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Reply to Winterhalder's Behavioral and Other Human Ecologies: Critique, Response and Progress through Criticism

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Introduction

In anthropology, the primary goal is understanding the human condition, what makes humans human or explaining human variation and change. To critically assess what a given theory has to offer anthropological attempts to understand the human condition requires a detailed understanding of a theory and its components. For some it may seem that there is a contradiction between the goal of theory-building and that of holism. After all, theories often make generalizations and attempt to simplify in order to understand. However, holism, as a fundamental principle of anthropology, goes for the big picture. That is, holism emphasizes that in order to understand a cultural system one must understand the "relations," "webs," or "interactions" between different subsystems or institutions—social, political, economic, religious, etc. Holism is invaluable in guarding against overly facile theoretical conclusions that are based on partial or single aspects of the human condition. While holism does not prevent retaining elements of simplicity in understanding human variation and change, simplifications or theoretical generalizations should be consistent with general anthropological insights as to what it means to be human. Attention to scale is crucial, for some simplifications might only apply to interactions at particular levels of human social organization. And, sometimes, theoretical simplifications may turn out to be flawed.

So what about Anthropological Evolutionary Behavioral Ecology (AEBE)? Theories or paradigms which rest on unverified assumptions and faulty premises are likely to produce questionable simplifications. In AEBE, many simplifications or generalizations about human behavior are based on unverified assumptions about optimality or adaptation. Similarly, theoretical interpretations of human behaviors rest on premises as to the meaning of "reproductive success" and the organism/environment dichotomy, which are untenable in human *sociocultural* systems. Theories which seek to establish a universal predictable human nature or identify a single currency (such as reproductive success) for understanding human behavior without seriously considering the role of culture and historical difference are especially prone to erroneous simplification. Likewise, theories that place primary emphasis on prediction are likely to be limited. Predictive success cannot be used as the principal criteria for evaluating a theory. The reason being that disparate theories often make similar predictions. The more important question is how well does the theory *explain* empirical observations? In order to address this question we must turn to the conceptual roster of theory. The question then becomes: does the theory have rich, well-developed or elaborated concepts and framework for explaining cultural and ecological difference?

In the case of AEBE, the principal focus is on simple predictive models. No serious attention is given to the role of culture. Most empirical observations are interpreted in light of the biological process of natural selection. It is virtually impossible to challenge or contain adaptationist arguments in AEBE because adaptation is assumed. This means that explanations of successful predictions rest on functionalist reasoning. And because no strict conditions or rigorous requirements must be met before invoking adaptationism (the "theo-

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retical safeguards" mentioned in Winterhalder's response do not hold up to careful scrutiny), adaptationist arguments are virtually unbounded or limitless. In short, the expansion of the domain of AEBE is almost limitless. Can such a theory contribute to our understanding of human variation and change? The likely answer is yes, in some ways; but its explanatory potential is likely to be rather limited, particularly when it comes to understanding human *sociocultural* systems. As a result, I conclude in this reply (as in my original critique) that it is necessary to delimit the domain of AEBE. I further indicate that in order to understand the complex behavioral patterning of our species in both time and space, more robust theoretical structure than that found within AEBE is needed.

The reply is divided into three sections. In *Section I: Reply to Winterhalder's Comments on the Critique Itself*, I further utilize the Pickett et al. (1994) method for theory to address Winterhalder's response to my original critique, with emphasis on major theoretical flaws raised in my critique of AEE and reexamined in light of Winterhalder's arguments. In terms of suggestions for refining AEE or AEBE, my suggestions, while not antithetical to Winterhalder's, contain some important disagreements. For one, Winterhalder suggests that AEBE's theoretical shortcomings can be addressed through more careful empirical research. However, I argue that many of the problems with AEBE stem from inadequate basic conceptual devices and theoretical framework. Theoretical concepts and frameworks interpret and give meaning to empirical phenomena. Likewise, empirical phenomena can often lead to the development of additional concepts or frameworks. Thus, it is essential that we examine how conceptual and empirical components of theory interact and mutually affect one another. Otherwise, we are left with an inadequate understanding of the potential and limitations of a given theory. In *Section II: Reply to Winterhalder's Comments on Critique as Practice*, I make some brief comments on Winterhalder's suggestions for writing effective critique. In particular, I highlight the general uses and limitations of critique toward theory-building and refinement. And in the final section, *Section III: Toward a More Inclusive Human Evolutionary Ecology*, I indicate the need for a more synthetic human evolutionary ecology—one that includes a more holistic framework for understanding human ecosystems.

Section I: Reply To Winterhalder's Comments on the Critique Itself

In critically evaluating AEBE, I employ the Pickett et al. (1994) method for theory (referred to as the PK&J method for theory). Thus, while I share Winterhalder's desire to achieve progress through criticism, I disagree that the PK&J method for theory is too complex. Actually, the PK&J method for theory is quite simple. Pickett et al. (1994) identify four major components of theory (for a list and definition of components see Box 1 Joseph 2000:7): (1) *Conceptual Devices* (i.e., assumptions, definitions, concepts); (2) *Empirical Content* (observations, facts, confirmed generalizations); (3) *Derived Conceptual Devices* (hypotheses, models, theorems); and (4) *Theory Framework and Structure* (framework, domain and translation modes). The columns in Figure 1 in Joseph (2000:7), simply characterize the level of development of theory (for definitions of different stages of development see Box 2 Joseph 2000:8). That is, well-developed theory has clear derived conceptual constructs, richer and more explicit conceptual and empirical components, more thoroughly refined overall components, and greater clarification of their interrelationship. Perhaps the most important contribution of the PK&J method for theory is that it highlights the relationship between different components of theory, particularly the link *between the empirical and conceptual components*. Without an appreciation of this link, scientific explanation in anthropology remains elusive. Explanation is defined as the "act of relating conceptual constructs to observable phenomena" (Pickett et al. 1994:31). Another strength of the PK&J method for theory is that it has incorporated fundamental developments in the history of science. The new philosophy of science recognizes that theory can take plural forms and is not reducible to the "statement view" of theory (which took the stated laws of motion from classical Newtonian physics as the primary model for all scientific theories). Scientific theories now incorporate multiple causality (Salmon 1984), emphasize *explanation* (as defined above), incorporate multiple methods for confirmation of theory (Lloyd 1988) (in addition to falsification), and examine *historical* influences on phenomena under study (see Pickett et al. 1994:12-19). Thus, it is not so much that criteria from Kuhn (1977) and McMullin (1983) are incorrect but incomplete and in need of revisions which incorporate additional key developments in the history of science.

