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Uncommon Ground: Holism and the Future of Anthropology

MICHAEL E. HARKIN

Parkin, David and Stanley Ulijaszek, eds. 2007. *Holistic Anthropology: Emergence and Convergence*. Methodology and History in Anthropology, 16. New York: Berghahn Books.

Segal, Daniel A. and Sylvia J. Yanagisako, eds. 2005. *Unwrapping the Sacred Bundle: Reflections on the Disciplining of Anthropology*. Durham: Duke University Press.

Sidky, H. 2003. *A Critique of Postmodern Anthropology: In Defense of Disciplinary Origins and Traditions*. Mellen Studies in Anthropology, 10. Lewiston, NY: The Edward Mellen Press.

Anthropology has long been a contested field with roots in both scientific and humanistic discourse. The "four-field" approach in American anthropology has been used by some as a means of suppressing conflict and enforcing a false sense of unity. While critiques of the four-field approach are warranted, in fact holism represents anthropology's greatest strength. Anthropology gains a competitive advantage over other disciplines in its ability to combine biological and cultural approaches. Moreover, the four-field model, while historically contingent, is well suited to the institutional and political realities of the American academy, especially the public research university.

KEYWORDS general anthropology, holism, subfield isolation

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UNCOMMON GROUND

Does any mature discipline worry more over its history and fundamental outline than anthropology? In the past decade the centennial of the American Anthropological Association and the millennium itself have spurred a recent bout of profound self-questioning (see Fisher 2007; Geertz 2002; Nader 2001; Sahlins 1999). The three books under consideration here appear at a moment when anthropology is struggling with foundational questions, primarily but not exclusively its status as a scientific or humanistic discipline. The two American books—*A Critique of Postmodern Anthropology: In Defense of Disciplinary Origins and Traditions* (Sidky 2003), and *Unwrapping the Sacred Bundle: Reflections on the Disciplining of Anthropology* (Segal and Yanagisako 2005b)—argue on opposite sides of this divide. Sidky believes, as do many in the profession, that anthropology has given up its precious birthright, which is a claim to scientific verifiability. Undone by postmodernists, the author argues, the field is deconstructing itself out of existence. The "defense of disciplinary origins" referred to is a fairly narrow defense of certain ancestors, such as Bronislaw Malinowski, Leslie White, and Julian Steward, who put forth explicitly scientific or scientistic models, while others, such as Margaret Mead and other Boasians, are less favorably viewed. The volume edited by Segal and Yanagisako, while more complex, reflecting its multiple authors, argues for more or less the reverse position: that cultural anthropology deserves to be free of its historically contingent entanglement with the more materialist subfields of archaeology and biological anthropology, and needs to create alliances with humanistic disciplines. It needs above all to avoid the siren song of science, which it claims is both illusory and fraught with ethical and political hazards. The third volume, *Holistic Anthropology: Emergence and Convergence* (Parkin and Ulijaszek 2007), coming from the UK, approaches the problem from a somewhat different angle. Lacking such a tradition of "four-field" anthropology, it seeks instead to demonstrate ways in which the culture concept as used in social anthropology can be joined with biological and archaeological perspectives to shed light on important topics of broad interest to anthropologists and the broader scientific and intellectual community.

Although anthropology is a field that periodically reinvents itself through episodes of self-critique, this period seems to be qualitatively different. In large part this is because we are in a post-paradigmatic era, when the debates are more wide-ranging and fundamental, rather than simply a series of polemics between, say, interpretivism and cultural materialism. In many ways, we are no longer debating the relative merits of two well-defined positions, or at least not only that, but are engaged in various types of meta-debates about the possibility of an epistemological, ethical, and political status of anthropological knowledge. Many factors play into this. Certainly, anthropology's

engagement with a problematic object (humanity itself) is a large part of it, although it might fairly be observed that no less a mature science than physics entertains disagreement over the very nature of the material universe itself, with little more discord than the current round of internecine battles between string and anti-string theorists. Still, in physics at least there is agreement that such an object exists. Anthropology is not so sure of the objectivity of its object, or the subjectivity of its subject. Caught on the horns of the Kantian dilemma, in which "man" is both transcendental subject and knowable object, anthropology has never been entirely clear or confident about its epistemological standing (Foucault 1973:308–309; Kant 1901). Add to that the legacy of colonialism and racism, with which anthropology was complicit in its early years—a legacy it has never entirely shaken, in part because a small minority of anthropologists have continued to give scientific support to racism, in larger measure because it still perforce piggybacks on the institutional legacy of colonialism—and we have a disciplinary version of what the French call "*la crise permanente*." Indeed, it is debatable whether anthropology constitutes a "discipline" in anything like the Foucauldian sense: as an official, self-regulating discourse (Foucault 1975:184). Certainly, there is no active "disciplining" of practitioners according to some paradigm of "normal science." As James Clifford (2005:24) points out in his contribution to the Segal and Yanagisako volume, this notion of discipline has its roots in ascetic practice, and suggests a uniformity of training and praxis in far-flung settings. By contrast, in anthropology models of training, standards of practice and intellectual affinities vary from department to department and even within departments. Although there is some broad agreement regarding anthropological ethics, the American Anthropological Association has no licensing or enforcement capacities, and effective sanctions can come only from the institutions that employ anthropologists. Institutional review boards, geared to the medical and biological sciences, are notoriously ill-informed about the issues involved in ethnography, and thus are ineffective instruments to provide ethical guidance to many anthropological researchers.

