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New perspectives on organism-environment interactions in anthropology

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Abstract

Anthropologists contend that the organism-environment connections responsible for human evolution are indirect—mediated by culture. This chapter reviews influential twentieth-century anthropological interpretations of the cultural mediation of human adaptations to environments, arguing that ethnography and other qualitative forms of analysis reveal important phenomena overlooked by quantitative analysts committed to methodological individualism. It highlights work by post-positivist anthropologists, who describe relations among human and non-human organisms, cultural forms, and features of environments as "natural-cultural" networks, an approach reminiscent of developmental systems theory and niche construction. Evolutionary theorists have much to gain by incorporating these sophisticated, contemporary post-positivist anthropological understandings of culture into their models of human-environment connections.

Keywords: ecological anthropology, cultural ecology, political ecology, niche construction, actor network theory

Introduction

In early twentieth-century North America, eugenicists were claiming to be able to sort and rank human populations in terms of biological "race," arguing that such biological "races" were the direct products of past natural selection. Anthropologist Franz Boas and his students challenged such claims, not by denying that the human species had evolved by natural selection, but by arguing that the organism-environment connections that produced distinct human adaptations were *indirect*, mediated by *culture*; that is, by learned beliefs and behaviors acquired by human beings as members of particular social groups. Because culturally-mediated human connections to the environment were shared, newborn human individuals were spared having to invent new mediated connections to the environment on their own, from scratch. Cultural mediation of human-environment connections appeared to be ancient, associated with a long period of post-natal dependency, during which human children learned from their elders the skills and knowledge (including language) required for successful survival and reproduction. In sum, human organisms were conceived as highly generalized, behaviorally plastic social organisms whose adaptive connections to their environments varied from population to population, because each population's connections to its environment were mediated by particular sets of cultural knowledge and skills, passed down from one generation to the next. Among other things, emphasis on the centrality of culturally mediated human connections to environments allowed anthropologists to explain human diversity without recourse to the concept of biological "race."

Since Boas's day, definitions of culture, and demonstrations of how culture mediates human adaptations to environments, have varied across subfields of anthropology, and have not been without contention (Abu-Lughod 1990, Trouillot 2002). Nevertheless, I argue here that contemporary post-positivist anthropological theorizing about culture can refine and strengthen understandings in theoretical biology about the roles culture may play in mediating human-environment connections. Post-positivist perspectives can be found in the work of anthropologists working across the subfields of biological anthropology, cultural anthropology, linguistic anthropology, and archaeology, and they are well-established in such growing specialties as environmental anthropology and the anthropology of science, technology, and medicine (both of which regularly incorporate insights derived from a newer subfield, applied anthropology). To be sure, post-positivist anthropology remains controversial among those anthropologists who continue to believe that positivist science is science *tout court*, and it is ignored by theorists of cultural evolution who cast their discussions of the cultural mediation of human-environment connections in terms of gene-culture coevolution (e.g. Boyd and Richerson 1985; Richerson and Boyd 2005; Durham 1991). At the same time, most anthropologists who adopt post-positivist perspectives often describe their diachronic analyses of culturally mediated human-environment connections as "historical" rather than "evolutionary," and they rarely engage with selectionist and adaptationist forms of explanation. But it is also true that adaptationist and selectionist arguments offer few resources for illuminating the messy contingencies shaping the kinds of organism-environment entanglements of interest to post-positivist anthropologists.

In my view, serious scholarly discussions of organism-environment connections can no longer afford to ignore the post-positivist anthropological contributions reviewed in this essay, for at least three reasons: (1) this work demonstrates the breadth and sophistication of contemporary post-positivist anthropological analysis, and exposes the speciousness of allegations that rejecting positivism means rejecting science, or that criticizing selectionism means rejecting evolution; (2) this work highlights valuable insights gained from ethnography and other qualitative forms of analysis, thereby exposing the limitations of social science perspectives that favor quantitative analysis and commitments to methodological individualism; and (3) this work displays surprising affinities with insights from "dissident" traditions in theoretical biology, such as developmental systems theory and niche construction, which should be further developed.

My argument is set out in four parts. Part 1 begins with Julian Steward's "classic" mid-twentieth-century account of the cultural mediation of organism-environment connections, recalls the criticism it generated, and describes successor approaches like political ecology that became well established in the late twentieth century. By highlighting struggles to better account for the patterns Julian Steward attempted to capture in his distinction between the core and periphery of human cultural adaptations, this section shows how some anthropologists came to abandon the assumption that either "cultures" or "environments" could be unproblematically conceived as separate, self-contained entities. In addition, it shows how ethnographic work in colonial and post-colonial settings led some anthropologists to draw attention both to the capacities of human beings to rework cultural mediations in drastically altered environments, and to the importance of contextualizing these processes within fields of power.

Under conditions of post-Cold-War globalization, however, some anthropologists have found that political ecology cannot fully account for the remarkable ways people everywhere now mediate connections to contemporary environments, mixing and matching cultural objects and practices inherited from the past with cultural objects and practices imported from elsewhere. Part 2 showcases some of the innovative post-positivist anthropological research that attempts to make sense of these processes. It also shows an imbalance in interdisciplinary exchanges between theoretical biology and anthropology, for this and other relevant anthropological work has been largely ignored by theoretical biologists who write about culture. I describe significant anthropological research inspired by thinkers like Charles Sanders Peirce, Bruno Latour, and Gilles Deleuze and Félix Guattari, which can help theoretical biologists provide more nuanced accounts of the cultural mediation of human-environment connections. Finally, in Part 3, I show one way of more directly connecting this newer anthropological work to current work in theoretical biology. It turns out that key features of actor network theory, developed by Bruno Latour and his colleagues, bear a strong family resemblance to key features of niche construction, developed by John Odling-Smee and his colleagues. Drawing on a recent ethnography, I show how conceiving of constructed niches as actor-networks can provide a path that permits insights from cutting-edge post-positivist archaeology and cultural anthropology to enter into current discussions of organism-environment connections in theoretical biology, where they are badly needed. This move requires abandoning dualistic "nature-nurture" thinking for new perspectives that conceive of relations among human organisms, cultures, and environments in terms of "natural-cultural" networks. But it also

promises to incorporate a more refined understanding of culture into theoretical biology, where it is long overdue.

1 Cultural Ecology and its Progeny

Any anthropological discussion of relations between human organisms and their environments must begin with Julian Steward because, as ecological anthropologist Emilio Moran observes, "Steward delimited, more than anyone before him, the field of human/environment interactions" (Moran 1990, 10). Indeed, Steward's *Theory of Culture Change: The Methodology of Multilinear Evolution* (1955) embodies both the achievements and the difficulties that continue to challenge anthropological thinking about culture, ecology, and evolution. This volume contains the fruit of Steward's prewar ethnographic comparisons and theoretical innovation; his critiques of L. H. Morgan's and E. B. Tylor's nineteenth-century unilinear evolutionary schemes (and their twentieth-century descendant, the "universal" evolution of Leslie White and V. Gordon Childe); the lessons learned from Boasian "cultural relativist" ethnography; and the limitations of British functional anthropology. His multilinear evolutionary approach is meant to avoid the pitfalls of these alternatives, without abandoning scientific cause-and-effect explanations. Steward struggles mightily, however, to bind all these threads together:

> Whereas [Morgan, Tylor, Childe, and White] have sought to formulate cultural development in terms of universal stages, my objective is to seek causes of culture change. Since 'evolution' still strongly connotes the nineteenth-century view, I hesitate to use it but find no better term.

