Sacramento Valley, California: The vertebrate evidence. *Journal of Archaeological Science* 21:501-514.
1997 Widening diet breadth, declining foraging efficiency, and prehistoric harvest pressure: Ichthyofaunal evidence from the Emeryville Shellmound, California. *Antiquity* 71:845-862.

BROUGHTON, J. M., AND J. F. O'CONNELL.
1999 On evolutionary ecology, selectionist archaeology, and behavioral archaeology. *American Antiquity* 64:153-165.

CARO, T., EDITOR.
1998 *Behavioral ecology and conservation biology*. Oxford: Oxford University Press.

CHAGNON, N. A., AND W. IRONS, EDITORS.
1979 *Evolutionary biology and human social behavior: An anthropological perspective*. North Scituate, MA: Duxbury Press.

CRONK, L., N. CHAGNON, AND W. IRONS, EDITORS.
2000 *Adaptation and human behavior: An anthropological perspective*. Hawthorne, NY: Aldine de Gruyter.

DURHAM, W. H.
1990 Advances in evolutionary culture theory. *Annual Review of Anthropology* 19:187-210.

ELSTER, J.
1982 Marxism, functionalism, and game theory: The case for methodological individualism. *Theory and Society* 11:453-482.
1983 *Explaining technical change: A case study in the philosophy of science*. Cambridge: Cambridge University Press.
1985 *Making sense of Marx*. Cambridge: Cambridge University Press.
1986 "Introduction," in *Rational choice*. Edited by J. Elster, pp. 1-33. New York: New York University Press.

FOLEY, R.
1985 Optimality theory in anthropology. *Man* 20:222-242.

GOULD, S. J., AND R. C. LEWONTIN.
1979 The spandrels of San Marco and the Panglossian paradigm: A critique of the adaptationist programme. *Proceedings of the Royal Society of London*, Series B 205:581-598.

GRAYSON, D. K., AND F. DELPECH.
1998 Changing diet breadth in the early Upper Paleolithic of southwestern France. *Journal of Archaeological Science* 25:1119-1129.

GREENBERG, J. B., AND T. K. PARK.
1994 Political ecology. *Journal of Political Ecology* 1:1-12.

HAMES, R.
2000 Reciprocal altruism in Yanomamö food exchange. In *Adaptation and human behavior: An anthropological perspective*. Edited by L. Cronk, N. Chagnon, and W. Irons, pp. 397-416. Hawthorne, NY: Aldine de Gruyter.

HAWKES, K.
1991 Showing off: Tests of an hypothesis about men's foraging goals. *Ethology and Sociobiology* 12:29-54.

HAWKES, K., J. F. O'CONNELL, AND N. G. BLURTON JONES.
1995 Hadza children's foraging: Juvenile dependency, social arrangements, and mobility among hunter-gatherers. *Current Anthropology* 36:688-700.
1997 Hadza women's time allocation, offspring provisioning, and the evolution of long postmenopausal life spans. *Current Anthropology* 38:551-577.

HILL, K.
1988 Macronutrient modifications of optimal foraging theory: An approach using indifference curves applied to some modern foragers. *Human Ecology* 16:157-197.

HILL, K., AND A. M. HURTADO.
1996 *Ache life history: The ecology and demography of a foraging people*. New York: Aldine de Gruyter.

HILL, K., AND H. KAPLAN.
1999 Life history traits in humans: Theory and empirical studies. *Annual Review of Anthropology* 28:397-430.

HRDY, S. BLAFFER.
1999 *Mother nature: A history of mothers, infants, and natural selection*. New York: Pantheon Books.

INGOLD, T.
1996 "The optimal forager and economic man," in *Nature and society: Anthropological perspectives*. Edited by P. Descola and G. Pálsson, pp. 25-44. London: Routledge.

JOSEPH, S.
2000 Anthropological evolutionary ecology: A critique. *Journal of Ecological Anthropology* 4:6-30.

