

ARTICLES

Anthropological Evolutionary Ecology: A Critique

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Abstract

The goal of this paper is to critically evaluate Anthropological Evolutionary Ecology (AEE) as a paradigm by utilizing the method for theory framework developed by Pickett et al. (1994). While AEE can contribute in some ways to our understanding of human behavior through methods and techniques derived from neo-Darwinian theory (as well as current approaches in animal behavior and decision theory), AEE as a paradigm remains theoretically ill-equipped for the study of human ecology. This critique will focus on Anthropological Evolutionary Ecology, however, references will be made to Biological Evolutionary Ecology (BEE) since AEE relies heavily on theoretical components derived from BEE.

Introduction

A critique of Anthropological Evolutionary Ecology (AEE) as a theoretical paradigm should begin with a definition of paradigm. Following Kuhn (1970), the definition reads: a paradigm is the world view, belief systems, series of assumptions, methods, techniques and exemplars for problem solution held in common by a scientific community. The critique here is meant to apply to AEE as a theoretical paradigm in general, often referred to as evolutionary behavioral ecology and to its submodels and subtheories, in particular optimal foraging theory and life history theory.

The critique is divided into seven sections, closely paralleling the structure of Figure 1. This scheme depicts the general components of theory and their degrees of development. Basically, as theory develops, it changes in two major ways: 1) through the addition of theoretical components (see rows in Figure 1); and 2) through the refinement of components (see columns in Figure 1). This scheme shows both increase in the number and refinement of components as theory matures. The column headings from left to right represent increasing development of theory, whereas the rows from top to bottom indicate

increasing completeness of theory. Box 1 describes the major components of theory that are referred to in the rows of Figure 1. Box 2 describes the stages of maturation depicted in the columns of Figure 1. During the early stages of theory development, the emphasis is on the addition of components. By the consolidating stage of theory development all of the components are in place. Subsequently, refinement of components is emphasized.

Thus, as theory develops, it becomes more and more complete, by the addition and refinement of theoretical notions, constructs, derived constructs, and structure. Increasing richness of components is a hallmark of maturing theory. For AEE, notions, assumptions, facts, and hypotheses developed early, with assumptions being the first to be fully developed. But notions are not yet fully explicit, and confirmed generalizations, models, translation modes, domain and the framework are still in the process of being refined.

The stages of maturation depicted in the columns of Fig. 1 can be thought of as an idealized developmental sequence. Theory change is actually often chaotic, reflecting a combination

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Component		DEGREE OF DEVELOPMENT				
		Pre-theoretic	Intuitive	Consolidating	Empirical-Interactive	Confirmed or Rejected
Basic Conceptual Devices	Notions	////	////	////	////	//////////
	Assumptions	////	////	////////	////////	//////////
	Definitions		////	////	////////	//////////
	Concepts		////	////	////////	//////////
Empirical Content	Facts	////	////	////	////////	//////////
	Confirmed Generalizations			////	////	//////////
Derived Conceptual Devices	Hypotheses	////	////	////	////////	//////////
	Models		////	////	////////	//////////
	Theorems			////	////	//////////
Framework and Structure	Framework			////	////	//////////
	Domain	////	////	////	////////	//////////
	Translation Modes			////	////	//////////

FIGURE 1. COMPLETENESS AND DEVELOPMENT OF THEORY.

See Box 1 for a description of the components. Degree of hatching denotes increased refinement and precision in theory development (modified from Figure 4.1 of Pickett et al. 1994).

BOX 1. COMPONENTS OF THEORY (modified from Pickett et al. 1994, Box 3.2).

Basic Conceptual Devices

- assumptions—conditions or axioms needed to build theory
- definitions—conventions and prescriptions necessary for the theory to work with clarity
- concepts—abstract ideas generalized from regularities in phenomena, or conceived through reflection and imagination

Empirical Content

- facts—confirmable records of phenomena, checked and re-checked
- confirmed generalizations—condensations and abstractions from a body of facts that have been checked and re-checked

Derived Conceptual Devices

- hypotheses—testable statements derived from or representing various components of theory
- models—conceptual constructs that represent or simplify the world or subject matter of concern
- theorems—ideas or propositions deduced or proposed as demonstrable deductions

Framework and Structure

- framework—nested causal or logical structure of a theory
- domain—the scope in space, time and phenomena addressed by a theory
- translation modes—procedures and concepts needed to move from the abstractions of a theory to the specifics of application or test

BOX 2. STAGES TO THEORY MATURITY (based on Pickett et al. 1994, Box 4.1 and Figure 4.1).

Pre-theoretic—represented by rudimentary development of a few components

Intuitive—simple and fundamental components present, including concepts, definitions and models

Consolidating—derived conceptual devices begin to mature

Empirical-Interactive—with concepts, definitions, and domain increasingly clarified, hypotheses are more amenable to evaluation

Confirmed or Rejected—judgement by the community of the adequacy of evaluation and strength of those outcomes for a mature theory; confirmed theories often permit practical application

of different empirical pursuits and different subtheories, and in the case of AEE, more complex or highly derived components have not yet accompanied simpler ones. 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. Nonetheless, the key idea is that the jobs a theory is able to do depend upon its stage of maturity (Box 2); that is, the richness of its roster of theoretical components and their refinement.

As a theory begins to take shape and to be used it often becomes clear that existing components must be replaced or refined. Theory may emerge from pre-theoretic notions by adding components, without showing much refinement. At the consolidating stage basic conceptual components are refined, empirical content is refined and expanded, derived conceptual components are added and refined, and the theoretical framework and structure begin to take shape.

Mature theories have well-defined basic conceptual constructs and derived conceptual devices, as well as well-delineated domains, with internal structure and empirical content, allowing the development of hypotheses that can be confirmed or rejected. Confirmed mature theory has developed through prior phases of pre-theory, intuition, consolidation and empirical-interaction (see Figure 1). Such theories come to represent the particular historical time periods within which they develop.

A major criticism of AEE as a theoretical framework that I explore at some length in this

paper is its failure to delineate a domain. I first came across AEE during the course of my graduate training in ecological anthropology. I found particularly attractive a field that seemed to hold the promise (both implicitly because of its name and explicitly in its purported goal) of adding methodological and theoretical rigor to ecological anthropology. Here, I thought, I will find the best of what anthropology has to offer the study of human evolution and ecology. However, it soon became clear that AEE was not what it seemed (i.e., a broad framework which includes an amalgam of sophisticated approaches to understanding human variation and change). While the purpose of this paper is not to develop an alternative framework for the study of human ecology and evolution, Figure 2 provides a brief glimpse into a more inclusive human evolutionary ecology; what I naïvely imagined the domain of anthropological evolutionary ecology to look like. Potential contributions to the study of human ecology and evolution are listed according to social hierarchies of major human ecological paradigms: population and ecosystems (for a discussion of the characteristics of “population” and “ecosystem” paradigms in biological ecology and their integration see Pickett et al. 1994: 3-25;151-165).