Chapter 2 of Steward's book develops a method for recognizing the ways in which culture change is induced by adaptation to environment. This adaptation, an important creative process, is called *cultural ecology*, a concept which is to be distinguished from the sociological concepts "human ecology" or "social ecology." The cross-cultural regularities which arise from similar adaptive processes in similar environments are functional or synchronic in nature.

> But no culture has achieved so perfect an adjustment to its environment that it is static. The differences which appear in successive periods during the development of culture in any locality entail not only increasing complexity, or quantitatively new patterns, but also qualitatively new patterns. Consequently, in the comparison of the history of two or more areas in which the cultural ecological processes are the same, it must be recognized that a late period in one area may be much more like a comparable late or homotaxially similar period in another area than the earlier periods in either area. Cultural development therefore must be conceptualized not only as a matter of increasing complexity but also as one of the emergence of successive *levels of sociocultural integration*…. Chapter 4 illustrates the application of this concept at a national-level system (1955, 5).

Three additional concepts central to Steward's cultural ecology were *cultural type, cultural core,* and *form-function*.

> The concept of *cultural type* … is based on the two frames of reference previously presented: cultural features derived from synchronic, functional and ecological factors and those represented by a particular diachronic

developmental level. Cross cultural regularities are . . . recurrent

constellations of basic features—the *cultural core*—which have similar

functional relationships resulting from local ecological adaptations and

similar levels of sociocultural integration . . . . The concept of culture type is

confronted by the apparent difficulty posed by the fact that forms, patterns,

or structures differ greatly. Since, however, similar functions maybe served

by different forms while similar forms may serve varied functions, the single

concept of *form-function* is introduced. (1955, 5-6)

Steward thus understood each cultural type he identified to be a

synchronic/ecological *and* diachronic/developmental (and perhaps evolutionary)

synthesis. Chapters 6-12 of his book discuss a series of cultural types "presented …

according to their level of sociocultural integration" (1955, 6). These included what he

called the "family band" (the lowest level); "patrilineal hunting bands" and "composite

hunting bands" with "slightly higher levels of sociocultural integration"; "nonlocalized

clans," which "represent a higher level of sociocultural integration than localized lineages"

and which "probably developed from such lineages many times in different parts of the

world"; and complex civilizations that developed on the basis of irrigation agriculture,

writing that "Chapter 11 shows how in each of these areas fundamentally similar cultural

ecological adaptations entailed a similar historical sequence" (1955, 7). In Chapter 12, he

applies cultural ecology, levels of sociocultural integration, and culture type to "a complex

contemporary society, Puerto Rico" (1955, 7).

Steward's approach was not lockstep: an "environmental possibilist," he highlighted

"instances where the interrelationship between culture and environment allows

considerable latitude or potential variation in sociocultural types. Where latitude is possible, historic factors may determine the nature of society" (1955, 6). Nevertheless, ten years after *Theory of Culture Change* appeared, cultural ecology had been thoroughly picked apart. Some anthropologists criticized Steward for assuming that "cultures," rather than people, might adapt to environments. Others found Steward's positivist analytic goals to be highly problematic. Recent ethnography was also revealing difficulties in the data Steward had used to construct his cultural types.

Still, it is worth lingering a moment over Steward's discussion of the culture core, which can be seen both as a jumping-off point for some later anthropological discussions of cultural change (e.g., dual inheritance theory), and as addressing issues of importance which these later discussions neglect:

> The concept of *cultural core* [is] the constellation of features which are most closely related to subsistence activities and economic arrangements. The core includes such social, political, and religious patterns as are empirically determined to be closely connected with these arrangements. Innumerable other features may have great potential variability because they are less strongly tied to the core. **These latter, or secondary features**, are determined to a greater extent by purely cultural-historical factors—by random innovations or by diffusion—and they give the appearance of outward distinctiveness to cultures with similar cores. **Cultural ecology pays primary attention to those features which empirical analysis shows to be most closely involved in the utilization of environment in culturally prescribed ways.** (1955, 37; boldface added)

That is, natural selection on "cultural variants" seems most applicable to what Steward calls "secondary features" of culture—those less strongly tied to the core, and free to vary "by random innovation or diffusion" (or, perhaps, by natural selection). Features of the culture core, by contrast, are basic to adaptive stability and are not similarly free. Today most anthropologists no longer accept Steward's account of culture cores. However, a satisfactory account of the origin and stabilization of the kinds of cultural features that Steward *attributed* to culture cores remains elusive. In particular, "Darwinian" theories of cultural evolution that emphasize natural selection on cultural variants (e.g., Boyd & Richerson, 1985; Richerson and Boyd 2005; Durham 1991) remain unable to account for those key features that Steward saw as indispensable for the culturally mediated adaptation of human organisms to their environments.

Steward's attempt to recast the study of cultural evolution by turning to ecology (rather than, for example, to population biology) directly shaped the next influential anthropological approach to (human) organism-environment interactions: the "new ecological anthropology" of Andrew P. Vayda and Roy Rappaport. Emilio Moran writes that Vayda and Rappaport

> found the concept of the culture core, and the cultural ecological approach, to give undue weight to culture as the primary unit of analysis, and found the presumption that organization for subsistence had causal priority to other aspects of human society and culture to be both untested and premature (Geertz 1963). (Moran 1990, 10)

Moving from *cultural ecology* to *ecological anthropology* thus involved increasing the emphasis on biology relative to culture; as Conrad Kottak explains, "the analytic unit

shifted from 'culture' to the ecological population, which was seen as using culture as a means (the primary means) of adaptation to environments" ([1999] 2006, 40). In addition, despite Steward's misgivings about "functional or sociological formulations," his successors embraced an intensified functional ecological analysis. Rappaport defined the ecological population as "an aggregate of organisms having a common set of distinctive means by which they maintain a common set of material relations with in the ecosystem in which they participate" (Rappaport 1971, 238; cited in Kottak [1999] 2006, 41). Their theoretical inspiration was cybernetics: systems theory and the role of negative feedback. "Cultural practices were seen as optimizing human adaptation and maintaining undegraded ecosystems" (Kottak [1999] 2006, 40). In this model, two analytic units were basic: (1) the *ecological population,* which might in some cases be said to correspond to a locally named group (such as the Tsembaga Maring of New Guinea) and (2) the *ecosystem*, a set of systemic environmental relationships that regulate themselves by means of negative feedback. Rappaport would later be criticized for his easy identification of ecological populations with locally named groups, for he could offer no explicit criteria distinguishing "ecological populations" from what other anthropologists called "cultures."