My original critique refers to Evolutionary Behavioral Ecology, often referred to as evolutionary ecology and which I term Anthropological Evolutionary Ecology (AEE) to differentiate from Biological Evolutionary Ecology. The important point is that the critique does not include Evolutionary Psychology or Boyd and Richerson's (1985) dual inheritance theory. While these might be considered sister disciplines, there are still some important theoretical distinctions between them. (For a critique of Evolutionary Psychology see Lloyd 2001.) Winterhalder conceives of evolutionary psychology and dual inheritance theory as being part of a larger theoretical family of evolutionary ecology. Either way, the important point is that we agree on what is and is not included in the critique. It seems that Anthropological Evolutionary Behavioral Ecology (AEBE) might be more amenable to Winterhalder's conceptualization, and thus I use it throughout the reply.

My major criticism of AEBE is that the domain[3] is much narrower than its current conceptualization. To examine the domain of theory is not to partake in bleak or obscure philosophical blather, but to examine a specific and indispensable component of theory. As PK&J state: "The first explicit component of theory that must be specified is its domain. Without a stated domain, it will not be clear to what the theory applies, nor how the universe [of discourse] might expand, contract, or subdivide as the theory is refined and tested" (Pickett et al. 1994:91). The domain of theory is not static. It changes as concepts are refined and elaborated and as empirical-conceptual components of theory inform one another. However, by locating or relocating (as theory develops) the scale in time and space to which a theory belongs, we have better clues as to its potential applications or misapplications. PK&J sum it up best: "Domain must be specified as explicitly as possible, otherwise important assumptions about the nature and function of phenomena may remain hidden . . . Spatial and temporal scale and hierarchical level of organization are critical aspects of the scale of a domain. Failing to specify them may result in misapplication of the theories or models" (1994:33).

Because AEBE as a paradigm relies heavily on Darwin's biological theory of natural selection, one might be tempted to think that it has universal applicability. But even in biology, the proper domain of the Darwinian theory of natural selection is being examined and refined. There is a growing understanding and appreciation that the Darwinian process of natural selection does not adequately explain salient empirical examples of biological evolutionary change (see Joseph 2000:10-12). Winterhalder's contention that my examples of theoretical refinements to the scope of Darwinian theory of natural selection "have no purchase" since neo-Darwinian theory will provide the "*necessary* micro-foundations for understanding natural processes at all scales" (Winterhalder 2002:9) misses the point. The important point is that there *are* important refinements being made and biological scientists are beginning to better appreciate the multiple conditions under which evolution by natural selection will occur. That is, biological sci-

[3] Winterhalder (2002:6 footnote 3) incorrectly states that "domain" is not a component of theory in PK&J's method for theory. It is the first component of theory mentioned in the "Components of Theory" (see Pickett et al. 1994:59, 82-83, 91, 32). Likewise, "theorem" was not added but can either be listed independently or subsumed under "models" (see Pickett et al. 1994:71-72). "Laws" was removed, however, because it is generally recognized by social scientists that laws in human social systems take such a highly contingent form that "confirmed generalizations" may be more apt for describing empirically confirmed relationships, constraints and dynamics in human ecosystems.

entists now recognize the *domain* of Darwinian theory, which includes an understanding of its *limitations*. These developments in evolutionary biology (and others from the study of hierarchy in ecology) also suggest that there is no expectation "that higher-level explanations in biology, psychology, economics, and sociology ultimately will be reduced to, or eliminated by, micro-explanations" (Trout 1991:391). That is, better appreciation for the scope or domain of theory implies that there is no need for reductionism, the requirement that a correct explanation can only be achieved by reducing a situation to the lowest possible hierarchical level. The current approach to hierarchy theory in ecology suggests that fundamental questions can be asked at any level of organization so that the explanation of multiple interactions at one level requires explanations of constraints found at broader scales, while mechanisms for those interactions will be found at a finer level (either below the level of immediate interest or close to it) (see Pickett et al. 1994:23-24, Allen and Hoekstra 1992). Either way, there is an important need in anthropology to limit the domain of neo-Darwinian theory and hence AEBE (since the cornerstone of explanation in AEBE is the Darwinian theory of adaptation by natural selection). An even more pressing reason to limit the domain of AEBE is that ecological anthropologists are faced with the additional challenge of understanding cultural evolution.