Thus, I will begin the second section with a critical discussion of the domain of AEE since it is problematic throughout the theoretical development of AEE. The domain is formulated early in theory development, but is only fully articulated at later stages of theory maturation. Discussion of the domain will help provide a gen-

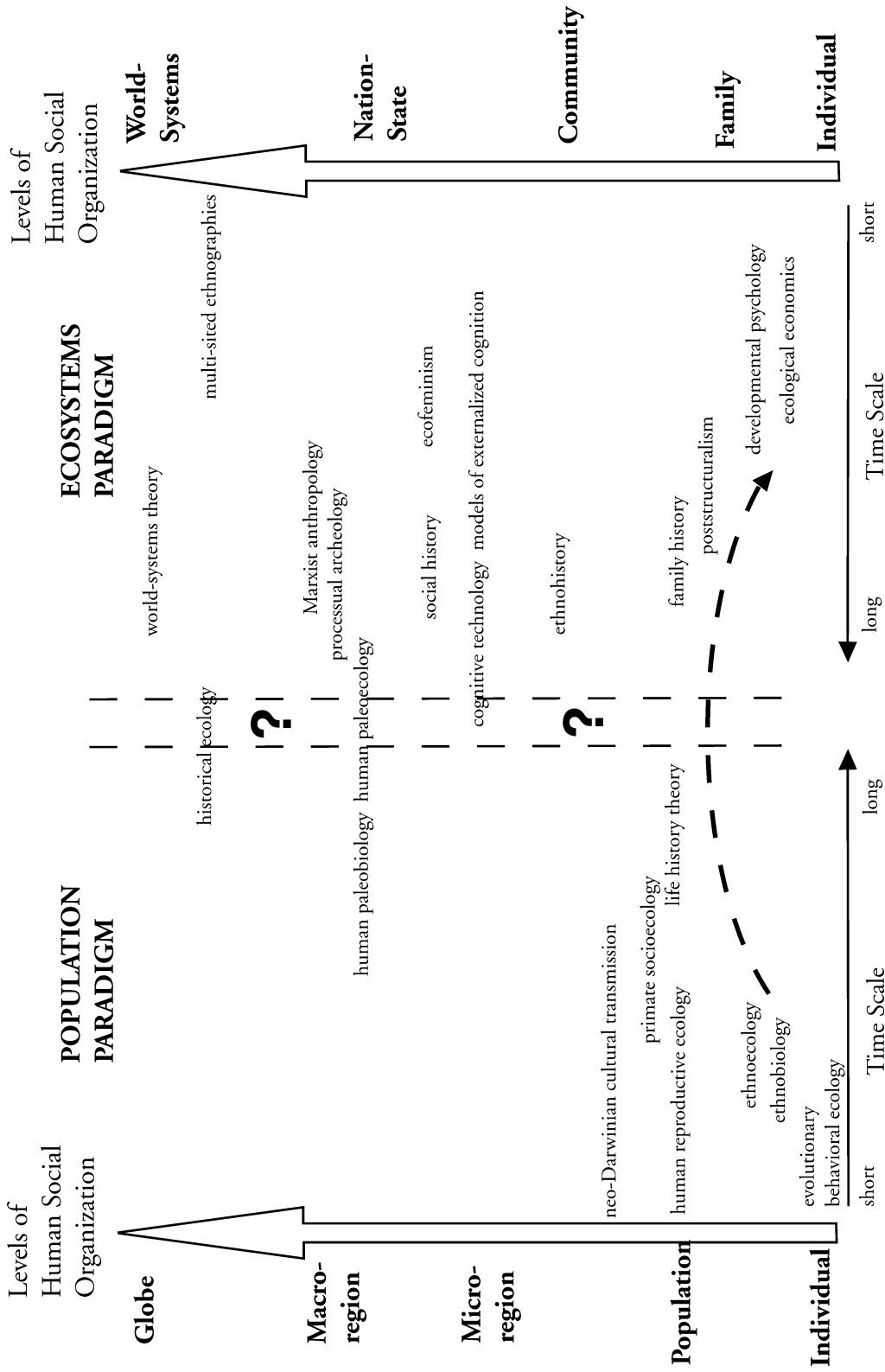


FIGURE 2. CONTRIBUTIONS FROM VARIOUS DISCIPLINARY AND SUBDISCIPLINARY THEORIES, SUBTHEORIES, AND MODELS TO THE ANTHROPOLOGICAL STUDY OF HUMAN EVOLUTION AND ECOLOGY.

eral understanding of the spatial and temporal scale and hierarchical level of organization to which AEE applies. The third section will critique the basic conceptual content of AEE, describing the basic conceptual devices found in AEE. The fourth section will evaluate the empirical content of AEE by examining the facts or confirmed generalizations AEE has contributed to our understanding of human ecology and evolution. The fifth section will critically discuss the derived conceptual devices employed by AEE such as hypotheses, theorems and models to determine their utility in understanding human behavior. The sixth section will discuss inadequacies in theory structure and framework as they exist today, and the conclusion will summarize major points of the critique.

Domain of Anthropological Evolutionary Ecology via Its Parent Discipline Biological Evolutionary Ecology

The domain “delimits the scope in space, time and level of organization of a class of phenomena assumed (by theory) to share certain properties and be of a distinct and general type” (Hirschfield and Gelman 1994:21). The domains of an inquiry should be explicit and specific, as much as possible, though they may be expanded or restricted as theory develops. Domains typically become more restricted as theory develops because refinement shows that the theory is not as grandly applicable as originally presumed.

AEE assumes that humans can be understood using the same assumptions and techniques that are used to analyze other animals (Hill 1993). Specifically, the AEE paradigm assumes that its neo-Darwinian approach can be used fruitfully to understand dungflies, reef fish, monkeys, swallows and humans (Betzig 1997). AEE sets out to understand how phenotypic traits of organisms in general can be understood as adaptations in ecological context. That is, AEE is concerned with “unleashing the power of Darwinism” to understand the design

features of organisms (whether human or non-human) and as such does not in principle address or concern itself with how an ecological understanding of human behavior is to differ from that of other complex organisms (Smith 1992a, 1992b). 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.

In fact, it is sometimes stated that the ultimate goal of AEE is to provide a single general model for understanding variation in foraging behavior in time and space (Hill 1988). And as Vayda (1995a) points out, even AEE models that fail to confirm expectations derived from neo-Darwinian theory are still argued to have heuristic value, which only obfuscates the attempt to establish and refine the domain of inquiry. Since the foundational core of AEE is its neo-Darwinian reliance on natural selection for explanation, a well-established theory in the biological sciences, it might be assumed that the domain is virtually boundless. However, as Mayr (1993) reminds us, Darwin’s theory was derived deductively, and depends upon the presence of certain conditions. So I will discuss how the domain of biological evolutionary ecology (BEE), and, in particular, the domain of the Darwinian theory of natural selection, is becoming more delimited within evolutionary biology.

Mayr (1993) points out that Darwin (1902) presented his theory of natural selection deductively, which was subsequently formalized by Huxley (1942) and Mayr (1993). The theory rests upon certain necessary conditions. If these conditions do not hold, then natural selection need not occur. Table 1 summarizes the conditions necessary for the occurrence of evolution by natural selection.

If Condition 1: Potential exponential increase of populations (superfecundity)	PLUS Condition 2: Observed steady state stability of populations	PLUS Condition 3: Limitation of resources	THEREFORE Inference 1: Struggle for existence among individuals
If Inference 1: Struggle for existence among individuals	PLUS Condition 4: Individuals vary	PLUS Condition 5: Heritability of much of the individual variation	THEREFORE Inference 2: Differential survival, i.e., natural selection
OR... If Condition 1: Potential exponential increase of populations (superfecundity)	PLUS Condition 2: Observed steady state stability of populations	PLUS Condition 6: Geographical isolation of populations	PLUS Condition 4: Individuals vary
PLUS Condition 5: Heritability of much of the individual variation	THEREFORE Inference 2: Differential survival, i.e., natural selection	AND THEREFORE Inference 3: Speciation	

TABLE 1. A COMBINED ADAPTATION OF MAYR'S (1993, FIGURE 1) AND BENNETT'S (1997, TABLE 8.1) FORMALIZATION OF DARWIN'S DEDUCTIVE THEORY OF EVOLUTION THROUGH NATURAL SELECTION.