In *Pigs for the Ancestors* ([1968] 1984, 4), Rappaport claimed that the Tsembaga *kaiko*, or ritual pig sacrifice, regulates the frequency of warfare among neighboring tribes because it

> operates as a regulating mechanism in a system or set of interlocking
> systems, in which such variables as the area of available land, necessary
> lengths of fallow periods, size and composition of both human and pig

populations, trophic requirements of pigs and people, energy expended in various activities, and the frequency of misfortunes are included.

Rappaport also insisted that it was important in ecological studies to distinguish between two different models of the environment: the *cognized model*, "the model of the environment conceived by the people who act in it" ([1968] 1984, 238) and the *operational model*, "which the anthropologist constructs through observation and measurement of empirical entities, events, and material relationships" ([1968] 1984, 237). Rappaport maintained that even if these two models overlapped, they were not identical and ought not to be confused.

By the 1980s, however, other anthropologists working in communities that had experienced European or American colonization had begun to adopt views influenced by dependency theory and world systems theory. They argued persuasively that key factors responsible for shaping contemporary ecological practices in the so-called "tribal" societies anthropologists were studying had actually originated *outside those societies themselves*, in colonial metropoles or in the core of the capitalist world system (Frank 1967; Wallerstein 1974; Wolf 1982). Acknowledging the impact of Western imperialism rendered deeply problematic the assumption that "tribal" ecosystems (or "cultures") were timeless, separate, self-contained, self-regulating entities. Accordingly, Rappaport was also criticized for uncritically accepting a "positivist" model of science that ignored history; this realization pushed many ecological anthropologists toward a political economic framework of analysis (Biersack 2006, 7; Goodman and Leatherman 1998).

Still other anthropologists, however, were being attracted by new analytic frameworks coming from biology. In 1963, biologist Niko Tinbergen had published a paper

in which he argued that asking "why" any form of animal behavior occurs actually masks

four separate questions about (1) the proximate (or immediate) *causal* explanation of the

animal's motivation; (2) the *ontogenetic* explanation of the behavior's development across

the animal's life span; (3) the *phylogenetic* explanation, tracing the evolutionary history of

the species-specific biological systems involved in the particular behavior; and (4) the

functional (or ultimate) *adaptive* explanation, showing how performing the behavior

influences the individual organism's ability to survive and reproduce (Tinbergen 1963).

Tinbergen welcomed the extension of ethological methods to humans (Tinbergen 1963,

430). Biological anthropologist Agustín Fuentes points out, however, that keeping all the

"why" questions separate in the study of humans is very difficult, partly owing to a "bias

toward the value of the ultimate, or functional, answer … which is seen as the most

important 'level' of analysis in terms of evolutionary understanding (the quest to find

human adaptations)" (2009, 29). These difficulties notwithstanding,

> the primacy of interest in Tinbergen's ultimate question combined with a
>
> series of mathematical models and perspectives on the role of kin and
>
> altruism that arose in the 1960s and 1970s laid the foundation for the most
>
> pervasive and influential contribution to the study of the evolution of human
>
> behavior since the early 1900s: Wilsonian Sociobiology." (Fuentes 2009, 29).

In my experience, E. O. Wilson's *Sociobiology: The New Synthesis* (1975) landed like a

bombshell in four-field anthropology departments across North America. For many

anthropologists—particularly, although not exclusively, biological anthropologists—it

seemed to be full of bright promise. But many others—especially cultural and linguistic

anthropologists (but also some biological anthropologists and archaeologists) were dismayed or angered as Wilson arrogantly proclaimed,

> Sociobiology is defined here as the systematic study of the biological basis of all social behavior. For the present it focuses on animal societies . . . . But the discipline is also concentered with the social behavior of early man and the adaptive features of organization in the more primitive contemporary human societies. . . . It may not be too much to say that sociology and the other social sciences, as well as the humanities, are the last branches of biology waiting to be included in the Modern Synthesis (1975, 4).[1]

As Fuentes explains, "[u]biquitous in this 'new synthesis' perspective was the primacy of ultimate explanations, a reliance on relatively linear mathematical models to model natural selection, and reduced concern with the physiological and genetic details of the mechanisms for behavioral adaptations" (2009, 30). Approaching interactions between humans and their environments with such a toolkit, however, could only appear perverse to cultural anthropologists like Marshall Sahlins (1976a), whose understanding of human-environment relations rested on human mobilization of complex, intricately interwoven

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[1] Although disrespect has been expressed on both sides of the divide, the kind of contempt often expressed by sociobiologists and evolutionary psychologists toward their critics has been particularly striking in my experience. Anthropologists and others committed to evolution, but critical of sociobiology, have risked being labeled "anti-science," "anti-evolution," or even "creationist." As a result, many of us have had to adopt the position of "anti-antievolutionists," who resist the critics of evolution, but who are unable to wholeheartedly affirm the hegemonic version of evolutionary theory (see Schultz 2009). The new developments and possibilities discussed in Parts 3 and 4 below may help change this state of affairs.

sets of cultural meanings and practices, resources deeply rooted in history and politics

rather than in the genes.[2] Critiques of sociobiology by anthropologists did not abate when

sociobiology gave birth to evolutionary psychology, human behavioral ecology, and other

variant perspectives (Marks 2009). For anthropologists who take culture seriously,

however, Boyd and Richerson's *Culture and the Evolutionary Process* (1985) counts as an

indispensable intervention in the debate. Using mathematical models to challenge the

mathematical modelers, dual-inheritance theory defended culture in an idiom that

sociobiologists found much harder to ignore. Of course, as noted above, anthropologists

may still object to dual-inheritance theory on other grounds.

Roy Rappaport did not respond to his critics by turning to sociobiology. On the

contrary, the 1984 edition of *Pigs for the Ancestors* contains a 180-page long epilogue in

which he addresses a range of complaints, and, in some cases, abandons positions he had

formerly defended. Until his death in 1997, Rappaport continued to reject any sharp

dichotomization between "nature" and "culture," arguing instead that the human condition

involves living "in terms of meanings in a physical world devoid of intrinsic meaning, but

subject to causal law" (cited in Biersack 2006, 7); and his later work highlighted the ways

in which political economic processes often promoted the "disordering of adaptive

structures" of local populations.

Put another way, "Rappaport's intellectual trajectory drew him slowly, tacitly

toward political ecology" (Biersack 2006, 8), an approach that attends to the ways

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[2] Sahlins began his career as a cultural evolutionary theorist (e.g. Sahlins and Service 1960). but his

views about cultural evolution changed following his experiences in France in the late 1960s (see Sahlins

1976b).

human/environment relations are shaped by political and economic processes. As Conrad Kottak explains, "a successor to ecological anthropology is the 'new' ecological, or environmental, anthropology, which blends theory with political awareness and policy concerns" (Kottak [1999] 2006, 40). For Aletta Biersack, the appeal of political ecology and environmental anthropology lies in the way that Marxist analysis, refracted through dependency theory and world systems theory, opens up the possibility of focusing "on human-nature relations in other than adaptationist and reductionist terms," because power is seen as "sociohistorical and structural" (2006, 8). She continues: "The implication for ecology is that the local is subordinated to a global system of power relations and must be understood entirely with respect to that subjection, in terms of what is commonly referred to as capitalist penetration and its effect" (2006, 9).