Winterhalder disagrees that the domain of AEBE is too broad. He provides examples of qualifying statements from his work and from his and Smith's work (see Winterhalder 2002:8,11) which sufficiently demonstrate a recognition (early in theory development) that the domain of AEBE was limited. Such statements, which acknowledge theoretical limitations, are exceptions which only prove the rule, since they did not lead to the restriction but rather an *expansion* of the domain of AEBE. Winterhalder recognizes that behaviors are complex (i.e., "Foraging behavior is complex," quoted in Winterhalder 2002:8). He also acknowledges that AEBE models are simple: "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" (Winterhalder 2002:4). But there is no parallel recognition within AEBE that formal models and unverified assumptions of these models seriously restrict or limit the explanatory scope of theory. Rather, the simplicity of formal models (with their unverified assumptions of optimality or adaptive efficiency) have provided ample and seemingly limitless opportunity for generating hypotheses and "expanding their domain [that of AEBE] as experience warrants" (Winterhalder 2002:9).

And within AEBE, experience seems to have warranted some rather major expansions. The far-reaching applicability of simple models seems to have come as an unexpected surprise to its practitioners. Winterhalder writes: "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" (2002:8). Other expansions of the domain include questions of "resource distribution . . . social hierarchy . . . hominid origins" and "multiple other topics" (Winterhalder 2002:5).

Winterhalder goes on to argue that AEBE forays into new domains of inquiry (such as demographic transition or fertility variation) are simply a matter of applying the theory to different "topical" areas. However, these topical areas deal with fundamental questions at specific time scales and levels of social organization. For example, demographic transition is a multi-leveled process and one that Fricke (1990:117) reminds us has only occurred among a culture-bearing species. So that to causally "explain" demographic transitions, for example, requires at a minimum a specification of 1) the time-scales (transition is a historical process after all) and spatial-scales or levels of social organization of the transition in human history with which a researcher is interested (e.g., individuals; class/ethnic groups; village/community; nation-state; macroregion/world-systems); 2) the socio-cultural and ecological dynamics in fertility, mortality or migration at a certain scale(s) with which a researcher is concerned (such interactions or dynamics may or may not be causally connected); 3) the broader constraints or possibilities (e.g., historical, socio-cultural and political structures) which help explain the interactions or patterns of interest at a given level(s) to the researcher; and 4) attention to finer scale(s) where biocultural mechanisms for those patterns or interactions at specific points in time and space are to be found.

AEBE with its strict methodological individualism, reductionism and insufficient attention to the importance of scale, as well as its emphasis on the neo-Darwinian biological concept of adaptation as an explanatory concept, can only contribute limited insights to our understanding of demographic transitions. To slightly digress, does this mean that a theory or theorist must always be holistic and invoke multiple scales and multiple environments in order to make a contribution to fundamental theoretical questions? No, but we have to do a better job of specifying the domain of theory so we can better assess where a given theory's contribution to fundamental questions in anthropology *does lie*. For example, biological anthropologists and demographers who examine the proximate determinants of fertility in a given population (see Bongaarts and Potter 1983, Campbell and Wood 1988, Wood 1990) are clearly contributing to our understanding of the mechanisms involved in demographic variation and change. Without an understanding of the biocultural mechanisms of fertility within and between populations, we are left with an incomplete causal explanation of demographic transitions.

Winterhalder acknowledges the expansion of the domain of AEBE, but paradoxically does not recognize this "expansion" as a question of scale.[4] Perhaps this explains why Winterhalder ultimately questions the importance of trying to establish domain itself: "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" (2002:9).

The expansion of the domain of AEBE should come as no surprise to its practitioners. It is both foreseeable and predictable; it is built into the theory itself. *A priori* assumptions of adaptationism allow for virtually limitless expansion of the domain of AEBE. Thus, the debate over the domain of AEBE can only be resolved by confronting the use of adaptation as a concept and framework within AEBE. The Darwinian theory of natural selection does not have unlimited applications to our understanding of the human condition. As social scientists, we have to guard against uncritical and improper use of the concept of adaptation as a kind of "Open Sesame" (see Hallpike 1988) to "explain" every unusual practice or institution (see Joseph 2000:20). Winterhalder seems to recognize that this is a serious issue. He reproaches me for not offering insights and even accuses me of using epithets: "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.'" (2002:12).

First, I do not use "epithets." If Winterhalder is referring to my discussion of Vayda's criticism of "naïve functionalism," this is not an epithet, but highlights an important weakness of the theoretical framework (see Joseph 2000:19-20). Second, I offer suggestions that specific components of theory

[4] Winterhalder mentions scale with regard to life history theory only to question its importance as a potential limitation of the subtheory. He states that "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" (2002:12). However, in their research on life expectancy, socio-economic inequality, homicide and reproductive timing in Chicago neighborhoods, Wilson and Daly (1997) do not unambiguously claim to be causally explaining their findings in terms of life history theory. The authors are more cautious: "The number of likely feedback loops among the phenomena of interest is daunting" (1997:1274). Their discussion illustrates the problem with applying natural selectionism (with its strict dichotomy between organismic variation and environmental selection—a necessary distinction for separating evolutionary theory from teleological arguments) to the study of human *sociocultural* systems where the organism/environment distinction is untenable (for a discussion see Joseph 2000:22; see also footnote 8 in this paper).

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be rejected or revised and discuss an important "safeguard" against facile adaptationism which I will revisit later in my response. Nevertheless, Winterhalder does not take these suggestions seriously. Thus, before discussing suggestions for improving AEBE, let us examine the "theoretical safeguards" already present to which Winterhalder refers.