The view of evolution espoused by most ecologists today is basically that of Darwin (1902) integrated by modern genetics. At the core of the modern synthesis is the acceptance of two conclusions:

Gradual evolution can be explained in terms of small genetic changes ("mutations") and recombination, and the ordering of this genetic variation by natural selection; and the observed evolutionary phenomena, particularly macroevolutionary processes and speciation, can be explained in a manner that is consistent with the known genetic mechanisms. (Mayr 1980: 1)

However, the modern synthesis acceptance of the Darwinian natural selection model of generation-by-generation change extrapolated back through geological time (Eldredge 1995)

is beginning to be refined. At the core of the debate is the question of the proper domain of Darwinian theory. For example, the debate between modern synthesis defenders and punctuated equilibria defenders is whether the fact of evolution at a broad scale can be explained in terms of present microevolutionary processes (substantive uniformitarianism). While most comments on the debate have focused on whether evolution has taken place by continuous gradual change over long periods of time or rapidly over shorter periods of time, many have pointed out that this is not the central issue (Bennett 1997).

Dawkins (1986) and Levinton (1988) have correctly pointed out that the rates of evolution may vary over time and can be sufficiently rapid as to appear instantaneous in the fossil record.

However, the central criticism of the modern synthesis does not center on rates of evolution, but on uniformitarianism (Bennett 1997: 34). As Bennett (1997: 34) points out, the architects of the punctuated equilibria model are questioning the modern synthesis position that knowledge of the present is adequate to explain the record of the past. The only way of resolving this dispute is to examine the fossil record.

One of the fundamental questions to be determined from the fossil record is: what happens to species during long periods of undoubted environmental change? Under uniformitarianism or phyletic gradualism, the expectation is that there would be evolutionary change, but under punctuated equilibria there is not necessarily an expectation of change (Bennett 1997: 43). Recent geological evidence (for a more thorough review see Bennett 1997:92-198) indicates that climates have been changing globally with frequencies on Milankovitch time-scales (ca. 20,000-100,000 yr) throughout Earth history. Yet, it is only during the most recent part of the geological record that sediments have been sampled finely enough, and there is a time-scale precise enough, to make useful statements about the response of organisms to global climatic change. The portion of the geological record most accessible is the Quaternary.

The overall evidence shows that for most fossil groups through most of the Quaternary, there is remarkably little evidence of evolutionary change. Species during the course of their history may experience three types of evolutionary patterns: stasis, gradual change, and quick change plus or minus speciation by lineage splitting. All these patterns of evolution occur in the Quaternary (Bennett 1997: 175). Nevertheless, stasis is the most frequent response to Quaternary climatic change. Through climatic oscillations on Milankovitch time-scales, the morphological stability of species is impressive (Gould 1992). In ecological time, on a microevolutionary scale, natural selection has been shown to have occurred (for example among populations of Darwin's finches) in response to environmental change, but there is no evidence that it accumulates over

longer periods of time to bring about speciation in the Darwinian sense (although this may still be a plausible hypothesis for a subset of evolutionary change)(see Steadman 1986).

Thus, many paleontologists in evolutionary biology have begun to closely consider the domain of Darwinian theory. Eldredge (1999) recognizes that BEE maintains at its core the Fisherian stance that natural selection is both necessary and sufficient to explain the evolution of life. As a theory, Eldredge (1999) indicates that it works best in the domain it was originally devised to explain, generation by generation adaptive change in gene frequencies. Similarly, he tries to bound the domain of a related sister discipline Sociobiology, which explains patterns of cooperation in social organisms in terms of degree of shared genes among the participants. Eldredge and Grene (1992) point out that this theoretical system works very well in the case of social insects; however, it does not work as well in other social organisms, such as birds and mammals. Theoretical refinements in attempts to go beyond the evolutionary ecology brand of evolutionary biology are evident. The example of Milankovitch cycles and physical processes in evolutionary change is one attempt. Another attempt at theoretical refinement can be seen in the theory of co-evolution, in which two species adaptive systems, such as butterfly/milkweed, are analyzed. Yet another is Leigh Van Valen's notion of the Red Queen, where the evolution of one species is considered as a reaction to changes from other species impacting it.

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 socio-cultural behaviors, structures, interactions, and flows across time and space. The failure to delimit the domain of AEE creates confusion and debate as to its applicability. In order to determine what AEE can contribute to human ecology, a more thorough understanding of its conceptual and empirical content is needed. The basic conceptual devices of AEE will be described next.

Basic Conceptual Devices: Assumptions, Definitions and Concepts in Anthropological Evolutionary Ecology

AEE is the first attempt to develop a theory of socio-cultural behavior in terms of the same principles that guide biological theories of evolution. AEE can be defined as the application of natural selection theory at both the macro and micro evolutionary scales to understanding adaptation and biological design in ecological context (Winterhalder and Smith 1992). AEE is a broad theoretical umbrella, which unites diverse subtheories and models. Typically, AEE studies the consequences of the assumed operation of natural selection on an organism's phenotype (behavior in particular). AEE comprises a wide range of phenomena, including study of foraging strategies, spatial organization, group size and formation, sex allocation, mate choice, life history patterns, etc. (Winterhalder and Smith 1992: 8). Oftentimes AEE is referred to as evolutionary behavioral ecology. However, if the behavior being studied involves social interactions (and physiology is de-emphasized) then the term socioecology can be used (Crook 1970).

Life history theory (LHT) is a subset of human behavioral ecology but focuses on particular aspects of human behavior and physiology. Specifically, life history theory analyzes how individual variation in life history traits leads to variation in reproductive fitness among individuals (Stearns 1992). Life history theory makes the simplifying assumption that the phenotype consists of demographic traits—size, number and sex ratio of offspring at birth, size at maturity, age at maturity, age and sex-specific reproductive investment, age and sex-specific mortality schedules, growth pattern, and life span—bound together by constraining relationships and tradeoffs (Charnov 1993, Roff 1992, Stearns 1992).

However, four general features serve to unite all of the loosely tied and/or diverse areas of investigation included under the theoretical umbrella of AEE. (1) Deductive modeling—simple mathematical models (i.e., optimization or game theo-

retic models) deduced from general principles are considered a useful means of generating powerful explanations (Smith and Winterhalder 1992a; Smith 1987, 1991; Borgerhoff-Mulder 1991), although AEE does not require that behavior be genetically determined or linked (Smith and Winterhalder 1992a). (2) A neo-Darwinian commitment to methodological individualism or the self-interested individual as the unit of analysis (Smith and Winterhalder 1992a, Smith 1992a, Borgerhoff-Mulder 1991). Methodological individualism is a position which holds that social and ecological aspects of groups or populations can be best understood as the result of actions/goals of aggregates of individuals who constitute these larger groupings. (3) Selectionism, which assumes the presence of variability that affects selective fitness of reproductive “units” and determines the presence of reproductive mechanisms by which this variability is carried from “unit” to “unit” (Smith and Winterhalder 1992a). It holds that the properties of individuals can fruitfully be analyzed using the theory of natural selection, that selection acts on all aspects of phenotype that exhibits variability and heritability (genetic or cultural) and has effects on survival and reproduction, and that selection favors phenotypic traits that exhibit high fitness or evolutionary stability (competitive superiority when fitness is frequency dependent). (4) Most aspects of behavior (whether genetically or culturally transmitted) are highly plastic and this plasticity can be modeled using “decision rules” or conditional strategies (i.e., “if the environment or payoff matrix looks like x , then do y ”). It is these strategies that are subject to selection for maximum reproductive success or evolutionary stability (Smith 1991).

Vayda (1995a) states that if one axiom were to be highlighted as central to AEE it would have to be (in the words of its practitioners “Darwin's central tenet”) that differential reproduction is the primary force shaping biological adaptation and diversity” (see Smith and Winterhalder 1992b: xiii), which is taken to mean that human behavior maximizes reproductive output.

However, AEE employs various measures of fitness; that is, "proximate currencies" (e.g., rate of food intake, number of surviving offspring, fertility rates, etc.), which are assumed to be highly correlated with the "ultimate currency" of reproductive success (Smith and Winterhalder 1992a: 34).