By the late 1980s, geographers and anthropologists doing political ecology were paying attention to linkages between global and local processes, an approach, Biersack says, that "continues to be productive today" (2006, 12).

In this connection, it is worth considering the legacy of Andrew ("Pete") Vayda, Rappaport's co-creator of the "new ecological anthropology," because his career over the past fifty years illustrates a willingness to grapple with many of the factors that are central to the post-positivist research I review below. One striking feature of Vayda's work has been his ongoing critique of theoretical accounts of the cultural mediation of human relations to their environments, not excluding his own previous views:

> In the 1960s when "cultural ecology" was in vogue, he argued for a "human ecology" instead, and was a leader in the development of systems approaches to human-environment relations. However, in the early 1970s he joined his

students in criticizing the teleology and other excesses of systems-based

human ecology, arguing instead for an agent-based approach. In recent years,

he has taken on widely-held assumptions about the nature—and culture—of

explanation in human-environment research, in the course of which he has

developed an analytical methodology that is informed by the pragmatic view

of scholars like Charles Sanders Peirce, David Lewis, Geoffrey Hawthorn, T.

Chamberlin, H. L. A. Hart, and Tony Honoré. . . . More generally . . . Vayda has

been highly critical of holism, essentialism, systems thinking, naïve

functionalism, and speculative adaptationism in anthropology and human

ecology. . . . He has also pointed to the dangers of a priori assumptions and

ready-made theories such as those of some cognitive anthropologists and

political and spiritual ecologists. (Walters and McCay 2008, 1-2)

A second striking feature of Vayda's scholarship is the extent to which his theorizing

has been powerfully informed by his experiences in applied anthropological research,

primarily in the forests of Indonesia and New Guinea. Introducing a recent collection of his

own essays, Vayda (2009, ix) observes that

an original stimulus for some of the essays was my desire to get at the causes

of particular phenomena, like intergroup fighting in the mountains of New

Guinea and extensive fires in the tropical moist forests of Indonesia. For

other essays, the original stimulus was more my being dissatisfied—on

logical, empirical, or pragmatic grounds—with research methods widely

used or kinds of explanations commonly made in such fields or subfields as

political ecology, Darwinian human behavioral ecology, and local knowledge

studies. Whatever the stimulus, going beyond my criticisms of the work of

others and achieving better explanations and identifying ways of achieving

them were among the positive goals I set myself.

Illustrative of this restless field-based critique of theoretical accounts of human-

environment connections is an article Vayda co-wrote with Bradley Walters in 1999,

entitled "Against Political Ecology." Vayda and Walters were, in fact, *not* urging that a

consideration of power relations be eliminated from ecological studies; rather, they were

challenging ecological analyses that *privilege the political*, emphasizing instead the

importance of *a range of heterogeneous causal factors*, none of which may be excluded *a*

priori (Vayda 2009, Chapter 6). Vayda and Walters' critique of narrowly political accounts

of causation in ecological studies in anthropology bears a strong family resemblance to

Bruno Latour's critique of narrowly "social constructionist" accounts of causation in

science studies (e.g., Latour and Woolgar 1986, Postscript). Vayda's "evenemental or event

ecology" (Vayda 2009, 13-34)—elsewhere called "progressive contextualization" (e.g.,

McCay 2008, 5)—bears an equally strong family resemblance both to the "constructivism"

of Ludwik Fleck (Smith 2006, Chapter 3) and to actor network theory (e.g., Latour 2005;

both Fleck 1979 and Latour 1988 appear in Vayda's 2009 bibliography).

These features of Vayda's legacy are illustrated in Paige West's ethnography

Conservation is our Government Now: The Politics of Ecology in Papua New Guinea (2006).

West follows the fortunes of Gimi people and their neighbors in the eastern highlands of

New Guinea between 1994 and 1999, as they are drawn into a "conservation-as-

development" project funded by outsiders and designed and implemented by

nongovernmental organizations (NGOs) staffed by local and international conservation

experts. "It was promised that if Gimi and Pawaia gave their lands for inclusion in the [Crater Mountain] Wildlife Management Area, they would derive cash benefits, access to economic markets for the forest products tied to local biological diversity, and "development"; in other words, "conservation was to be the development" (West 2006, 5). By 1994, most Gimi involved in the project had already altered a number of earlier connections to their forests after converting to Seventh Day Adventism, which obliged them to alter their hunting practices give up pork. But their sense of identity was fluid, and they did not equate these changes with a loss of "traditional" Gimi culture. On the contrary, they expressed to West "the feeling that they had, and have, a choice about which 'traditional' practices they wish to continue and which they wish to abolish" (2006, 66), and by the mid-1990s, many of them wanted "development." However, the "development" they expected to receive in exchange for their cooperation with "conservation" were substantive goods and services (medicine, technology, education for their children), not cash and access to capitalist markets; in the event, neither was forthcoming to the degree anticipated, nor distributed evenly among all members of the community. When the project ended, both Gimi and the conservation practitioners were frustrated and dissatisfied.

In the first chapter, West recounts an event that she witnessed in 1999, a knife fight between two Gimi men, Kelego and Lasini (2006, 15ff). Six pages from the end of the final chapter, West returns to the fight, and compares her account of this fight to the account offered by Napoleon Chagnon and Timothy Asch in their classic ethnographic film, *The Ax Fight* (Chagnon and Asch 1975). West draws both on Vayda's critique of Darwinian ecological anthropology and his discussion of progressive contextualization to differentiate her approach from that of Chagnon:

While Chagnon's goal with his reading of the ax fight is a positivist

explanation, he is looking for answers about human nature that can be

generalized from the Yanomamo to all people. I spent the past seven years

looking for explanations for one fight between Kelego and Lasini and trying

to trace out the causal chains that led up to it and the layers of meaning that

encompass it (2006, 230.)

West concludes that "The fight was about imbalances, both perceived and real, that

have come into being because of the conservation-as-development project (2006, 231). But

tracing the causal chains meant that West had to "try to disentangle the connections

between New Guinea and New York, conservation and development, and birds of paradise

and commodities" (2006, 4), efforts recounted in the body of her ethnography. West

describes historical processes through which Gimi people entered into relations with a

variety of outsiders, including colonial administrators, missionaries, linguists,

ethnographers, environmentalists, and others. Over time, these relationships transformed

both Gimi identity and the cultural practices Gimi people used to mediate their relations to

living and nonliving features of their environments. But those environments themselves

were expanded and restructured as they were connected to transnational institutions and

resources situated within the global capitalist market.

Exposing these entangled processes involved West in both archival research and

fieldwork, among both Gimi people and conservation practitioners, both in New Guinea and

New York. Multisited research complicated West's understanding of the role of

anthropology in analyzing how human groups use culture to mediate connections to their

environments. The story she tells "is not a story of 'good guys' and 'bad guys' or even 'the

Gimi' and 'the conservationists.' It is a story about the social lives of people associated with a large bit of the forest in Papua New Guinea" (2006, xv). Her final text, she insists, is neither a "translation or legibility-making service" for conservation activists nor "a devastating critique of conservation as a way of knowing and producing knowledge. . . . Rather, my goal is to provide an ethnography of the project and perhaps to persuade conservation practitioners, activists, scientists, and others to question the assumptions about nature, culture, and development that underlie many of today's biodiversity conservation efforts" (2006, xviii).