Perhaps the most interesting point Winterhalder raises is that my critique fails to appreciate the limited use of adaptation or optimal adaptations as assumptions or concepts in AEBE (2002:12), which inform derived conceptual devices (formal models and hypotheses), and in turn affect the interpretation of empirical observations. Winterhalder states that such assumptions are "justified by the directional tendency of selective processes; it [optimization] provides a framework for generating testable predictions" (2002:12). This assumes that evolutionary change is largely natural selection-driven (see Joseph 2000:12 for discussion of refutations in evolutionary biology) *and* that cultural evolution can be understood by invoking the biological process of natural selection. However, Winterhalder states that such assumptions are really more tactical than actual claims about social reality. Such "premises" are not designed to test if optimal adaptations occur but how they occur. Ultimately, such assumptions are used for hypothesis-testing convenience. That is, AEBE uses "simple, formal models as heuristic devices for generating testable hypotheses about resource use, reproductive and social behavior, and life history traits" (Winterhalder 2002:4 *Abstract*). Formal models and predictive hypotheses are thus primarily used to teach us about the (assumed) potential workings of adaptation.

This would be an effective safeguard *if* the conclusions derived from such predictive statements (hypotheses) or formal models were always carefully qualified by statements like: "The conclusions generated from simple predictive models or hypotheses are *at best* of heuristic value since they do not test the general proposition that nature optimizes, but rather optimization is built into the theory itself and cannot be tested," or "Formal models in AEBE are better seen as heuristic models of behavior and should not be seen as providing adequate *explanations* of *observed* behaviors. Likewise, hypotheses in AEBE are limited heuristics. That is, even if they are confirmed by empirical case studies, it is problematic to *explain* these empirical observations by invoking the concept of 'optimal adaptations' since these are built-in or *a priori* assumptions used to generate predictive statements."

The use of adaptationism in AEBE has produced "common misunderstanding" [(as Winterhalder calls it (2002:12)], because its use in AEBE *is* confusing. Practitioners claim that they are "explaining" behavior (e.g., Borgerhoff-Mulder 1987, Turke 1988, Low 1990, Cronk 1991b). However, not only is prediction not equivalent to explanation (see discussion of Vayda's argument in Joseph 2000:19) but as an assumed premise adaptationism in AEBE is a problematic conceptual and theoretical framework for generating explanation. Ultimately, the limited explanatory potential of AEBE's conceptual framework and derived conceptual devices should become incorporated into formulations of the theory itself and not simply left to each individual practitioner. The reader then has well-formulated and established guidelines which aid the interpretation of conclusions and empirical findings which rest on derived conceptual devices.

Winterhalder also discusses another "safeguard" against functionalist adaptationist arguments, that is "the requirement that models of ecological circumstances be matched by those of evolutionary mechanism" (Winterhalder 2002:12). This is an example of another safeguard that isn't working. Cronk identifies the primary goal of AEBE as "deciphering the *ultimate causes* of behaviors by examining their reproductive consequences in living populations and by determining their *adaptive significance* for our ancestors" (1991a:29, italics mine). That is, "proximate mechanisms," are treated as "means" to the "end" goal of biological reproductive success. For example, Turke (1988) writes that "individuals should have evolved to be pleased by fulfilling proximate goals that maximize (or at least recently maximized) inclusive fitness" (p. 185) and "social and economic success has always been an important step toward [biological] reproductive success" (Turke 1989:71). Likewise, Cronk (1991b) describes how "culturally defined values and goals are proximate means of enhancing reproductive success" (p. 345). Voland (1998) writes " Through diverse mechanisms, socioeconomic success secures reproductive success" (p. 352).

In order to demonstrate neo-Darwinian adaptationism, AEBE practitioners must merely show *how*, not *if* "proximate" "cultural" factors track "ultimate" "biological success" (see Cronk 1994:183-184; Cronk 1991a, 1991b). This means that if practitioners can demonstrate that "proximate" "cultural" factors *correlate* with "biological" "reproductive success," then voila, we have a confirmed case of adaptationism.[5] Betzig (1988) provides a summary of numerous empirical studies which confirm the correlation between "status" or wealth ("cultural success") and fertility ("biological success") among men (pp. 4-6) and women (pp. 6-7). A discussion of some of the proximate sociocultural mechanisms (such as polygyny, age at first marriage and spouse's age at first marriage) which "promote reproductive success" among men is also provided (pp. 5-6). Thus, what we find in AEBE is functionalist interpretation of reproductive variation as indications of greater or lesser "adaptive efficiency" or "reproductive success." Culture is casually brushed over as a means to an end. The "real" explanation for people's behavior is biological striving for "reproductive success." Furthermore, in addition to theoretical assumptions of how natural selection has "designed" individuals who are best adapted (in terms of inclusive reproductive success) to their environment, there is yet another problematic premise. Within AEBE variation in fertility is taken to mean variation in "reproductive success." That is, there is the implicit or explicit premise that reproduction is *solely* a biological phenomena *and* indicator of "biological success." However, this premise is incorrect because in human societies, reproduction is both a biological *and* cultural phenomena (see Bongaarts and Potter 1983, Campbell and Wood 1988). So discussions about "cultural success" tracking "biological success" are actually nothing more than discussions of how culture tracks culture or how biology and culture interact and mutually effect each other in complex ways (see also the discussion of problems with cultural selectionism in Joseph 2000:22).

So, what happens, in the above examples described by Betzig (1988), if you don't accept as *ipso facto* that "reproductive success" is synonymous with "biological success?" What happens if you don't accept as an assumption, premise or tactic that natural selection has "designed" individuals who are best adapted to their environment? What happens if you don't accept phenotypic tracking made possible by evolved cognitive abilities that allow humans to perceive the relative efficiency of various ways of acquiring resources? (The latter establishes adaptive efficiency by arguing that the human cognitive machinery has been "designed" by natural selection to make optimal decisions). Then you are left with interesting examples of how at particular points in time and space, certain *socio-economic* or *cultural* factors can help *predict* variation in fertility among men or women.