The assumptions made by AEE about natural selection, adaptation, fitness, optimality and equilibrium are problematic. First, AEE assumes that the human groups studied exhibit high reproductive success and stability (Smith and Winterhalder 1992a). However, as Bettinger (1980) notes, a theory that is founded upon generalizations about consequences is difficult to transform into one of evolutionary process or change (which Bettinger argues is what evolution is supposed to be about). Bettinger also claims that to start out with theoretical assumptions about evolutionary outcomes (in this case about optimality and equilibrium) retards "an evolutionary inquiry from the start by placing the cart before the horse and positing consequence as cause—assuming as it were one's conclusions" (1991: 216). This methodological approach, described by Gould and Lewontin (1979) as the adaptationist program, characterizes AEE research. 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 (Bettinger 1991: 222). Further discussion of how such problematic assumptions and concepts lead to weaknesses with models and overall explanatory framework within AEE will be taken up in the sections on derived conceptual devices and on theory framework and structure.

Empirical Content of Theory: Facts and Confirmed Generalizations in Anthropological Evolutionary Ecology

AEE can contribute in some ways to our understanding of human ecology. Empirical contributions from optimal foraging theory (which is considered to be the most developed of all AEE

theories, see Smith and Winterhalder 1992a; Smith 1992a, 1992b) and life history theory will be evaluated. Optimal foraging theory will be discussed first.

The key assumption of optimal foraging models is that foragers will make choices that yield the highest feasible rate of return (usually measured in energy) on their foraging effort (usually measured in time) (Smith 1991, 1992a). Researchers utilizing these models demonstrate that foragers often choose prey that maximize the mean rate of energy return per unit handling time and that other edible lower ranked resources (that offered lower returns) were ignored (Hawkes et al. 1982, Hill et al. 1987, Smith 1991). The problem with the empirical observations generated by these simple optimality models is not that they have no empirical merit, but that they have a much narrower meaning and significance than recognized within AEE.

Empirical observations do not stand alone. They are linked to conceptual constructs and the explanatory framework of theory. A major problem with optimal foraging models lies in their failure to consider the distinction and interplay between prediction and explanation, pattern and process, as well as the importance of scale. For example, Boone and Smith (1998) cite numerous examples which predict, using foraging theory, that humans will expand their prey choice to include lower-ranked (higher-cost) prey types as encounter rates for higher-ranked resources are reduced (p. S146). They believe this optimal-prey-choice model (often referred to as diet-breadth model) "explains" individual decisions. Explanation here is equated with prediction with no discussion of the distinction between prediction, causal process, pattern, and mechanism. There is no recognition that the processes which gives rise to the observed behavioral pattern may be entirely different in different cases, or that just because you have successfully predicted something does not mean that you have satisfactorily explained it (see also discussion of predictive models within AEE in the section on derived conceptual devices).

This point can be illustrated with a few examples. Boone and Smith (1998) provide the example from the work of Broughton (1994, 1995) who finds that the pattern of decline in the abundance of more profitable (higher-ranked) large-bodied prey (such as deer, elk and sea otters) to include lower ranked prey such as shellfish, small game, and fish in pre-historic Central California was due to long-term increase in human population density. However, the authors point out that whether the cause of dietary expansion was due to climatic change, human overpopulation and/or human overexploitation is not the issue since all that matters is that the optimal model predicts the same broadening of the diet. However it is the environmental and historical context in which behaviors are found that gives them theoretical significance. Otherwise, empirical facts lose their meaning and become trivial. Let us take another example.

In a more recent example of technological change in foraging behavior, Boone and Smith (1998) cite the work of Winterhalder (1980, 1981) who found that Boreal Forest Cree adopted the use of snowmobiles at the expense of snowshoes in hunting. Boone and Smith (1998) "explain" this behavior using the same actor-based optimality logic they use to explain the previous example by Broughton (1994, 1995); that is, by arguing that because individuals have evolved cognitive capabilities that allow them to perceive the relative efficiency of different means for acquiring resources, they will make decisions which will produce the highest net gains (p.147). No consideration is given to historical contingency in their discussion. That the recent example of snowmobile adoption may be due to penetration of capitalism into precapitalist modes of production is not considered relevant. However, as Bradby (1980) and Bettinger (1991) suggest, the economic "rationality" of hunter-gatherers is closely intertwined with the historicity of capitalism and thus a more detailed investigation of how the choice of prey, technology, and other

socio-economic and cultural behaviors in traditional societies have been shaped by capitalist exploitation, dependency and hegemony is required. The empirical facts provided by AEE are thus either applicable in highly limited circumstances or better explained by alternative theoretical formulations which recognize biophysical and socio-cultural structures and their historical contingency.

Smith (1991) seems to be at least implicitly aware of the importance of historical processes. For example, in his research among the Canadian Inuit (Smith 1991), he observes that even when utilizing different methods and measures and cost-benefit analyses among these Arctic foragers, foraging still emerges from these calculations as less profitable than alternative sources of livelihood (p. 393). Simple optimal models cannot account for foraging behavior at different historical periods. Thus, Smith is left with a dilemma: if humans make optimal decisions,² then why do the Inuit continue to engage in a large amount of foraging during the observation period of his study?

A more complex theory, unburdened by optimality assumptions, which recognizes for example the trend of increasing monetization, mechanization and proletarianization in traditional societies on the one hand, and the historical process of dissolution (which may range from gradual to rapid, partial to complete) of direct producers from the means of production engendered by the historical expansion of capitalist structures and relations, on the other, is required. 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 contingent upon particular historical epochs and social structures (see Hobsbawm 1961: 9-65). 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. Rather, such historical processes involve the presence of complex state structures, socio-

²The persistence of behaviors and institutions that are suboptimal and/or maladaptive are numerous. For a discussion of the 'survival of mediocre' institutions/practices in human social evolution, see Hallpike 1988: 81-145.

political 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 (see Pickett et al. 1994:37) and locate individual behavior in their complex multiple environments, which include physical, biological, social and cultural at a minimum (see Figure 3 for a graphical representation of a more complete concept of environment).

Such criticisms point to the limitations of empirical findings derived from simple optimality models. Bettinger (1991: 83-130) provides an excellent summary of optimal foraging models, including their limitations. Several points of Bettinger's argument are relevant to the discussion of the AEE domain and will thus be reiterated here. Bettinger (1991) points out that while simple contingency models (e.g., prey choice model) found within AEE might be useful in understanding some