This situation became particularly critical in the wake of the publication of Patrick Tierney's (2000) *Darkness in El Dorado* and the subsequent professional responses to his allegations of unethical research among the Yanomamo of the Venezuelan Amazon. One part of Tierney's accusation rested on misuse of biomedical techniques, particularly the issues of informed consent and potential exposure to exogenous pathogens. This is a question that few sociocultural anthropologists were able to assess for themselves, it being far outside their own professional practice. Nevertheless, one of the key figures in this affair was a sociocultural anthropologist, Napoleon Chagnon, a previously controversial figure, who was accused of a range of unethical behavior. However, the most provocative of the accusations foundered on a lack of hard evidence, and the

official AAA report often reads more like an unsympathetic book review than a professional sanction:

> Chagnon began his work at a time when the sort of detailed history of "people without history" (Wolf 1982) was not a major trend in anthropology—although the kinds of points well-known from the work of Wolf and Fabian had been raised long before by Ortiz (1994 [1940], 1946). However, the systems of classification and metaphors that Chagnon uses into the 1990's fall directly into the discursive system that Fabian (1983) has called the "denial of coevalness." (AAA 2002:1:39)

That is to say, Chagnon's inheritance of a certain style of anthropological discourse, one that rests on metaphors of othering and the specific schema of sociocultural evolution, is both ethically and intellectually problematic. Anthropologists, in this view, should no longer be peddling the sort of argument that has "primitives" standing in for "our" ancestors, or comparable to apes or other primates, available for a "savage slot" and the sort of ready-made arguments (the origins of warfare, etc.) that surround it (see Trouillot 1991). As Daniel Segal and Sylvia Yanagisako argue the case, this "pre-ness" of non-Western others derives from the very legacy of anthropological holism, which in its American form (Anglo North American form, more precisely) manifests itself as the famous "four-field" approach, combining sociocultural, linguistic, biological, and archaeological anthropology (Segal and Yanagisako 2005a:9).

Others, such as H. Sidky, argue precisely the opposite: that the problem, if any, with the tetralogy is that humanistic and postmodernist approaches are overrepresented, both numerically and politically, and that anthropology can never aspire to being a science as long as we are critiquing discourse and not facts (Sidky 2003). Contrary to popular view, this does not necessarily, or even primarily, break down into a division among subfields. Rather, it is a struggle, played out largely on the terrain of sociocultural anthropology, on the role of the other three subfields in anthropology, the nature of evidence, and indeed the nature of the human subject-object itself. This division is less between two competing visions of anthropology, than between two opposed worldviews. Whether one reads *Human Ecology* or *Anthropology and Humanism* (both publications geared to sociocultural anthropologists) has predictive value for other issues, such as one's opinion on the *Darkness in El Dorado* controversy.

And yet, as in politics, we find plenty of undecideds and late deciders, minor party advocates, and others who complicate a clean and simple dualism. I would argue that this is particularly true if one looks at professional practice, rather than explicitly theoretical and methodological writing. Much of an anthropologist's professional praxis (in the United States and Canada at least) takes place among a diverse group of colleagues whom one influences and is influenced by, and whose ideas are not dismissed as quickly as might be expected. Indeed, a movement away from the high ground of

epistemology and theory and toward the everyday and the practical would be a useful exercise. Anthropology in my view is best understood as what anthropologists do, and this varies greatly, both across and within the subfields. Indeed, in a rare moment of disciplinary self-affirmation, let it be said that only by ethnographic means can the great diversity of anthropological practice be made comprehensible. Just as sociocultural anthropologists such as Paul Rabinow (1996) have taken ethnography into the laboratories of biotechnology, so we could see an application of this method to understanding the research praxis of, say, primatologists or bioarchaeologists.