Here is where Winterhalder's inference that I distrust theories which emphasize mechanism (see 2002:17, *Conclusions*) is incorrect. What is problematic is the AEBE practice of stating the mechanism(s) and proceeding to the *ultimate*

[5] It is difficult to imagine containing the domain of AEBE if you also consider that there are different "proxies for fitness" (see Joseph 2000:14). This allows for even further correlations and opportunities to expand the domain, which may even further "surprise its practitioners." However, an attempt to restrict the vast domain of AEBE (which seems to be another exception that proves the rule) can be seen in qualifying discussions in anthropological evolutionary reproductive ecology which emphasize that while "cultural success" and "reproductive success" are correlated in "traditional and historical populations," they are not correlated in modern industrial societies (Voland 1998:352; see also Kaplan et al. 1995; Pérusse 1993; Vining 1986). However, the implicit or explicit claim is that (barring a few details) AEBE has pretty much "explained" variation in reproductive behavior in "traditional and historical" societies. And thus, it seems that the only real challenge which remains is to "explain" reproductive behavior in post-transitional societies (see Vining 1998).

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adaptationist explanation. This has the effect (intended or unintended) of undermining socio-cultural and bio-physical environmental influences on behavior since the goal is less about explaining hierarchical structural constraints or mechanisms within these multiple environments, than demonstrating the assumed workings of adaptation. Likewise, Winterhalder (2002:11) incorrectly believes I am criticizing the work of Jack Broughton, which I do not do (see Joseph 2000:14-15). It is functional adaptationism and equating prediction with explanation that I question: "all that matters [in Boone and Smith's (1998) discussion] is that the optimal model predicts the same broadening of the diet" (Joseph 2000:15). Thus, in as much as the work of Jack Broughton or the work of practitioners from AEBE enhances our understanding of the multiple environmental influences on behavior they are a contribution to ecological anthropology. *However, in as much as theoretical arguments uncritically rely on neo-Darwinian adaptationism to interpret observable phenomena, they are suspect.*

Winterhalder mentions another "safeguard" against naïve adaptationism—methodological individualism. That is, Winterhalder claims that AEBE incorporates a "focus on agents which (who) actually have adaptive agency" (Winterhalder 2002:12). How does viewing human beings as economic optimizers or reproductive maximizers in order to generate predictive statements take the role of individual agency seriously? If your concern is in demonstrating *how* individuals are largely acting out an adaptationist script, then the individual as human agent is little more than a pawn with little or no agency.

Another "safeguard" Winterhalder refers to is insistence on "clearly specified hypothesis testing" (2002:12). However, as already discussed, hypotheses in AEBE are problematic because one is using assumptions to generate hypotheses—the results of which are then interpreted in light of those assumptions. This is what Bettinger refers to as assuming one's conclusions (see Joseph 2000:14). Thus, what we have in AEBE is a situation of more and more testing of hypotheses which will potentially lead to the accumulation of more and more empirical data (and such empirical observations *are* an important contribution to ecological anthropology) but in terms of concepts and frameworks which guide scientific explanation, we are still left with defunct adaptationist functionalist reasoning or "just so stories." Empirical observations or facts don't just interpret themselves (see the definition of explanation in Section I).

Again, Winterhalder does not seem to understand that my criticism centers on the proper domain of AEBE. He seems to suggest that my criticisms are invalid if recent developments in the history of the field, particularly when studies which employ complex techniques for modeling human behavior, are taken into consideration (see Winterhalder 2002:5,7).[6]

First, I agree that such models (e.g., game theoretic or dynamic programming) can have heuristic value, provided that the extent to which they rely on unverified assumptions is clearly stated from the outset. Such models may help to illustrate the need for empirical research to evaluate whether or not certain assumptions of theory are tenable or whether certain relationships in models are verified by empirical research. Such empirical research can then prompt the rejection of overly facile conceptual constructs or point to the need for new and more refined conceptual constructs, etc. However, the major trend in AEBE is that basic concepts and framework of theory are remaining virtually static and unchanged (neo-Darwinian adaptationism as the principle explanatory framework is still justified in terms of assumptions about optimality and adaptation), while new quantitative modeling techniques are increasingly incorporated into the theory. I do not believe Winterhalder would propose that the solution to theoretical conundrums of AEBE is to rely more and more on mathematical models of behavior rather than behavior itself! We cannot rely *too* heavily on computers and complex techniques to elevate theory. Here I agree with Winterhalder's later comments that more compelling data-rich studies are needed (2000:9). I would add the fol-

[6] Some of the examples to which Winterhalder refers post-date my critique.

lowing: There is a further and even more pressing need within AEBE to develop richer basic conceptual devices and theoretical framework(s). The assumptions of theory must themselves be subjected to serious scientific study and evaluation.