KACELNIK, A., AND J. R. KREBS.
1997 "Yanomamö dreams and starling payloads: The logic of optimality," in *Human nature: A critical reader*. Edited by L. Betzig, pp. 21-35. Oxford: Oxford University Press.

KAPLAN, H., K. HILL, J. LANCASTER, AND A. M. HURTADO.
2000 A theory of human life history evolution: Diet, intelligence, and longevity. *Evolutionary Anthropology* 9:156-185.

KELLY, R. L.
1995 *The foraging spectrum: Diversity in hunter-gatherer lifeways*. Washington: Smithsonian Institution Press.

KOHLER, T. A.
2000 "Putting social sciences together again: An introduction to the volume," in *Dynamics in human and primate societies: Agent-based modeling of social and spatial processes*. Edited by T. A. Kohler and G. J. Gumerman, pp. 1-18. Oxford: Oxford University Press.

KUHN, T. S.
1962 *The structure of scientific revolutions*. Chicago: University of Chicago Press.
1977 "Objectivity, value judgment, and theory choice," in *The essential tension: Selected studies in scientific tradition and change*. Edited by T. S. Kuhn, pp. 320-339. Chicago: University of Chicago Press.

KUZNAR, L. A.
2000 Application of general utility theory for estimating value in non-Western societies. *Field Methods* 12:334-345.

LANCASTER, J. B., AND H. S. KAPLAN.
2000 "Parenting other men's children: Costs, benefits, and consequences," in *Adaptation and human behavior: An anthropological perspective*. Edited by L. Cronk, N. Chagnon, and W. Irons, pp. 179-201. New York: Aldine de Gruyter.

LAUDAN, L.
1977 *Progress and its problems: Towards a theory of scientific growth*. Berkeley: University of California Press.
1990 *Science and relativism: Some key controversies in the philosophy of science*. Chicago: University of Chicago Press.

https://digitalcommons.usf.edu/jea/vol6/iss1/1 | DOI: http://dx.doi.org/10.5038/2162-4593.6.1.1

LESLIE, P., AND B. WINTERHALDER.
2001 Demographic consequences of unpredictability in fertility outcomes. *American Journal of Human Biology* (in press).

LIESEN, L. T.
1998 The legacy of woman the gatherer: The emergence of evolutionary feminism. *Evolutionary Anthropology* 7:105-113.

LOW, B. S.
2000 *Why sex matters: A Darwinian look at human behavior*. Princeton, NJ: Princeton University Press.

LUTTBEG, B., M. BORGERHOFF MULDER, AND M. MANGEL.
2000 "To marry again or not: A dynamic model for demographic transition," in *Adaptation and human behavior: An anthropological perspective*. Edited by L. Cronk, N. Chagnon, and W. Irons, pp. 345-368. New York: Aldine de Gruyter.

MACE, R.
1993 Nomadic pastoralists adopt subsistence strategies that maximize long-term household survival. *Behavioral Ecology and Sociobiology* 33:329-334.
1996 When to have another baby: A dynamic model of reproductive decision-making and evidence from Gabbra pastoralists. *Ethology and Sociobiology* 17:263-274.

MADSEN, D. B., AND D. N. SCHMITT.
1998 Mass collecting and the diet breadth model: A Great Basin example. *Journal of Archaeological Science* 25:445-455.

MAYNARD SMITH, J.
1978 Optimization theory in evolution. *Annual Review of Ecology and Systematics* 9:31-56.

MAYR, E.
1974 Behavior programs and evolutionary strategies. *American Scientist* 62:650-659.
1983 How to carry out the adaptationist program? *The American Naturalist* 121:324-334.

MCMULLIN, E.
1983 Values in science. *Philosophy of Science Association* 2:3-28.

O'CONNELL, J. F., K. HAWKES, AND N. G. BLURTON JONES.
1999 Grandmothering and the evolution of *Homo erectus*. *Journal of Human Evolution* 36:461-485.