ANTHROPOLOGY'S TETRALOGY

The current set of books, however, are less ethnographic than a-priorist, which makes two of them (Segal and Yanagisako 2005b and Sidky 2003) in particular frustrating to those who feel they have a stake in retaining the openness and flexibility that come with not having to make an irrevocable choice about anthropology's nature and future. Interestingly, the two American contributions to this debate are, with some notable exceptions in the Segal and Yanagisako volume, less open to the possibility of what could be called hybridity, and are marred by a tendency to be tendentious and solipsistic, while the British volume, despite the U.K.'s much-noted lack of a national four-field tradition, provides some fine examples of ways that anthropology can capture and hold valuable ground in the borderlands between scientific and humanistic inquiry. Indeed, in many ways the four-field idea has become an impediment to North American anthropology, as a chimera both infertile and of questionable genealogy and form. As the contributors to the Segal and Yanagisako volume mostly argue, the "sacred bundle" (a snide University of Chicago label for the four subfields) functions as a sort of professional loyalty oath and *churinga*-like embodiment of group self-regard. Viewing the four-field approach as an idol of the tribe is by no means farfetched, although it smacks of the by-now rather tired trope of ethnographic irony, in which self and other trade places (see Horace Miner's [1956] moldy chestnut, "Body Ritual among the Nacirema"). Nonetheless, many readers will be sympathetic with the tone of frustration struck in the introduction, in which the vast majority of us who have little allegiance to the orthodox and institutionally reinforced vision of the tetralogy are nevertheless forced to genuflect to this mysterious abstraction, usually without the benefit of a benevolent St. Patrick to explain it to us in layperson's terms. Many is the job interview blown, key colleague offended, or grant not won by the failure to speak the magic words to the right people. Painful personal experience aside, the irony of a field that prides (and often sells) itself on its antinomianism placing such great store by professions of orthodoxy is rich indeed, although perhaps not unexpected to readers of Victor Turner

(1967), who described the ways in which the two poles could be seen as moments in a ritual process. (If fieldwork is liminality, then structure is represented by defending the dissertation to your department, including that one guy with the four-field fetish.)

Segal and Yanagisako's volume raises interesting theoretical and political questions. As good comparativists, we must ask why it is that the anthropological traditions of North America, and to a lesser extent Australia and New Zealand, combine anthropology in its broadest, 19th-century form, heir both to Broca's comparative anatomy and Grimm's comparative philology, and archaeology, which had its own distinct history and identity, along with the new field of cultural evolution. As Yanagisako argues, this has much to do with these nations' identity as "settler colonial" societies. In contrast to Europe, where archaeology is more closely aligned with history, as a means of tracing the genealogy of contemporary society past the horizon of written history, in settler colonial societies the burden of archaeology is to place indigenous peoples clearly within the framework of the primitive and of what archaeologists continue to call "prehistory."[1] In an apt turn of phrase, Yanagisako discusses "the complex mix of knowledge production and erasure of indigenous peoples" (Yanagisako 2005:83) by which anthropology, in full alignment with the political interests of newcomers, sought to deflect attention from extant indigenous societies to a theorized holism of primitivism. By such means, settler society established a claim to knowledge about the new land and its original occupants that is deemed to be superior to that of the indigenous people themselves. This was achieved in part through various constructed genealogies giving rise to a pseudo-indigeneity (Dominy 1995; Harkin 2007). One such construction was the idea of adoption, as practiced famously by Lewis Henry Morgan (Yanagisako 2005:83–85; see Harkin 2001). Another was often some notional biological descent (the "Cherokee Princess" phenomenon). Thus, many elite whites have claimed connections to certain figures in Native American history, most notably Pocahontas, without losing their status as elite whites (what is known in Virginia state law as "the Pocahontas Exception"). Or, in a more theoretical and generic mode, the model of sociocultural evolution places indigenous people in the role of "ancestors of us all."

However, none of these modes of appropriation can really be seen as originating within the field of anthropology. Indeed, as early as 1585 the artist and colonial governor John White made explicit the connection between

[1]The problem with the term "prehistory" in archaeological usage is the multivalent nature of the term "history." It is fairly uncontroversial that many of the cultures that archaeologists study had no written history (although Andean *quipus* and other forms of recording would seem to undermine that assumption in many parts of the New World). However, history refers as well to the *res gestae*, or events themselves, and more broadly to notions of historical consciousness and agency. Thus to speak of "prehistory" is, like with other examples of the trope of "pre-ness," to ascribe a lesser humanity to those in such a state.

contemporary coastal Algonquians and the tribal ancestors of the Britons, such as the Picts (Harkin 2007). Anthropology as a field has been a synthesizer and proponent of such discourse, but it makes little historical or logical sense to pin the blame on anthropology, much less on a particular national configuration of the field such as the tetralogy.