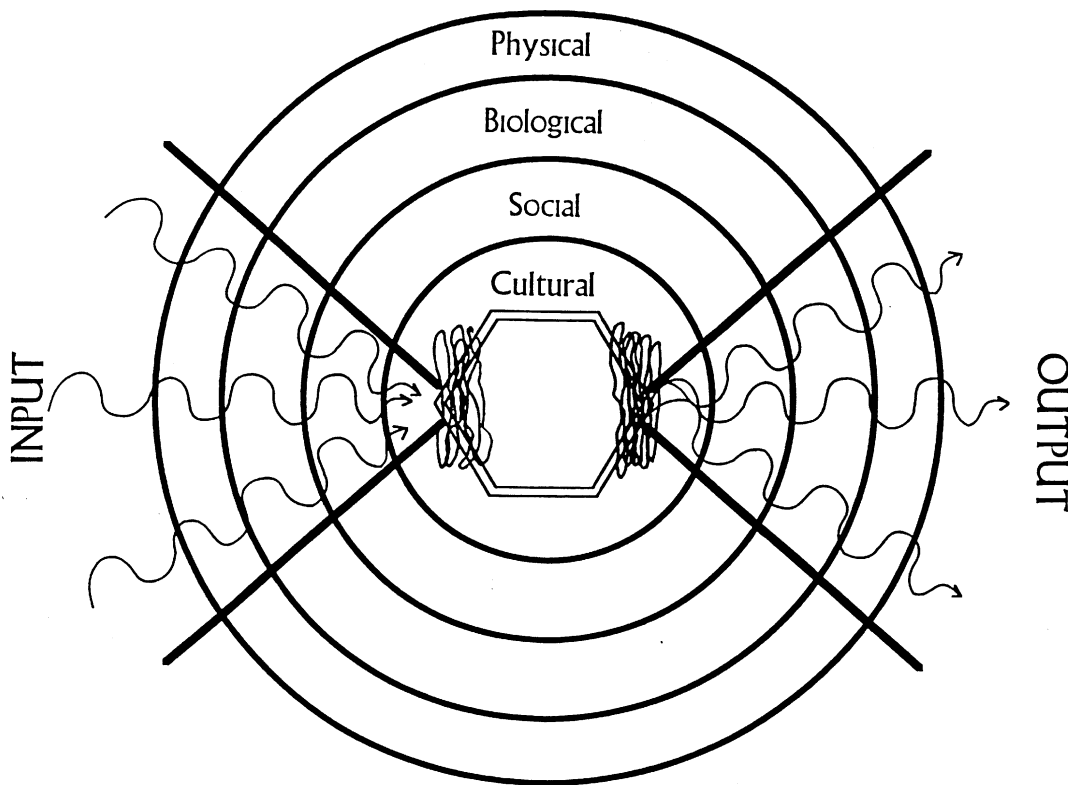


FIGURE 3. A PARTIAL THEORY OF MULTIPLE ENVIRONMENTS.

Concentric spheres denote an evolutionary arrangement of different environments, with an aggregated consumer symbol at the center. Information inputs and outputs to the system pass through epistemological filter/field/editor/screens (Stepp 1999).

sorts of behavior, it does not follow that properties observed in contingency models (e.g., cost/benefits) are fixed except for relative abundance of resources, and that individual foragers exist in isolation and that groups/collectivities can be conceived to act as independent individuals) are of general applicability in the real world of *Homo sapiens*. Bettinger provides an example from the work of Winterhalder et al. (1989) within AEE to illustrate this point.

Winterhalder et al. (1989) use simulation models of predator prey interactions to examine the impact of foraging behavior on prey (which is not accounted for in optimal foraging models of dietary breadth, since the model rests on momentary contingencies and assumes that prey availability is unaffected by foraging behavior) (see Smith 1991, Bettinger 1991). One of the unanticipated insights to emerge from these simulations is that increases in the amount of time spent foraging results in long-term decreases in human population (Winterhalder et al. 1989). If one assumes that prey availability is less than required to sustain a given forager population level and individual caloric requirement, then increasing work hours only acts to decrease further the size of the prey population, which in turn leads to further energetic deficits for foragers. This simulation reveals that over the long-run, predator/prey interactions are of fundamental importance, particularly given constraints such as time and resources.

Another example which points to the empirical inadequacy of simple individual actor-based optimal foraging models is found in the work of Winterhalder (1986) on sharing. When modeling human foragers as isolated individuals or unified collectives that act like individuals, one loses sight of the importance of the social and cultural environment in influencing foraging behavior. In assuming interaction beyond the individual level, Winterhalder (1986) shows that sharing is a very effective means of reducing risk among threshold-sensitive hunter-gatherers (i.e., hunter-gatherers

who are confronted by variable rates of energetic return—a possibility ignored in simple optimal foraging models which assume fixed rates of return—and thus whose survival is increasingly threatened if rates of energetic return fall below a minimum threshold). Winterhalder (1986), utilizing hypothetical data, observes that varying diet breadth alone can provide only small incremental increases in risk reduction³ and results in large reductions in foraging efficiency. In his simulation, resources were assumed to be randomly distributed and the coefficient of variation (i.e., SD/x , where SD is the standard deviation and x is the mean) of pursuit time was uniformly set at 0.33. Increasing diet breadth to minimize Z (the probability that the behavior will result in a return rate below the threshold) reduced standard deviation in return rates by 8% and foraging efficiency by 6%. In contrast, under the same simulation conditions, sharing between just two foragers whose rates of return were considered independent of each other, reduced the standard deviation in the rate of return of each forager by 30% and did not affect the mean rate of return of either individual.

Winterhalder then uses a simple formula to explore how correlation between foraging rates would affect the size of the potential gain that results from sharing. Winterhalder finds that there is a diminishing effect of increasing size so that as group size increases, the costs associated with the addition of new members (in terms of resource depletion for example) would eventually outweigh further gains that would result from reduced variation in pooled rates of return. Thus, he concludes that relatively small groups capture most of the benefits of risk reduction associated with sharing. This example illustrates the importance of cooperation in human societies where risk reduction and resource shortfall are prevalent (Bettinger 1991: 126). One could imagine a host of other social, economic, political and cultural factors ignored by optimality models which directly or indirectly influence foraging behavior (e.g., politi-

³ Under the expectations of risk-minimization explored by Z-score models, foragers ought to constrict diet breadth when resources are abundant (that is, when resource thresholds are greater than the maximum expected rate of return) and expand diet breadth when resources are scarce (that is, when thresholds are below the maximum expected rate of return) (see Stephens and Charnov 1982, Winterhalder 1986).

cal-economic trade alliances, endemic warfare, political ties to neighboring nation-states, storage and its defense requirements, technologies, socio-cultural redistribution systems, gendered division of labor, cultural preferences, etc.). Many of these influences can be modeled qualitatively if not quantitatively and would contribute even further to social and cultural environmental influences on foraging behavior.

So, as Bettinger (1991) maintains, when discussing models of evolutionary processes, it is important to keep in mind that "Even the simplest [social and cultural] systems that contain interactive elements often behave in unexpected ways" (p. 130). However, there seems to be little attempt within AEE to delineate the boundaries or domain of their investigation. 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.

What about life-history theory (LHT)? Does LHT make empirical contributions to our understanding of human ecology? LHT has contributed to our understanding of differences in life history traits among higher level taxa.⁴ Specifically, LHT has made an important contribution to human ecology by highlighting central features of our life history, such as long post-menopausal lifespans, late ages at maturity, short interbirth intervals, and high fecundities (Hawkes et al. 1997). Theoretical explanations of human life history traits, as well as a precise knowledge of their timing in our species' evolutionary history are still at an early stage of theoretical maturity. However, within AEE, functionalist/adaptationist explanations are still the rule (cf. Hawkes et al. 1997).

It is important to note that much of life history theory involves patterns derived from comparisons across taxa (e.g., Promislow and Harvey 1990 who discuss the effects of mortality sched-

ules on species' life-history characteristics). Biological anthropologist Carol Worthman (1993) cautions that determinants in the evolution of life-history characteristics deduced from such comparisons cannot be directly applied to explanations of phenotypic variation among individuals (see Smith-Gill 1983 and Lessells 1991 for discussion). Furthermore, Lee suggests that an understanding of human reproductive ecology requires attention to socio-cultural factors. As Lee (1993) maintains, LHT might be less influential in explaining reproduction in human societies where "cultural escape routes" from biological constraints are evident (e.g., in humans, sex and reproduction are not always synonymous; neither are age at maturity and age at first birth synonymous; nor is sexual activity synonymous with mating effort in the classic mammalian sense of the term).

However, practitioners within AEE have failed to appreciate the importance of the domain and the role of culture when utilizing life history theory, which may go a long way toward explaining the failure of some key predictions from LHT to help account for behavioral variation within human societies. The empirical contributions (or lack thereof) of three central issues in human LHT will be evaluated: 1) birth spacing as a life history trait; 2) the life history trade-off between current and future reproduction; and 3) the "production function" that Charnov (1993) sets at the root of life history theory.

Birth spacing using the optimal birth interval model has been tested in a few cases. Blurton Jones and Sibly (1978) investigated such trade-offs among the !Kung of the Central Kalahari. The model is based on the logic implicit in Lack's (1954) proposition: the best number of offspring to produce is that which yields the highest number of surviving offspring. While the !Kung study was successful in that the expected distribution matched the observed distribution of birth intervals in the population (the modal interbirth interval), many problems with the model were later revealed (see

⁴ Nested ANOVAS which assign the total variance among life history traits in mammals to orders, families, genera and species, indicate that most variation in life history traits arises at the level of orders and families (Read and Harvey 1989).