ODUM, E. P.
1969 The strategy of ecosystem development. *Science* 164:262-270.

PARKER, G. A., AND J. MAYNARD SMITH.
1990 Optimal theory in evolutionary biology. *Nature* 348:27-33.

PICKETT, S. T. A., J. KOLASA, AND C. G. JONES.
1994 *Ecological understanding: The nature of theory and the theory of nature*. San Diego: Academic Press.

PIPERNO, D. R., AND D. M. PEARSALL.
1998 *The origins of agriculture in the lowland neotropics*. San Diego: Academic Press.

PIROLLI, P., AND S. CARD.
1999 Information foraging. *Psychological Review* 106:643-675.

RICHERSON, P. J.
1977 Ecology and human ecology: A comparison of theories in the biological and social sciences. *American Ethnologist* 4:1-26.

RUTTAN, L. M., AND M. BORGERHOFF MULDER.
1999 Are East African postoralists truly conservationists? *Current Anthropology* 40:621-652.

SANDSTROM, P. E.
1994 An optimal foraging approach to information seeking and use. *Library Quarterly* 64:414-449.

SELZER, J., EDITOR.
1993 *Understanding scientific prose*. Madison, WI: University of Wisconsin Press.

SMITH, E. A.
1984 "Anthropology, evolutionary ecology, and the explanatory limitations of the ecosystem concept," in *The ecosystem concept in anthropology*. Edited by E. F. Moran, pp. 51-85. Boulder, CO: Westview Press.

1996 Human life history comes of age. *Evolutionary Anthropology* 5:181-185.

2000 "Three styles in the evolutionary analysis of human behavior," in *Adaptation and human behavior: An anthropological perspective*. Edited by L. Cronk, N. Chagnon, and W. Irons, pp. 27-46. New York: Aldine de Gruyter.

SMITH, E. A., AND B. WINTERHALDER.

1992 "Natural selection and decision-making: Some fundamental principles," in *Evolutionary ecology and human behavior*. Edited by E. A. Smith and B. Winterhalder, pp. 25-60. New York: Aldine de Gruyter.

SMITH, E. A., M. BORGERHOFF MULDER, AND K. HILL.

2001 Controversies in the evolutionary social sciences: A guide for the perplexed. *Trends in Ecology and Evolution* 16:128-135.

SOSIS, R.

2000 "The emergence and stability of cooperative fishing on Ifaluk Atoll," in *Adaptation and human behavior: An anthropological perspective*. Edited by L. Cronk, N. Chagnon, and W. Irons, pp. 437-472. New York: Aldine de Gruyter.

STEWARD, J. H.

1955 "The concept and method of cultural ecology," in *Theory of culture change: The methodology of multilinear evolution*. Edited by J. H. Steward, pp. 30-42. Urbana: University of Illinois Press.

SUTHERLAND, W. J.

1996 *From individual behaviour to population ecology*. Oxford: Oxford University Press.

TURNER, J. H., AND A. MARYANSKI.

1979 *Functionalism*. Menlo Park, CA: Benjamin/Cummings Publishing Co.

TURNER, J. H., M. BORGERHOFF MULDER, L. COSMIDES, B. GIESEN, G. HODGSON, A. M. MARYANSKI, S. J. SHENNAN, J. TOOBY, AND B. M. VELICHKOVSKY.

1997 "Looking back: Historical and theoretical context of present practice," in *Human by nature: Between biology and the social sciences*. Edited by P. Weingart, S. D. Mitchell, P. J. Richerson, and S. Maasen, pp. 17-64. Mahwah, NJ: Lawrence Erlbaum Associates.

VAYDA, A. P.

1983 Progressive contextualization: Methods for research in human ecology. *Human Ecology* 11:265-281.

1995a Failures of explanation in Darwinian ecological anthropology: Part I. *Philosophy of the Social Sciences* 25:219-249.