2 New Directions in Anthropological Studies of Organism-Environment Relations

A rich conversation is in the making between anthropologists and biological theorists who write about the cultural mediation of human connections to their environments, but the exchange could be much more balanced. In this section, I begin by reviewing the way some forms of theoretical biology have begun to shape recent anthropological thinking. But I end by presenting additional anthropological work that, like Paige West's ethnography, merits serious consideration in discussions of the cultural mediation of human-environment connections in theoretical biology, but that so far has not been considered.

Let us start by looking at the recent work of biological anthropologist Agustín Fuentes (2009). He analyzed five contemporary theoretical approaches to the evolution of human behavior that have influenced biological anthropology: Neo-Darwinian (ND) Sociobiology / Human Sociobiology, Human Behavioral Ecology (HBE), Evolutionary Psychology (EP), Gene-Culture Coevolution / Dual Inheritance Theory (DIT), and Memetics.

All five, he reminds us, take "Wilson's sociobiology, Hamilton and Trivers' kin selection and reciprocal altruism, and the Dawkinsian genic selfishness as baseline assumptions" (2009, 37). All five claim Darwin as their inspiration, focus on natural selection as the architect of behavior, and tend to de-emphasize other processes of evolution recognized in the modern synthesis, such as gene flow and genetic drift. HBE and EP also emphasize the importance of Ernst Mayr's's distinction between ultimate and proximate levels of explanation (2009, 62). But Fuentes is troubled by what all five leave out (Fuentes 2009, 62-63):

> Missing from HBE, EP, DIT, and Memetics is much of the evolutionary anthropological approach pioneered by Sherwood Washburn. In the 50 years since Washburn proposed his "New Physical Anthropology," there has been an explosion in the paleoanthropological data base, resulting in a series of important changes and enhancements of the scenarios for human physical (and social) evolution. Unfortunately, ND-Sociobiology is the only one of these perspectives to regularly exploit both the fossil and archeological records and primate studies as comparative tools. Of the other four, HBE does occasionally incorporate fossil/archeological/primatological datasets (Hawkes et al. 2003) and EP uses assumed Pleistocene selection pressures as its baseline, but neither EP, CIT, or Memetics regularly use fossil or cross-species comparisons in their construction of scenarios and hypotheses for the evolution of human behavior.

To fill in the gaps, Fuentes (2009, 172) incorporates recent work that focuses attention on ontogeny: Jablonka and Lamb's (2005) arguments for "evolution in four dimensions", West-Eberhard's (2003) arguments linking developmental plasticity to

evolution, Oyama's (2000) developmental systems approach (see also Oyama et al. 2001), and the niche construction perspective developed by Odling-Smee, Laland, and Feldman (2003). In particular, Fuentes (2009, 172-75) is persuaded that niche construction is an evolutionary force that can be tested against the human evolutionary record, and he and two colleagues recently performed such a test, proposing a new explanation for an old puzzle. Between 2.5 and 1 million years ago, the fossil record shows that the genus *Homo* and the genus *Paranthropus* coexisted in eastern and southern Africa, but by 1 million years ago, *Paranthropus* was extinct, and *Homo* had expanded. Most explanations of this transition attribute it to the superior foraging efficiency of *Homo,* based on increased brain size, tool use, and meat consumption, the sharing of "cultural" information, and (recently) also to niche construction. However, Fuentes and colleagues "propose a model wherein a focus on the role of predation and differential ability to share information and cooperatively modify functional facets of the environment provide an important component of the explanation of the success of the genus *Homo* relative to . . . *Paranthropus*" (Fuentes et al. 2010, 436). Using evidence suggesting that both *Paranthropus* and *Homo* were likely vulnerable to the same predators, their model shows how niche construction could have made *Homo* less desirable as prey, shifting predation pressure onto *Paranthropus,* while at the same time providing positive feedback to protective niche-constructing behaviors in *Homo.* Overall, Fuentes recommends abandoning or deemphasizing optimality models, single-trait models, simple proxy measures of fitness, and the focus on DNA; retaining the focus on natural selection (together with niche construction), on the role of symbolic communication and culture, and on past and present environments; and expanding attention to plasticity, multilevel

selection on multiple inheritance systems, and the role of behavior as an agent of evolutionary change in humans (2009, 180-186). The outcome, he believes, will be a biocultural approach to the study of human behavior appropriate for the twenty-first century.

Attention to ontogeny has also been central to the work of British anthropologist Tim Ingold. A Cambridge-trained social anthropologist who carried out fieldwork among Sami reindeer herders in Finland, Ingold grew dissatisfied with theoretical proposals treating human life "as merely consequential, the derivative and fragmentary output of patterns, codes, structures or systems variously defined as genetic or cultural, natural or social" (2011, 3). His own work, therefore, "has been driven by an ambition to reverse this emphasis: to replace the end-directed or teleonomic conception of the life-process with a recognition of life's capacity continually to overtake the destinations that are thrown up in its course" (2011, 4). The result is a unique perspective on the relations between humans and their environments that innovatively combines insights from James Gibson, Susan Oyama,[3] Maurice Merleau-Ponty, Martin Heidegger, Alfred North Whitehead, Henri Bergson, Gilles Deleuze, and Félix Guattari (Ingold 1990, 2007, 2011).

Problematic accounts of the labor process led Ingold to explore the phenomenology of human productive accomplishment, and to conclude that production was not "about transforming the material world, but rather about participating in the world's transformation of itself" (2011, 8). Accordingly, he has taken up the challenge of

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[3] Ingold is the only anthropologist who contributed an essay to the DST compendium *Cycles of Contingency* (Oyama et al. 2001).

rehabilitating cosmologies denominated "animist" by Western thinkers: "once we recognize the primacy of movement in the animic cosmos … we are not required to believe that the wind is a being that blows. . . . Rather the wind *is* blowing, and the thunder *is* clapping, just as organisms and persons *are* living in the ways peculiar to each" (2011, 73). Ingold has also written insightfully about the evolution of the human foot, and criticizes "the division of labor between hands and feet" that informs most discussions of the evolution of human bipedalism since Darwin (2011, chap. 3).