Harpending 1994; Hill 1993; Hill and Hurtado 1996; Pennington and Harpending 1988, 1993). One of the major methodological problems with the model was the fact that child survivorship was not actually measured (Hill 1993). In a study that utilized data from a wider !Kung population, Pennington and Harpending (1988) failed to fit predictions they drew from Blurton Jones and Sibly. However, the closest replication of the Blurton Jones and Sibly model is by Hill and Hurtado (1996). However, the authors failed to predict observed modal interbirth intervals when using this model among the Ache of Paraguay. Thus, we are still left with a case of one, with several methodological shortcomings.

Hill and Hurtado (1996) also examine the life-history trade-off between current and future reproduction. The expectations of trade-offs between alternative life history functions that require energy is a basic premise of LHT. Thus, the authors predict that high fertility early in the reproductive careers of females should entail negative impacts on their subsequent survival or fertility. However, the authors failed to find empirical confirmation of this trade-off even after controlling for socioeconomic status, body size, and hunting return rate through multiple regression analyses (Hill and Hurtado 1996: 391). Early fertility was positively correlated with subsequent fertility among the Ache (*ibid.*). In fact, the authors indicate that they failed to demonstrate that other significant life-history trade-offs exist: "Nevertheless, for the most part, we were unsuccessful in demonstrating that life history trade-offs do exist" (Hill and Hurtado 1996: 391).

The third hypothesis derived from LHT referred to as the "production function," states that biological productivity increases with size; at maturity (in determinate growers) productivity is switched from growth to reproduction. Hill and Hurtado (1996) found that larger Ache women have more frequent births. Their finding appeared to establish a simple explanation for differences between !Kung, Hadza, and Ache fertility (adult women weigh 40, 48, and 53 kg., respectively, and bear 4.7-5.0, 6.2, and 7.8-8.3 babies per career).

However, enlarging the sample by taking fertilities from Hewlett (1991) and obtaining adult female body weights from other publications gives a very small and nonsignificant correlation between fertility and body weight (Pearson correlation coefficient = 0.06, $P = 0.90$, $n = 7$) (Blurton Jones 1997). Thus, while LHT can contribute to our understanding of the differences in life history traits between humans and other species and/or higher level taxa, broadening the domain of LHT to account for phenotypic variation within human societies has contributed few if any interesting insights, suggesting a need to limit the domain of LHT.

Derived Conceptual Devices: Hypotheses and Models

Hypotheses in AEE are usually generated from simple optimality fitness models driven directly or indirectly by natural selection and as such are relatively easy to grasp. However, the use of models in AEE requires further discussion since many methodological problems with these models have been identified. For example, Vayda (1995a) has argued that practitioners within AEE believe that they are explaining behavior by eliciting the beneficial consequences of behavior. Vayda (1995a) suggests that AEE sees explanations in much the same way as economics did after the 1950s when predictive schemes were equated with theory with no attention to the other components of theory. Popper (1965) himself warned against regarding theories simply as predictions and reducing explanation to a practical technological interest in predictions. Thus, Vayda (1995a) argues that the hypothetico-deductive method found within AEE has led to equating explanation with setting forth predictive "models," testing predictions generated by them and obtaining confirmation of these predictions. Vayda (1995a, 1995b) refers to this problem as "naive functionalism," or what might be better described as hyperfunctionalism, whereby finding beneficial consequences is tacitly regarded as sufficient for explanation with no allowance made for the possibility that the benefits in question are the by-products of behavior not caused, either now or in the past, by the benefits themselves.

Awareness of this problem is growing in evolutionary biology (e.g., concepts such as “exaptation” have been constructed to address the evolution of characters that may not have been shaped by natural selection but now influence reproductive success; Gould and Vrba 1982). However, little attempt has been made by practitioners of AEE to refine their methodological approach.

Vayda (1995a) suggests that a partial solution to this problem consists in demonstrating (rather than assuming or asserting *ipso facto*) how particular occurrences of certain behaviors at particular points in time have consequences causing the same (or similar) behaviors to occur at later points in time. So causal mechanisms or causal chains would have to be demonstrated, assuring researchers that the beneficial consequences under discussion are not merely by-products of behavior (see Vayda 1995a).⁵ There is no consideration as to whether or not a Darwinian analysis is appropriate in a given context. However, Vayda (1995a) warns that usually there are no data on actual cause-and-effect sequences—no data on whether there was trial and error learning by some which was followed by imitation; whether there was intense negotiation and consensus by yet others, whether greater reproductive success of those who possess certain behaviors then taught those to their children, or whether the “fitness” consequences were merely a by-product of other socio-cultural processes, etc. (p. 228). This means that disposing of the by-product possibility when there is no direct historical evidence of the causal history of the behavioral trait in question is difficult if not impossible, and as Vayda (1995a) maintains, may leave us a long way from being able to

invoke adaptation by natural selection as having any special explanatory import for a given behavior (p. 228).⁶

As Bettinger (1991) points out, without an independent model of cultural transmission, AEE (much like neofunctionalism and cultural materialism) is forced to interpret cultural behavior in purely adaptive terms—as behaviors that promote “adaptation.” Some argue that this is a reasonable working hypothesis and constitutes a viable way of understanding how phenotypic traits/behavior fit the biological environment (Borgerhoff Mulder 1991, Maynard Smith 1978). However, the downside is that oftentimes one simply assumes that human behaviors are “adaptive” and it is one’s job to show that this is so. Hallpike (1988) describes how social scientists use the concept of adaptation as a kind of “Open Sesame!” to explain every puzzling institution. As a result, explanations degenerate into the unfalsifiable belief that survival is itself the proof of adaptive value. The dangers are obvious and Maynard Smith (1978) recognizes this as the most serious and damaging challenge facing evolutionary behavioral ecology.

Many have argued that adaptation and optimality are not necessary implications of Darwinian theory (Bettinger 1991: 223). Shouldn’t a Darwinian analysis begin with the question “is behavior adaptive” rather than assuming it? Also, the behavior may be adaptive but never selected for in the Darwinian sense. This illustrates the further weakness of AEE models—the Darwinism employed by AEE.

Darwinian theory is equated with a simple prediction within AEE which can be stated as fol-

⁵Vayda (1995a) maintains that many evolutionary biologists have tackled this problem similarly to social scientists and attempt to deal with the criticism of distinguishing between traits that are by-products or the effects of non-Darwinian circumstances from those that have been fashioned by natural selection by using either comparative studies or “design” analyses.

⁶Also problematic with AEE assumptions about natural selection is the failure to distinguish clearly between natural and sexual selection as Darwin did; but rather AEE tends to subsume sexual selection within natural selection (see Mayr 1972 for a discussion of distinctions between natural and sexual selection). Mayr (1972) thus points to the importance of questioning, as Darwin did, whether there are forms of sexual selection which result in the evolution of characters that are useless or deleterious to the species (e.g., the excessive plumes of birds of paradise and peacocks as well as antlers of certain cervids such as the Irish elk). Thus, as Mayr maintains, “it is distinctly conceivable that extreme courtship adaptations acquired as a result of sexual selection may actually reduce the ecological success, that is, the fitness, of a species” (1972: 101).

lows: as a result of natural selection, human beings like other organisms, will “maximize” the net (benefits minus costs) fitness results of their possible behavioral options (Kaplan and Hill 1992). Criticisms of the procedure of reducing Darwinian theory to this simple prediction have been made by evolutionary psychologists, most notably Symons (1989, 1990, 1992). Symons (1989) states that nowhere in his published or unpublished writings does Darwin suggest that the theory of evolution by natural selection implies that human behavior can be expected to maximize reproductive success. So the criticism that AEE has reduced Darwinian theory to a prediction that Darwin never made is a criticism that deserves more attention. Inadequacies in AEE models are severe, and sophisticated mathematical modeling is no substitute for more robust theoretical assumptions, basic concepts and derived conceptual devices.