Conclusions

Are the flaws of AEBE, which largely stem from failure to specify a domain or what might be termed "broad-stroke" adaptationism, best seen as nails in the coffin (see Winterhalder 2002:12,18) of the theory? No, but they do reinforce the old saying: "if your only tool is a hammer, then everything looks like a nail." AEBE failures will become "coffin nails" if and only if past failures continue to inform future theory-building. Many practitioners implicitly if not explicitly recognize the problems with neo-Darwinian adaptationism. However, the problematic ways in which conceptual constructs in AEBE inform derived conceptual devices, the interpretation of empirical observations and the domain of theory have not been carefully examined and worked out, and until they are theoretical refinement remains a distant prospect. I do not suggest, either in my critique or here, that AEBE be dismissed. AEBE has made important contributions to ecological anthropology, particularly in empirical studies which have provided valuable observations of human behavior and techniques for studying human behavior. Most notably, AEBE has identified some unique life history traits of our species in what may be considered confirmed generalizations of human reproductive ecology (see Joseph 2000:18). However, the basic conceptual devices and adaptationist framework found in AEBE are problematic and have resulted in serious disagreements over its proper domain in anthropology. While it may seem that functionalism and adaptationism have plagued ecological anthropology from its inception, this does not mean that we cannot or have not established better safeguards against hyperfunctionalism or adaptationism. For AEBE, perhaps the most sophisticated safeguard against crude adaptationism is provided by Vayda (1995) who suggests trying to establish historical causal chains[7] (for a discussion see Joseph 2000:19-20). However, in addition to better safeguards, we need to pay more careful attention to the premises on which Darwin's biological theory of adaptation by natural selection is based. Many of these criteria are inapplicable to the evolutionary study of human *sociocultural* systems. First, in human ecosystems, reproductive or fertility behavior is shaped by both biology *and* culture. Likewise, the strict organism/ environment dichotomy[8] is problematic in human

[7] The role of history is something AEBE has traditionally undermined. AEBE's definition of history is functional and ahistorical (see Joseph 2000:21-24). Winterhalder simply dismisses my questions of AEBE's conceptualization of history and evolution by invoking Alfred Wallace's fallacy of culture without-evolution (and vice versa) and suggests that "dual inheritance" and other discussions within AEBE have already addressed the question. Within AEBE it seems that history is largely a subset of a particular type of biological evolutionary change; i.e., natural selection-driven change. Likewise, dual inheritance theory incorporates a narrow understanding and definition of history. History only refers to (1) "change that does not repeat itself "(Boyd and Richerson 1992:185) or "the occurrence of long-term change" (ibid, 202); and (2) a specific kind of change in which "similar initial conditions give rise to qualitatively different trajectories" (ibid, 186)– that is, "the tendency of initially similar systems to diverge"(ibid, 202). Practitioners from dual inheritance theory argue that we can accept as a premise that analogous cultural processes (to the biological process of natural selection) occur in human societies and such processes can adequately explain important convergences in cultural evolution (e.g., the emergence of state-level societies in human history; see Boyd and Richerson 202-203). Again, we have to accept the premise and ignore the problem of cultural selectionism and the structural properties of human socio-cultural systems. We also have to ignore the fact that unlike biological evolution, in the evolution of sociocultural systems, ontogeny and phylogeny are identical (see Hallpike 1988:33, 79). Ultimately, we must agree to the following: "If most of the historic context is taken as given, Darwinian arguments can be very powerful heuristics" (Boyd and Richerson 1992:203). Not surprisingly, history seems to have made a sudden exit. In order to further theoretical discussions surrounding these questions and achieve "progress through criticism" wouldn't it be more productive to actually engage in discussion over the definition and role of history in AEBE (or in evolutionary ecology broadly defined) rather than casually brush-off such debates?

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sociocultural systems (Joseph 2000:22). In human ecosystems, we do not only find individual actors and their aggregations, but social institutions or "organized systems of meaning bounded together largely by information flows (Hallpike 1988:27)" (Joseph 2000:22). This means that in addition to refining AEBE, there is a need to incorporate and develop additional theoretical structure in the study of human evolution and ecology. Ultimately, such an undertaking means that we must go beyond critique. Section III provides some suggestions for future theory-building. However, before proceeding to the final section, it is important to briefly consider the role of critique in theory-building, particularly since "critique as practice" figures prominently in Winterhalder's response.

Section II: Reply To Winterhalder's Comments on Critique as Practice

Defining Critique: It's Role in Theory-building

Perhaps a great of deal of pain, frustration, and misunderstanding surrounding critique comes from failing to appreciate its uses *and* its limitations. The New Shorter Oxford English Dictionary (1993) defines "critique" (the verb) as follows: "discuss critically; write a critique of; make a critical assessment of" and "critical" as "given to judging, esp. unfavourably; fault-finding; censorious." Thus, to critique is to pass judgement upon something with respect to its faults or merits, but it often connotes *unfavourable* judgement. When reading Winterhalder's suggestions for effective progress through critique, it seems that the onus of responsibility for achieving such progress is largely on the critic. The critic must confess to her pet-theories, offer alternatives to the theory under scrutiny, engage in a comparative critical evaluation of different theories but still engage the theory on its own terms, provide a detailed overview of the history of the theory being evaluated and then offer suggestions for reconstructing theory. Are these laudable ground rules for critique writing? (I mean, after all, if you want a history of the field, wouldn't it be wiser to go read a history of the field?) Winterhalder's suggestions, however, fail to consider the possible ramifications of *expanding the definition of critique*. Specifically, isn't there a danger that if we put unprecedented demands on the writing of critique, that the net effect will be to discourage criticism? Practitioners of whatever theory under scrutiny can then deflect criticism by drawing attention away from problems with theory and toward deficiencies in the practice of critique writing or critic for that matter. It is vital that we recognize the *limitations of critique*—that it is a *crucial part*, but only a *part* of theory refinement and rebuilding. Perhaps the most valuable contribution of critique toward the goal of theory-building, lies in the *dialogue* generated through criticism/response. Good discussion can help anthropologists evaluate strong/weak criticisms and identify where there is a need for further discussion. Effective dialogue requires a serious commitment and mutual responsibility on the part of *both* critic and criticized. There is also an implicit assumption on the part of the critiquer and criticized that the reader possesses some familiarity with the theory being evaluated. The essential point is that without serious dialogue, theory-building via progress through criticism is unlikely to occur. This reply is a further attempt to clarify and refine critical arguments as part of an ongoing dialogue and process of theory-building in ecological anthropology.