Ingold's ongoing reflections on the relations among anthropology, art, and architecture have attracted wide attention, inside and outside of anthropology. Recently, he has debated anthropologists and archaeologists who study material culture. All of them deplore nature/culture dualism, agreeing that matter has been unjustly neglected by positivist science. Ingold, however, rejects their attempts to generalize about "materiality," insisting that

> so long as our focus in on the *materiality of objects*, it is quite impossible to follow the multiple trails of growth and transformation that converge, for instance, in the stuccoed façade of a building or the page of a manuscript. These trails are merely swept under the carpet of a generalized substrate upon which the forms of all things are said to be imposed or inscribed. I propose that we lift the carpet, to reveal beneath its surface a tangled web of meandrine complexity. (2011, 26)

Rehabilitating the status of matter has also been central for post-processual archaeologists[4] wishing to incorporate into their accounts of the human past the cultural meanings of material artifacts for their makers and users. Because they regularly deal with things that neither speak nor carry written linguistic representations, these archaeologists need methods for studying non-linguistic meaning-making. Post-processual archaeologist Robert Preucel writes that

> material culture, like language, often plays a central role in mediating social
> identities and relations. However … material culture does not participate in
> the same kind of structured system as language. Objects are not words and
> there is nothing in material culture comparable to syntax or grammar in
> linguistics. But because material culture has form and substance, it has the
> power to fix meanings in ways that are not possible in language. (2010, 84).

In the 1970s and 1980s, post-processual archaeologists like Preucel who were disappointed by attempts to adapt Saussurean *sémiologie* for the study of material culture were inspired by the work of linguistic anthropologists like Michael Silverstein, who were

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[4] So-called "processual archaeology" emerged in the 1960s and is closely associated with the work of Lewis Binford (1962). It encompasses a variety of different approaches, but "all share a common processual orientation grounded in cultural evolutionary theory and a systemic view of culture" based on the structuralism of Claude Lévi-Strauss (Preucel 2010, 94). "Post-processual archaeology" encompasses a variety of different approaches sharing "a common dissatisfaction with the scientistic approach of much of processual archaeology, particularly its focus on positivism and general laws of human behavior. In its place they adopt hermeneutic methods and emphasize the social salience of ideology and power," commenting, "as an empirical social science which privileges material culture, archaeology retains a strong modernist core and resists full colonization by poststructuralism and postmodernism" (Preucel 2010, 123).

questioning Saussure's distinction between *langue* and *parole*. Research in linguistic

anthropology showed that linguistic meaning in contexts of use depended heavily on

speech, or *parole,* rather than on idealized symbolic meanings supposedly encoded in

langue. But this meant that linguistic anthropologists needed a method of analysis that

would allow them to study dimensions of meaning communication that were not purely

symbolic. They found what they were looking for in the semiotics of Charles Sanders

Peirce; some cultural anthropologists and post-processual archaeologists soon joined them.

What resulted was the emergence of a pragmatic anthropology critical of the limitations of

symbolic, structural and cognitive anthropology, but also resistant to the poststructural

claims about the radical ambiguity of meaning (Preucel 2010, Chapter 4).

Scholars in many fields are familiar with Peirce's tripartite division of signs into

icons, indices, and symbols. Michael Silverstein (1976) proposed that each of these sign

functions constitutes a separate mode of meaningfulness, and argued that indices are

indispensable for the study of language in use. Silverstein and others later demonstrated a

variety of ways in which indexicality is mobilized ideologically by speakers to modify

linguistic structures in contexts of use (Silverstein 1985). When Preucel and other

archaeologists reviewed Peirce's writings about signs, they discovered that by the time of

his death in 1906, Peirce had elaborated a typology of at least 66 signs that linked icons,

indices, and symbols both to their interpretants and to other signs (Preucel 2010. 56-60).

In a historical archaeology project at Brook Farm, Massachusetts, Preucel uses Peirce's

typology to explicate a range of different kinds of meanings mediated for their original

Transcendentalist residents by the buildings they used and built. He also shows how these

meaningful architectural mediations were undermined when later residents, committed to

Fourierism, and with different class origins, promoted a different kind of communal architecture at odds with Transcendental cultural practices. Preucel concludes that the varied buildings used and constructed by Transcendentalists were "a material expression of the Transcendentalist celebration of the individual in society" that also exercised "house agency" as they "actively engendered certain habits of thought and social practices at the core of Transcendentalism" (2010, 209).

Semiotic archaeology is not the only variety of post-processual archaeology that investigates relations between human organisms and their environments, but it is a provocative "dissident" version. Another dissident version is the "social archaeology" tradition associated with Ian Hodder. Over the past thirty years Hodder's career has taken him from processual to post-processual archaeology[5] and he is surely the most influential post-processual archaeologist at work today. Hodder is probably best known among anthropologists who are not archaeologists for his work in ethnoarchaeology: in the 1970s, he carried out ethnographic fieldwork in several East African societies to test the correlation between distributions of material artifacts and the social identities of their makers and users. Hodder's conclusion that such connections were unreliable set him apart from Lewis Binford, who also carried out ethnoarchaological research, but drew the opposite conclusion.

Over the years, Hodder has published a series of texts in which he has relentlessly explored the consequences for archaeology that follow from challenges to its former identity as a disinterested scientific enterprise. In recent years, these challenges have come

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[5] A term he coined (Preucel 2010, 126).

not only from indigenous communities who connect archaeology with colonial domination and expropriation, and from government laws mandating repatriation of human remains and artifacts, but also from epistemological challenges by science studies scholars like Bruno Latour. While acknowledging the close ties that still bind many processual archaeologists to the tenets of positivist philosophy of science, Hodder has chosen to embrace the challenges of reflexive scrutiny and the critique of positivist science. He has addressed in detail the issues surrounding hermeneutical analysis in science, arguing that even though the whole of an archaeological site is understood in relation to its parts, "this circle of part-whole relationships is not vicious. . . .Rather, the objects of study can cause us to change our ideas about the whole or about the relationship between the parts. This circle can best be described as a spiral" (1999, 33). He has incorporated ideas from science studies to open up archaeological concepts like the *chaîne opératoire*, which specifies the sequence of practices that produces particular material artifacts (1999, 76). Hodder honors the skills archaeologists have developed to trace long-term and large-scale cultural processes, but he insists they must also develop narrative techniques for interpreting, whenever possible, the human activities they are able to reconstruct at a human scale: "both are needed in an archaeology which accepts diversity, uncertainty and relationality in human behavior" (1999, 147).

Because he acknowledges, but goes beyond, the resolutely local, phenomenological focus of Ingold, Hodder cannot avoid coming to terms with heterogeneous global flows of wealth, commodities, people, images and ideologies that have been unleashed since the end of the Cold War. Indeed he must do so, for his ongoing archaeological project at Çatalhöyök, in Turkey, is sustained by these flows: it is financed by private capital, employs local and

international workers, requires the ongoing support of local and national governments, attracts tourists from Turkey and elsewhere, and for some years has had a presence on the internet (http://www.catalhoyuk.com/). Hodder has paid close attention to the work of Arjun Appadurai, an anthropologist whose book *Modernity at Large* (1996) has profoundly shaped cultural anthropologists' understanding of these global flows (Appadurai et al. 2001). Like Ingold, Appadurai turns to Deleuze and Guattari's *A Thousand Plateaus* (1988) for language capable of articulating "the special problems that beset the production of locality in a world that has become deterritorialized" (1996, 188). But the global lines of flight Appadurai describes generate heterogeneous, hybrid forms of movement spun out of rootlessness, alienation, and transgenerational instability of knowledge, with both points of departure and points of arrival in cultural flux (1996, 29, 43, 44).