Theory Frameworks and Structure in Anthropological Evolutionary Ecology: Framework and Translation Modes

While domain has been discussed at some length, framework and translation modes are also key components of theory framework and structure. Frameworks serve to unite all components of theory into a coherent conceptual structure. Relationships between conceptual devices are specified, including the relations between assumptions, theoretical constraints and the conceptual components of theoretical models. Translation modes facilitate the operationalization of abstract ideas, generalizations and models to actual field situations and/or experiments and back again.

AEE has attempted to refine its neo-Darwinian explanatory framework by distinguishing between history and evolution. Historical change refers to the period of human history characterized by nonselection-driven change, but still capable of producing adaptive changes due to phenotypic plasticity in response to environmental conditions (Boone and Smith 1998). “History” is said by AEE to be driven by human intent or decision-making

whereas evolutionary change is natural selection-driven. In short, AEE assumes “that the [historical] trait under study has been designed by natural selection to have sufficient phenotypic plasticity to track environmental variation optimally” (Boone and Smith 1998:S145). That is, humans have evolved cognitive abilities during our evolutionary past for perceiving the relative efficiency of different ways of acquiring resources. These skills are expected to be reflected in behaviors today. Thus even though the ability to respond facultatively is a product of natural selection, AEE argues that this is not evolution in the strict sense since it does not necessarily involve changes in culturally transmitted trait or gene frequency.

However, there are several problems with this framework. First, 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? How do translation modes, which facilitate the conceptual transposition of abstract ideas, generalizations and models to on-the-ground research differ when examining *Homo sapiens neanderthalensis* as opposed to *Homo sapiens sapiens*, early and contemporary hunting and gathering groups, early urban and modern urban folk? In order to untangle the complexities of human behavior, more robust translation modes which address human behavior at different evolutionary and historical scales and levels of social organization and complexity are required.

Equally problematic is the use of natural selection-driven change as a major explanatory framework within AEE. Whether one argues that a given trait exists because of phenotypic adaptation or natural selection, both explanations are functional/adaptationist in nature. Behaviors or traits, whether directly or indirectly the result of natural selection, are still posited as being largely the result of the explanatory mechanism of natural selection.⁷

⁷The natural selectionist framework is also at the heart of another anthropological Darwinian theory, Evolutionary Archaeology (EA). EA considers social systems to be populations of competing behaviors and their products; that is, cultural artifacts. Under selectionism, cultural evolution refers to the differential representation of variation at all scales among cultural traits or behaviors (Jones et al. 1995: 28).

No role is given to cultural processes or socio-cultural constraints. Further, AEE tends to ignore the role of social institutions and other socio-cultural processual explanations of change. AEE with its strong methodological individualist stance fails to appreciate the hierarchical nature of social life and the role of social structures and processes in shaping and constraining individual behavior, and envisions a problematic separation of the organism from its broader environments.

The theory of environment found in AEE is too narrow in its adherence to a Darwinian separation of the organism from its environments. The problem with this distinction in human socio-cultural systems will be discussed next.

AEE upholds the Darwinian notion of the distinctiveness of inherited variations in organisms on the one hand, and environmental selection on the other. As Lyman and O'Brien (1998) point out, the notion of independence between organismic variation and environmental selection cuts evolutionary theory loose from the teleological system which had bound it up until the writing of *The Origin of the Species*. The problem however, lies in applying this distinction to human socio-cultural systems. Should we assume that, like organic traits, cultural traits and cultural behaviors are distinct from their environment?

In human systems, the distinction between organism and environment raises serious problems. Cultural traits are not clearly bounded entities like organic traits or biophysical environment traits, which add up to form populations of traits; rather in human societies, cultural ideas, institutions, values, and behaviors are parts of organized systems of meaning bounded together largely by information flows (Hallpike 1988: 27). In human societies, we thus have various subsystems such as marriage systems, inheritance systems, kinship systems, gender systems, political systems, etc., which have certain structural properties. The attempt to discretely isolate a particular trait or a number of traits from these subsystems is to ignore social organization. Ingold (1998) similarly points out that recent work in anthropology stresses the inseparability of knowledge and practice and the embod-

ied character of cultural skills of action and perception (see also Bourdieu 1990, Csordas 1990, Lave and Wenger 1991). 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. Behavior must be understood in the context of social institutions.

For those who are unconvinced that cultural traits and behaviors are not without paradoxical structure and hence not merely an aggregated population of (behavioral) traits (or adaptations resulting from individual decision rules or behavioral strategies), it might be useful to consider exactly what is implied by consideration of cultural traits as phenotypic traits. Hallpike (1988) highlights the problem of viewing human cultural traits in isolation from their environment. He states: "artifacts, firms, customs, institutions, etc. do not just compete in an environment, they are the environment, since they mutually affect one another and contribute to one another's existence." Thus cultural selectionism leads to a complete confusion of the organism/environment distinction crucial to Darwinian theory. As Hallpike points out, "selection itself is no longer an independent variable operating from outside society but becomes an integral aspect of its internal working, a dependent variable in fact" (p. 57). For Darwin, selection as a concept is not a teleological force and thus the process that does the selecting should not itself be directly affected by what it selects. Thus, I submit that the concept of cultural selectionism has no conceptual utility and should be rejected. Again Hallpike sums it up best: "If as in [human] society, everything to some extent can potentially affect everything else, there is absolutely no point in retaining the concept of selection at all: mutual selection is nothing more than mutual interaction" (ibid.).

However, AEE claims to differ from other reductionist theories in biology (e.g., sociobiology) because of its emphasis on behavioral "plasticity" in response to environmental contingency. Upon closer examination, however, this distinction is little more than cosmetic. The notion of environment

employed by AEE is inadequate. In terms of basic concepts and explanatory framework, AEE continues to look internally to answer fundamental questions about human behavioral variation and change. "Environment" is no more than a passive background, a shifting mosaic against which an ahistorical functionalist narrative is imposed.⁸ In AEE, when the environment is alluded to, it is so artificially restricted as to be useless.

AEE defines environment as "everything external to an organism that impinges upon its probability of survival and reproduction. The effects can bear on development, physiology, or behavior" (Winterhalder and Smith 1992: 8). AEE distinguishes between parametric and strategic environmental contexts (ibid.). Parametric environmental contexts are defined as contexts (e.g., physical environment) independent of the organism's decisions and can be analyzed using simple optimization models. Strategic environments (e.g., social environment), those where the optimal behavior is dependent on the frequency of optimal and alternative behaviors, are studied using the concept of evolutionarily stable strategies (ESS) with concepts and models based on game theory. Foraging behavior is usually assumed to occur in a "strategic" environment and studied with optimization models. However, whether strategic or parametric, the environment is less significant than one might expect. It seems that whatever the context, the outcome is still predetermined (i.e., equilibrium in strategic environments or optimal fitness in parametric environments). Thus, in practice the concept of plasticity in AEE means the following: *Environments change and behaviors change accordingly, but the outcome, the name of the game remains the same: stability or fitness.* Is it not gratuitous to

invoke concepts of context (the notion of environment) and variability (plasticity) when the outcome is already predetermined?

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. As noted earlier, the sharp organism-environment dichotomy is a problematic concept in human socio-cultural systems. Individuals cannot be divorced from their broader environments and the complex structures and feedback processes they include. Moreover, because of the structural duality of externalized cognition, social institutions prevalent in human social environments are ontologically complex and not easily placed exclusively in either the input environment, the output environment, or the organism (see Stepp 1999). However, there is yet another problem with the theory of environment utilized by AEE.