[8] The organism/environment dichotomy requires making a clear distinction between a) what is doing the selecting and b) what is being selected for (Hallpike 1988:57). If that which is doing the selecting is also that which is being selected for, then you have forsaken the Darwinian process of natural selection and have invoked cultural selectionism instead (for a discussion see Joseph 2000:22). Cultural selectionism is an example of misapplication of Darwinian theory to the study of human socio-cultural systems. It has led to important disagreements over how best to account for *some* of the successful *predictions* of optimal behavior at specific temporal and spatial scales in human social evolution. Specifically, AEBE has failed to consider how examples of "optimal" efficiency or "rational" behavior in human ecosystems are better explained by alternative theories (e.g., structural Marxism) which recognize the influence of historically-specific socio-cultural processes of capitalism (including subsidiary processes like mechanization) rather than the biological process of natural selection (see Joseph 2000:15).

Second, I disagree with Winterhalder's caveat of avoiding "criticism by proxy" in that we must pay attention to earlier criticisms so that we don't just keep reinventing the wheel. However, I do believe that we should build on previous good criticisms and try to elevate the level of theoretical discussion. By utilizing the method for theory developed by PK&J, critics can locate their criticisms in terms of specific components of theory, rather than simply providing an endless laundry list of flaws. This helps put similar criticisms (of the same components of theory) together and allows more precise identification of the weaknesses and strengths of a given theory. I believe that the PK&J method for theory is an invaluable tool for achieving progress through criticism and the broader goal of bringing about theoretical integration in ecological anthropology.

The last point I would like to add is that if critique is to be effective, it should never be personal. Criticism should be aimed at deconstructing theory not individuals. I took great pains not to make my critique personal. This is why I framed my criticisms as criticisms of "AEE" or "AEBE" and refer to examples as examples from "practitioners." Individual practitioners cannot and should not be reduced to theoretical straw men or women which critics then proceed to take apart. Sometimes in anthropology, criticisms specifically target individuals as if the main point of the entire endeavor is to bring down an individual or at least cast doubt on his/her competency, as opposed to evaluate theory. I mention this because Winterhalder states that I never point out in the criticisms I raise whether or not the person who made the criticism is sympathetic or unsympathetic to the theory. I do not believe that it is relevant to characterize the sympathies of every individual concerned. The merits of a criticism should not be based on whether or not the criticism was raised by a practitioner, nonpractitioner, sympathizer or unsympathizer. Likewise, flaws in theory are not attributable to the flaws of individuals, but to the structural components of theory. With that said, it is a barrier to progress to simply ignore or dismiss critical nonpractitioners who are labeled "unsympathetic" (although, I am still not sure I understand what this means). If we take our role as ecological anthropologists seriously, we should be willing to engage in dialogue over general or specific theoretical disagreements/agreements raised in critique writing/response. The question remains, what fundamental theoretical questions should guide anthropologists interested in the study of human evolution and ecology?

Section III: Toward a More Inclusive Human Evolutionary Ecology

I have argued that "unleashing the power of Darwinism" cannot simply be justified by a series of unverified assumptions or by premises, which, upon close examination, turn out to be of highly questionable applicability to human sociocultural systems. Thus, parallel to the need to refine and limit the domain of AEBE, is a need for a new more inclusive human evolutionary ecology, one that goes beyond adaptationism, and one that not only focuses on the ways in which humans are just like other species, but also the ways in which humans are another unique species.

Figures 2, 3 and 4 in Joseph (2000) were attempts to *graphically* represent what a more inclusive human evolutionary ecology would look like. But perhaps more importantly these figures attempt to identify contributions to and initial suggestions for the study of human evolution and ecology. They suggest a need for theoretical frameworks which incorporate hierarchy, multiple causality and multiple environments. Also needed are conceptual constructs which account for the existence of empirical phenomena (behaviors and structures) in socio-cultural evolution which are suboptimal (in terms of their functional efficiency; e.g., Hallpike's concept of "Survival of the Mediocre" [1988:81-145]). These are only initial suggestions, and ultimately hope to reaffirm the central role of ecological anthropology in addressing a fundamental question at the very heart of anthropology: how do human ecosystems differ from nonhuman ecosystems? Attempts to highlight the unique properties of human ecosystems and unique processes in human socio-cultural evolution are apparent in both anthropology, sociology and history (for an example in history see Mandelbaum 1971; for ex-

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amples in sociology see Giddens 1981, 1984; Zeitlin 1973; Sanderson 1990,1999; for examples in anthropology see Flannery 1972; Rappaport 1984; Hallpike 1988; Tainter 1988; Stepp and Kuchka 2001). Addressing this larger fundamental question of ecological anthropology will require greater attention to the components of theory, particularly the domain of different theories or subtheories and far greater interdisciplinary *and* intradisciplinary (i.e., between biological and cultural anthropologists) collaboration.

References

ALLEN, T. F. H., AND T. W. HOEKSTRA.
1992 *Towards a unified ecology*. New York: Columbia University Press.

BETZIG, L.
1988 "Mating and parenting in Darwinian perspective," in *Human reproductive behavior*. Edited by L. L. Betzig, M. Borgerhoff-Mulder and P. W. Turke, pp. 3-20. Cambridge: Cambridge University Press.

BONGAARTS, J., AND R. G. POTTER.
1983 *Fertility, biology and behavior: An analysis of proximate determinants*. New York: Academic.

BORGERHOFF-MULDER, M.
1987 On cultural and reproductive success: Kipsigis evidence. *American Anthropologist* 89: 617-634.

BOYD, R., AND P. J. RICHERSON.
1985 *Culture and 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.