Nevertheless, Appadurai also argues that an upside to globalization can be perceived when new global technologies and connections are mobilized to solve old problems (1996, 43). This phenomenon may be glimpsed at the Çatalhöyök Research Project, where Hodder and his collaborators have worked for some years to develop and institutionalize reflexive archaeological practices that now are mediated by a sophisticated computer database providing access, in different ways, to field staff, laboratory specialists, and internet viewers. Perhaps most innovative of all is the project's employment of cultural anthropologists specializing in science studies, who carry out participant observation on the entire research process and feed back their insights into the ongoing project (Hodder 1999, chap. 10).

I conclude with one final example illustrating my conviction that archaeology may be the most lively source of innovative thinking in contemporary anthropology. In some

ways, Nicole Boivin's recent volume *Material Cultures, Material Minds: The Impact of Things on Human Thought, Society, and Evolution* (2008) brings my observations in this section full circle. Trained at Cambridge University in the Hodderian social archaeology tradition, Boivin insists that the physicality of matter gives things agency that is independent of human organisms. While she was studying domestic space in rural Rajasthan, India, she noticed that

> much of the way that houses assumed a social and symbolic role relied on the use of soil to create them. . . . Mud houses are infinitely malleable, and are constantly plastered and replastered in ways that enable them to acquire a new appearance, texture and feel. . . . I thus began to think about the first mud houses and how they may accordingly have played a role in generating new symbolic and social possibilities within prehistoric society (2008, 133-34).

Boivin eventually concluded that "soil was an active agent in the process of Neolithicization in the eastern Mediterranean, among many other active agents, both human and non-human" (2008, 138), and she found Tim Ingold's arguments helpful for imagining how domesticated species and artifacts might emerge "as a result of the 'mutual involvement of people and materials in an environment' in which outcomes cannot always be anticipated" (2008, 156). She acknowledged that "locating agency is a complex exercise that probably demands a new way of thinking about it, as well as about humans and things" (2008, 168), and she found such a new way of thinking in actor network theory (2008, 176). Combining insights from Tim Ingold and Bruno Latour, she then determined that "the realms of technology and environment become difficult to differentiate" (2008, 178). This

31

realization led to an extended exploration of niche construction, development, and cognitive plasticity, in which Odling-Smee, et al. (2003) and Oyama et al. (2001) are prominent sources (2008, 197, 220). Boivin closes her discussion by urging cognitive scientists to pay attention to archaeologists and anthropologists, and for niche construction theorists to talk to social anthropologists: "Material culture, which by very definition straddles [the social sciences and humanities,] demands an integrated approach that brings these very different models together" (2008, 229). In this way, citing many of the same sources who inspired Fuentes, Boivin likewise echoes Fuentes's call for an integrative anthropological approach that is "holistic, messy, but potentially highly profitable" (Fuentes 2009, 249).

3 Integrating Post-positivist Anthropology into Theoretical Biology: A Proposal

In this section, I propose a theoretically informed way of articulating neglected work in post-positivist anthropology into analyses by theoretical biologists of the cultural mediation of human-environment connections. Paige West's work in Papua New Guinea, discussed in Part 2, is a good place to begin. West writes that she has begun to see her role and the purpose of her work as related to a "new ethnography of development" that "takes seriously the governmentality of projects—the fact that social lives, environments, and subjects come to make and be made by the productive power of the structures created by projects (Foucault 1977)—and the social interactions during all sorts of projects (be they conservation, development, or resource extraction) which create new communities (Golub 200[6])." Because projects like this are remaking people's environments all over the world, ignoring them in accounts that attempt to describe the cultural mediation of a human

population to its environment cannot continue. The highland village of Maimufa where West carried out much of her fieldwork was a hybrid community, consisting not only of Gimi people, but also of numerous non-Gimi from Australia, the United States, and elsewhere in Papua New Guinea, jointly engaging with Gimi people and their neighbors in the conservation-as-development project. West carried out participant-observation among the conservation scientists as well as the Gimi residents because the causal interventions of the scientists could not be ignored:

> The value of the eagle is not in and of the eagle, though its commodification might make it seem so—it is a value produced by a set of social relations of production in science and in the imagination of scientists. And what of the labor and value that went into the eagle that is forgotten as it becomes a commodity? That labor is the labor of scientific practice (Latour 1987:7), and the nature of the bird is its relation to all the processes of the forest that it influences and that influence it. (2006, 212)

Or, to put it another way, the eagle is an actor network. Latour (2011, 797-798) has recently defined an actor network as follows:

> In its simplest but also in its deepest sense, the notion of network is of use whenever action is to be redistributed. . . . Take any object: At first, it looks contained within itself with well-delineated edges and limits; then something happens, a strike, an accident, a catastrophe, and suddenly you discover swarms of entities that seem to have been there all along but were not visible before and that appear in retrospect necessary for its sustenance. You thought the Columbia shuttle was an object ready to fly in the sky, and then

33

suddenly, after the dramatic 2003 explosion, you realize that it needed NASA

and its complex organizational body to fly safely in the sky . . . . The action of

flying a technical object has been redistributed throughout a highly

composite network where bureaucratic routines are just as important as

equations and material resistance. . . . What was invisible becomes visible,

what had seemed self-contained is now widely redistributed. . . . the search

for the production of object and of objectivity is totally transformed now that

they are portrayed simultaneously in the world and inside their networks of

production.

Let us now return to Nicole Boivin, who pulled together insights from Tim Ingold,

Bruno Latour, developmental systems theory, and niche construction. As an archaeologist,

the affinities between actor-network thinking and niche construction helped her

understand that the physicality of matter gives things agency independent of human

organisms. DST theorist Susan Oyama is also sensitive to actor-network thinking in her

own thinking about developmental systems, noting that the "swarms of entities" to which

Latour refers above (called *actants* in actor network theory) bear a family resemblance to

the "interactants" Oyama identifies as components of developmental systems (2000, 123).

Actor-network theory appeals to her for an additional reason as well:

Latour (1987:71-72) has described the scientist as the spokesperson for that

which is studied. One of the many reasons I have found it worthwhile to think

and write in developmental systems terms is that it allows me to speak for

the background—the mute, manipulated materials, the featureless surround.