The last weakness regarding theory framework and structure that will be discussed is that pertaining to culture. In the case of optimal foraging theory, there is an assertion that all other things save efficiency are essentially random and thus cancel each other out. The untestable position taken is that behavioral data on subsistence is more interesting and important than behavior with regard to cultural preferences, identity, style, etc. (see Bettinger 1991: 164). Given that culture can be ignored as noise in models (e.g., O'Connell et al. 1982) or 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 fu-

⁸In BEE, similarly, the environment is merely a changing background against which the details of the game of reproductive success are played out (for a discussion of "environment" in BEE see Eldredge 1999: 148). In BEE, the driving force behind evolution is competition for reproductive success among genes or among organisms of the same species within local populations. While BEE looks internally to address the question of what drives evolution, paleobiologists and paleoecologists see evolution differently. They see patterns in the evolution of life as both episodic and characteristically cross-genealogical, meaning that evolutionary pulses typically affect many lineages of a region simultaneously. Thus, Eldredge (1999) argues that the ecological realm must be more explicitly linked to the evolutionary process. If common ground is to be achieved in evolutionary biology, at the very least, additional theoretical structure must be developed. As Eldredge points out "Such a structure must go beyond the genetics of organisms within populations, or even beyond concepts such as punctuated equilibria, which acknowledges the episodic, but not cross-genealogical, aspects [for example, ecosystems] of patterns in the history of life" (1999:148).

ture to seriously consider the role of culture. Bettinger (1991) best characterizes the puzzling dismissal of cultural preferences in models employed by evolutionary ecologists. He writes:

Those who are convinced that these distinctly cultural phenomena are wholly unimportant are by any objective measure in the wrong business: It would be easier and far more profitable to work with another species on which any number of experiments can be performed and where there is no need to consider and circumvent by deference and respect alternative systems of thought and custom. In short, where the data are not, as Harris (1968: 361) puts it, "emically contaminated." Surely the study of economic optimality would be simpler without the "epiphenomena" of culture—without having to work around quaint native misunderstandings of the world as it actually is. (p.166)

Conclusions

AEE as a theoretical paradigm remains conceptually inadequate in several respects, but principally in its failure to consider the domain of theory and how historical and cultural processes impinge upon the structure and function of human ecological structures and practices. Specifically, in terms of basic conceptual devices, the concept of "environment" is too simplistic; the concept of "history" is still functionalist and ahistorical; *a priori* assumptions about optimality and adaptation seriously limit the empirical investigation of socio-cultural patterns and processes and usually leads to hyperfunctionalist post hoc accommodative reasoning; the assumption of methodological individualism prevents serious consideration of the role of institutions in social and cultural evolution as well as recognition of the hierarchical nature of social life; and the selectionist view leads to a fragmented analysis of human socio-cultural variation and change. The individual human as agent is even a caricature—sort of a mechanical and predictable cartoon character—an object of humor more than an actor in the complex play of life.

In terms of the empirical content of AEE, when taking the domain into account, most of the empirical findings are of narrow significance. One exception lies in the realm of life history theory which has contributed to our understanding of unique features of human life history. However, again the inability to find empirical confirmation of life history trade-offs within human societies points to a failure by AEE to consider the scope of theory. The derived conceptual devices employed by AEE are also problematic. Predictive models are often equated with explanation and the use of "behavioral strategies" used as translation modes do not specify the temporal scale of investigation.

The major critique pertaining to the theoretical structure and framework of AEE surrounds domain. The failure of AEE to bound the domain of its investigation is clear. In addition, in terms of causal explanation, AEE has failed to develop a more holistic theory of environment and seriously consider the role of culture in human evolution. All of these criticisms point to clear limitations of AEE as a theoretical paradigm and suggest the need for substantial refinement and revision or rejection of basic conceptual and derived conceptual devices.

In short, the search for how humans are just like other species is the underlying tradition in the approach criticized here. But each species is also unique, and it is in the search for explanations of human lifeways that we must develop theoretical frameworks more encompassing and ecologically sophisticated than those that can be borrowed largely unmodified from biology *sensu stricto*. Figure 4 provides a futuristic look, an initial attempt to develop a more encompassing framework for human evolutionary ecology. For many it may seem that the promise of an anthropological evolutionary ecology has been stolen. However, what I am calling for is a new evolutionary ecology, one that goes beyond the apologetic capitalist world-view of biology.

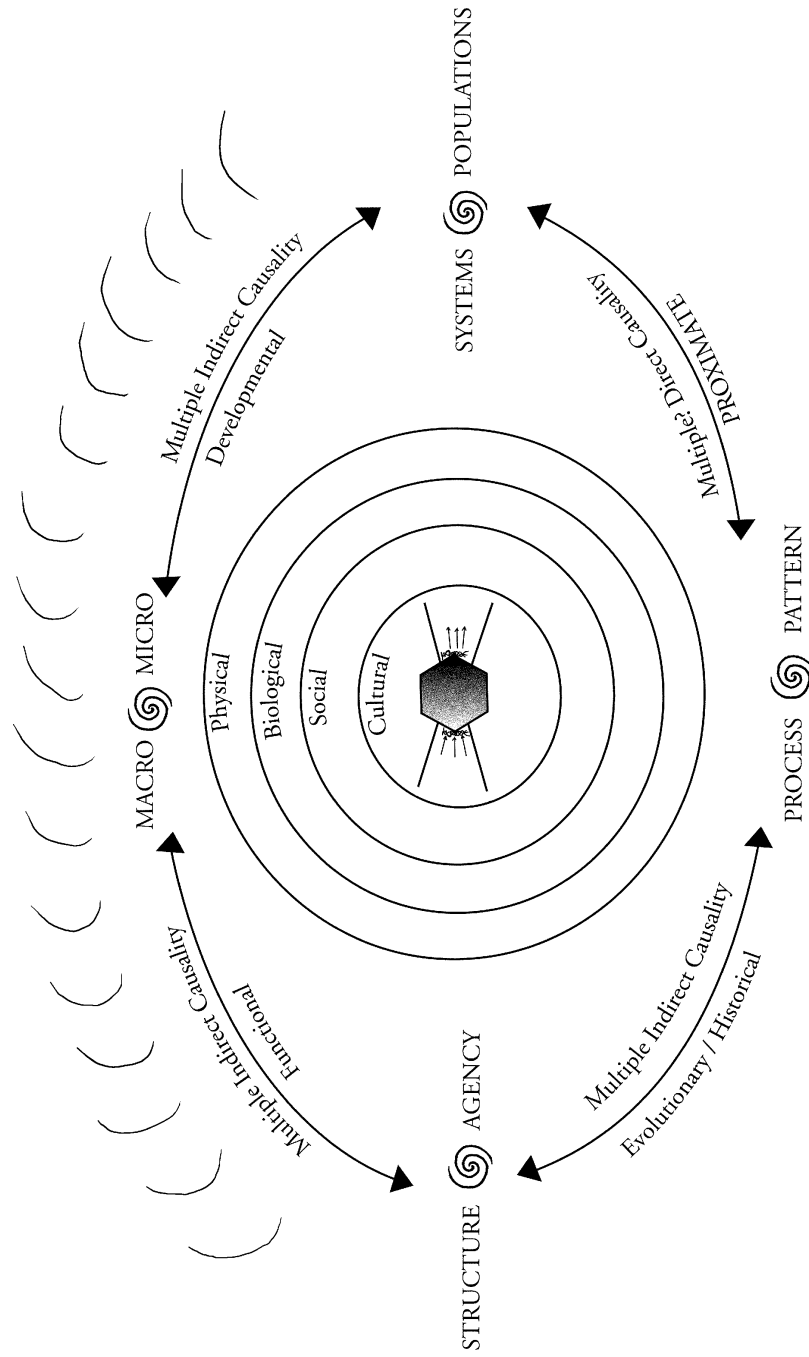


FIGURE 4. A PARTIAL FRAMEWORK FOR A NEW HUMAN EVOLUTIONARY ECOLOGY. (See Pavao-Zuckerman, Appendix B, this volume, for a key to symbols.)

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