CAMPBELL, K. L., AND J. W. WOOD.
1988 "Fertility in traditional societies," in *Natural human fertility: Social and biological determinants*. Edited by P. Diggory, M. Potts and S. Teper, pp. 39-69. London: Macmillan.

"critical [4]" *The new shorter Oxford English dictionary on historical principles*. Volume 1. Edited by L. Brown. Oxford: Clarendon Press, 1993.

"critique" *The new shorter Oxford English dictionary on historical principles*. Volume 1. Edited by L. Brown. Oxford: Clarendon Press, 1993.

CRONK, L.
1991a Human behavioral ecology. *Annual Review of Anthropology* 20:25-53.
1991b Wealth, status, and reproductive success among the Mukogodo of Kenya. *American Anthropologist* 93:345-360.
1994 Is there a role for culture in human behavioral ecology? *Ethology and Sociobiology* 16:181-205.

DARWIN, C.
1859(1909) *The origin of species*. The Harvard Classics Series. Edited by C.W. Eliot. New York: P.F. Collier.

HALLPIKE, C. R.
1988 *The principles of social evolution*. Oxford: Clarendon Press.

FLANNERY, K.
1972 The cultural evolution of civilizations. *Annual Review of Ecology and Systematics* 3:399-426.

FRICKE, T. E.
1990 Darwinian transitions? A comment. *Population and Development Review* 16(1):107-119.

GIDDENS, A.
1981 *A contemporary critique of historical materialism*. Berkeley: University of California Press.

GIDDENS, A.
1984 *The constitution of society*. Berkeley: University of California Press.

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

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

KAPLAN, H., J. LANCASTER, J. BOCK, AND S. JOHNSON.
1995 "Fertility and fitness among Albuquerque men: A competitive labor market theory," in *Human reproductive decisions: Biological and social perspectives*. Edited by R. I. M. Dunbar, pp. 96-136. London: Macmillan.

KUHN, T. S.
1977 "Objectivity, value judgement, 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.

LLOYD, E. A.
1988 *The structure and confirmation of evolutionary theory*. New York: Greenwood Press
2001 "Why do people find evolutionary psychology so compelling?" Paper presented at the meeting of the International Society for the History, Philosophy, and Social Studies of Biology. Quinnipiac University, Hamden, CT. 2001.

LOW, B.
1990 Occupational status, landownership, and reproductive behavior in 19th-century Sweden: Tuna Parish. *American Anthropologist* 92: 457-468.

MANDELBAUM, M.
1971 *History, man, and reason: A study in nineteenth-century thought*. Baltimore: Johns Hopkins University Press.

MAYR, E.
1993 *One long argument: Charles Darwin and the genesis of modern evolutionary thought*. London: Penguin Books.

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

PÉRUSSE, D.
1993 Cultural and reproductive success in industrial societies: Testing the relationship at the proximate and ultimate levels. *Behavioral and Brain Sciences* 16:267-322.

PICKETTT, S. T. A., J. KOLASA, AND C. G. JONES.
1994 Ecological understanding: The nature of theory and the theory of nature. New York: Academic Press.

RAPPAPORT, R. A.
1984 *Pigs for the ancestors: Ritual in the ecology of a New Guinea People* (New, enlarged edition). New Haven: Yale University Press.

SALMON, W. C.
1984 *Scientific explanation and the causal structure of the world*. Princeton: Princeton University Press.

SANDERSON, S. K.
1990 *Social evolutionism: A critical history*. Oxford: Basil Blackwell.
1999 *Social transformations: A general theory of historical development* (Expanded edition). Lanham, MD: Rowman & Littlefield Publishers, inc.

STEPP, J. R., AND H. E. KUCHKA.
2001 "Unique and remarkable properties of human ecosystems," Paper presented at the Annual Meeting of the American Anthropological Association. Washington, DC. 2001.

TAINTER, J.A.
1988 *The collapse of complex societies*. New York: Cambridge University Press.

TURKE, P. W.
1988 "Helpers at the nest: Childcare networks on Ifaluk," in *Human reproductive behavior*. Edited by L. L. Betzig, M. Borgerhoff-Mulder, and P. W. Turke, pp. 173-188. Cambridge: Cambridge University Press.

https://scholarcommons.usf.edu/jea/vol6/iss1/2 | DOI: http://dx.doi.org/10.5038/2162-4593.6.1.2

1989 Evolution and the demand for children. *Population and Development Review* 15:61-90.

TROUT, J. D.
1991 "Reductionism and the unity of science," in *The philosophy of science*. Edited by R. Boyd, P. Gasper, and J. D. Trout, pp. 387-392. Cambridge, MA: MIT Press.

VAYDA, A.
1995 Failures of explanation in Darwinian ecological anthropology: Part I. *Philosophy of the Social Sciences* 25(2): 219-249.

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

VINING, JR., D. R.
1986 Social versus reproductive success: The central theoretical problem of sociobiology. *Behavioral and Brain Sciences* 9:167-216.

WILSON, M., AND DALY, M.
1997 Life expectancy, economic inequality, homicide, and reproductive timing in Chicago neighbourhoods. *British Medical Journal* 314(7089): 1271-1274.

WINTERHALDER, B.
1986 Diet choice, risk, and food sharing in a stochastic environment. *Journal of Anthropological Archaeology* 5:369-392.
2002 Behavioral and other human ecologies: Critique, response and progress through criticism. *Journal of Ecological Anthropology* 6:4-23.

WOOD, J. W.
1990 Fertility in anthropological populations. *Annual Review of Anthropology* 19:211-242.

ZEITLIN, I. M.
1973 *Rethinking sociology: A critique of contemporary theory*. Englewood Cliffs, NJ: Prentice-Hall.