Sometimes the peripheral is the political. (2000, 126)

Developmental systems theory and niche construction seem to require joint consideration (Oyama et al. 2001), because niche construction draws attention to the ways in which organisms make themselves, in part, by making their own environments. With Boivin, I agree that niche construction and actor-network theory also require joint consideration, and suggest that this may be more easily facilitated once it is recognized that both views rely on the same mechanism. According to John Odling-Smee and his colleagues, a major motivation for their development of a theory of niche construction was the desire to link ecological studies that focused on abiotic processes with ecological studies that focused on biotic processes. Bruno Latour has written: "As soon as you start to have doubts about the ability of social ties to durably expand, a plausible role for objects might be on offer" (2005, 75). Odling-Smee and his colleagues apparently came to a similar conclusion, which led them to propose the concept of an "artifact,"

> a third kind of object in ecosystems that is neither biotic nor conventionally abiotic, but intermediate between the two. Artifacts are not alive, yet they can only be built by living organisms. Also, once built, they are likely to respond to niche-constructing organisms in a different way from either biota or raw abiota. (Odling-Smee et al. 2003, 190)

The concept of artifact allowed them to propose a second concept, the environmentally mediated genotypic association (EMGA), in which constructed artifacts mediate between one population of organisms and another by modifying the selection pressures experienced by the second population:

> If, in a single population, genetic variation is expressed in a niche-
>
> constructing phenotype that affects natural selection acting on other genes in

the same population, then the population will merely codirect its own

evolution. However, if the niche construction modifies natural selection

acting on genes in a second population, then the first population will now

codirect the evolution. Conceivably, the induced change in the second

population could feed back to the first population in the form of another

modified natural selection pressure. The two populations would therefore

coevolve through niche construction. (2003, 23)

To me, an EMGA looks suspiciously like a stripped-down proto-actor-network for

two reasons: (1) because it is a heterogeneous assemblage linking together living and non-

living actants within an ecological network, and (2) because each actant contributes its

own causal influence to the network's activity—i.e., serves as a mediator—rather than

serving as an intermediary that merely transports causation without affecting it (Latour

2005, 39). Biota, abiota, and artifacts would all seem to be mediators, rather than

intermediaries, since they all have the capacity both to respond to further niche

construction and to modify natural selection pressures (Odling-Smee et al. 2003, 191).

Latour also contrasts the way that an assemblage of heterogeneous mediators can

be stabilized (or black-boxed) and turned into a unified whole that acts as one and is

capable of transporting agency without affecting it. But black-boxed intermediaries can

degenerate into networks of mediators, as, for example, when complex pieces of technology

break down. Odling-Smee et al.'s EMGAs appear to have these properties. First, "niche-

constructing organisms work in open systems," which means that they can "potentially

drive some selected components of their environments in both thermodynamic directions,

by either locally increasing or locally decreasing entropy levels." Second, "like organisms,

artifacts demonstrate negative rather than positive entropy because they are usually quite highly organized; yet, unlike organisms, they have no ability to defend their own organization nor to prevent their own dissipation. Artifacts are therefore likely to demand repetitive niche construction from organisms to maintain them" (2003, 190).

If these parallels are persuasive, actor-network theory might provide a bridge that allows work in post-positivist anthropology to be articulated with developmental systems theory and niche construction in theoretical biology—thereby allowing "history" to be incorporated into discussions of "evolution." Actor-network thinking already informs ethnographic studies in science, technology, and medicine. But it is also implicit in the ethnography of development. Paige West draws readers' attention to the "abiotic" artifacts that sustained life in her field settings. The cultural mediations she describes involved not just "humans" in the lump, but a specific heterogeneous community of humans composed of Gimi people and outsiders from Australia, the United States, and elsewhere in Papua New Guinea; and they are connected not just to "the environment" in the lump, but to specific mountains and forests, to birds of paradise and trees with harpy eagle nests, to game animals, and to swiddens. These heterogeneous living actants intertwined with heterogeneous nonliving actants: the tools of the hunters and farmers, such as bush knives; imported tinned fish that replace the pork they no longer eat; dwellings for residents, five church buildings, a health post perennially out of medicine, and a school. Particularly salient are the village airstrip and planes run by the Seventh Day Adventist Church that provide the community's sole link to the outside world in the absence of roads: "The point cannot be made too strongly—everything that comes to Maimafu comes on an airplane . . . The village airstrip is the site of new things, ideas, people, money" (2006, 76). The airstrip

is also the site where important goods, like locally grown cash-crop coffee, go out: "all residents of Maimafu have to pay freight charges. . . to the missionary planes that pick the coffee up and take it to Goroka [the provincial capital]. . . . The airfreight charges paid to the mission planes fluctuate according to the price of fuel, thus tying Maimafu and other rural places that grow 'airstrip coffee' to the global political economy of oil" (2006, 106).

In her description of the knife fight, West also observed that Kelego was wearing a bath towel around his neck that had been given to him by a visiting biologist. "It is not the material nature of the towel that is most important to him," she writes. "Rather, the importance. . . is the meaning of the exchange with the biologist [showing] that he has a tie to conservation and to someone who is somewhere else" (2006, 15). And yet, as Latour reminds us, the towel as a material actant plays an indispensable role as one of those objects that enables social ties to durably expand.

Niche-construction, explicitly informed by actor-network theory, would therefore consider the role of "the social" in the production of space, but "the social" would be reconceptualized in terms of "collectives," in which humans are attached to nonhumans, living and nonliving, physical and nonphysical (Latour 2005). Acknowledging this would mean, among other things, that organisms, cultures, and environments would need to be approached as emergent hybrid products of "natureculture" (Haraway 2008, 6-7; Latour 1993, 7). For example, it would mean acknowledging the naturalcultural heritage of Gimi country itself: as West argues, "The biodiversity that exists in and around Maimafu is the by-product of human habitation and use. . . .The people of Maimafu, through the subsistence patterns that the NGO wishes to curtail, produced the landscape in which they live. So there is, therefore, no 'pristine condition' to preserve" (2006, 178).

A natural-cultural, actor-network understanding of niche construction might help

resolve the problems faced by Steward and other analysts, inside and outside of

anthropology, who have struggled to fit culture and history into discussions of human

(cultural) adaptation and (cultural) evolution. For example, there was nothing predestined

about Gimi country becoming the location of a biodiversity conservation project; it was a

serendipitous development, connected to the fact that the husband of an ethnographer

working among Gimi people in the 1970s took an interest in birds of paradise (2006, 130-

131). But that contingent event led to a conservation-as-development project that

mobilized features of the naturalcultural constructed niches of Gimi people and of outside

conservation practitioners; both were "folded into each other," leading to the emergence of

a powerful hybrid naturalcultural construct, the Crater Mountain Wildlife Management

Area, (2006, 32). The results of that process, for good and for ill, could not easily foreseen

or controlled, but may be explored and explained in part by post-positivist ethnographers

and their allies.

Cultivating "naturalcultural" thinking by elaborating niche construction with

insights from actor network theory could help biological theorists grapple with a range of

issues tied to the very basic connections that organisms, particularly human organisms,

forge with their environments. As Ian Hodder has recently observed,

> the brute matter of things has effects on us that go beyond social meaning.
>
> We cannot reduce things solely to the relational, to a semiotics of things. To
>
> do so undermines the power of things to entrap, and particularly to trap the
>
> more vulnerable whether these be the victims of the AIDS virus, the work
>
> gang bound by chains, the women bound by child rearing, the populations

bound by global agricultural systems. . . . There is much to be done in terms

of understanding the different paths we have taken as humans, caught up in

our varied ways with things. But the big picture is clear. Since a dependence

on made things became an evolutionary pathway, there has been one long

movement, initially slow, but speeding up exponentially as the strands of

human-thing entanglement lengthened and intensified. (2012, 220)

If attachments to things are part of our evolutionary pathway, expecting to escape

from them, in theory or in life, is futile. Rather, the task, as Latour tells us, "is no longer a

matter of abruptly passing from slavery to freedom by shattering idols, but of

distinguishing those attachments that save from those that kill" (2010, 61).

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