1995b Failures of explanation in Darwinian ecological anthropology: Part II. *Philosophy of the Social Sciences* 25:360-375.

VAYDA, A. P., AND B. J. MCCAY.

1975 New directions in ecology and ecological anthropology. *Annual Review of Ecology and Systematics* 4:293-306.

VOLAND, E.

1998 Evolutionary ecology of human reproduction. *Annual Review of Anthropology* 27:347-374.

WALLACE, A. R.

1871 *The action of natural selection on man*. New Haven, CT: Charles C. Chatfield and Company.

WEINGART, P., S. D. MITCHELL, P. J. RICHERSON, AND S. MAASEN, EDITORS.

1997 *Human by nature: Between biology and the social sciences*. Mahwah, NJ: Lawrence Erlbaum Associates.

WILSON, M., AND M. DALY.

1997 Life expectancy, economic inequality, homicide, and reproductive timing in Chicago neighbourhoods. *British Medical Journal* 314:1271-1274.

https://digitalcommons.usf.edu/jea/vol6/iss1/1 | DOI: http://dx.doi.org/10.5038/2162-4593.6.1.1

WINTERHALDER, B.

1977 Foraging strategy adaptations of the boreal forest Cree: An evaluation of theory and models from evolutionary ecology. Ph.D. Dissertation, Cornell University, Ithaca, NY.

1983 "Boreal foraging strategies," in *Boreal forest adaptations: The Northern Algonkians*. Edited by A. T. Steegmann, Jr., pp. 201-241. New York: Plenum Press.

1986 Diet choice, risk, and food sharing in a stochastic environment. *Journal of Anthropological Archaeology* 5:369-392.

1987 "The analysis of hunter-gatherer diets: Stalking an optimal foraging model," in *Food and evolution: Toward a theory of human food habits*. Edited by M. Harris and E. B. Ross, pp. 311-339. Philadelphia: Temple University Press.

1994 "Concepts in historical ecology: The view from evolutionary ecology," in *Historical ecology: Cultural knowledge and changing landscapes*. Edited by C. L. Crumley, pp. 17-41. Santa Fe, NM: School of American Research Press.

1996 A marginal model of tolerated theft. *Ethology and Sociobiology* 17:37-53.

1997 Gifts given, gifts taken: The behavioral ecology of nonmarket, intragroup exchange. *Journal of Archaeological Research* 5:121-168.

n.d. "Models," in *A handbook of concepts in modern evolutionary archaeology*. Edited by J. P. Hart and J. E. Terrell. New York: Greenwood Publishing Group (in press).

WINTERHALDER, B., AND E. A. SMITH, EDITORS.

1981 *Hunter-gatherer foraging strategies: Ethnographic and archaeological analyses*. Chicago: University of Chicago Press.

WINTERHALDER, B., AND E. A. SMITH.

1992 "Evolutionary ecology and the social sciences," in *Evolutionary ecology and human behavior*. Edited by E. A. Smith and B. Winterhalder, pp. 3-23. New York: Aldine de Gruyter.

2000 Analyzing adaptive strategies: Human behavioral ecology at twenty-five. *Evolutionary Anthropology* 9:51-72.

WINTERHALDER, B., W. BAILLARGEON, F. CAPPELLETTO, I. R. DANIEL JR., AND C. PRESCOTT.

1988 The population ecology of hunter-gatherers and their prey. *Journal of Anthropological Archaeology* 7:289-328.

WINTERHALDER, B., F. LU, AND B. TUCKER.

1999 Risk-sensitive adaptative tactics: Models and evidence from subsistence studies in biology and anthropology. *Journal of Archaeological Research* 7:301-348.

WONG, S.

1978 *The foundations of Paul Samuelson's revealed preference theory: A study by the method of rational reconstruction*. London: Routledge and Kegan Paul.