If anthropology did not originate the comparative, anachronistic discourses surrounding questions of primitivism and national identity, it certainly filled a perceived ideological need. Indeed, as has often been pointed out, the history of diffusion of anthropological knowledge has closely tracked the nation-building project. The displays at the Chicago Columbian Exposition, including Franz Boas's ethnographic ones, were explicitly linked to technologies of transportation and communication, and to the broader notion of progress, to which the indigenous and traditional peoples provided a dramatic counterpoint (Fogelson 1991; Raibmon 2005). Parenthetically, of course, the native actors had a much different project in mind as the basis of their participation (Fogelson 1991; Gleach 2003; Raibmon 2005:65). However, the construction of a radically disjunctive indigenous identity, at the heart of the Boasian project, certainly contributed to the ideological edifice of a modern "nation of immigrants."

A key point here is the above-mentioned "denial of coevalness," specifically what Fabian calls "allochronism," a notion that holds that the proper time of the Indian or savage was in the past (Yanagisako 2005:85; Fabian 1983). If they continue to exist in the present, they are mere remnants, through which the white observer may nonetheless gain some glimpses of an earlier temporality. This is a productive concept, which certainly can play a large role in the critical understanding of American anthropology in the 19th and early 20th centuries, particularly approaches marked by a romanticism of the disappearing savage. I would agree, though, with Herbert Lewis (1998:720) in his assessment of mid-century anthropology as being nearly the opposite of allochronistic. Nevertheless, Yanagisako sees it lurking behind every sort of anthropological representation of the native, from evolutionism to what she calls "ethical-retributive commitments," which I take to mean action anthropology and related projects, such as ethnohistory. While it is perhaps not difficult to see evolutionism and Boasian salvage anthropology as flip sides to the same coin, it is more difficult to view approaches that specifically take historical and political data as their subject matter as allochronistic. Indeed, the origins of an anthropology of indigenous people that is ethically committed, politically aware, and historically oriented may be seen in the rise of American Indianist anthropology after World War II (McMillen 2007; Harkin in press). Yanagisako mistakenly subsumes this under the label of "Indianology," presumably to contrast it with what she believes to be more worthwhile projects in other parts of the world. This is a pernicious trope within sociocultural anthropology, which has the effect of decoupling the "domestic" from the international sides of issues such as

indigenous sovereignty or environmental justice, when in fact they are closely linked, and when such issues tended to receive their first treatment in the Native American context.

In a more general sense, the authors contributing to *Unwrapping the Sacred Bundle* possess an unearned certainty, smugness even, that a certain brand of postmodern liberal (although clearly not neoliberal) cosmopolitanism with which they identify is the only acceptable anthropological "brand." Just as in electoral politics, a process of erasure and the creation of *topoi* suffice to represent the other side. One gets the message that those who do not agree with them are naive positivists, politically retrograde, or worse. Of course, the straw man is a fixture of any polemic, which this book partly is.

If one reads these books in the order that I did, just as one is finishing up the Segal and Yanagisako volume, convinced that it is an argument based, if not on shifting sand, at best on the back of a straw man, this straw man himself is conjured to life in the form of H. Sidky. (I am tempted, given his fortuitous appearance, to think of him purely in terms of his authorial function, "H," a sort of modern counterpoint to the "J" or "Q" of the biblical texts.) (see Bloom 1990). Sidky's book is almost an instantiation of the straw man that the pomos construct. The title positions itself as a rearguard action, in defense of "disciplinary traditions." In fact, the point of this thorough if obviously slanted recounting of anthropology's history, is to defend "science." This is not science as it would be understood today, but as a positivist folk model predating Ludwig Wittgenstein, Thomas Kuhn, and Michel Foucault, among many others. Wittgenstein (1953) demonstrated that, contrary to the hopes of the logical positivist project to which he himself had contributed, language has meaning only within the context of a particular shared set of assumptions and a community of discourse. Kuhn (1970) showed that science was by no means exempt from these limitations, but indeed exemplified them. Foucault (1977) made the further observation that science was not merely one discourse among many, but was intimately allied with political power and technologies of control. Thus to suggest that one might reconstruct some notion of value-free science as imagined by Max Weber (1946) is really just to dream a dream of a golden age that of course never existed.

One of the many problems with Sidky's book is its confusion on the basic outline of anthropological history. The golden age model and the oft-repeated (six times at least in the text) subtitle "in defense of disciplinary origins and traditions" suggests that he must find the source of our problems in some modern rupture. As James Clifford (2005) argues in the Segal and Yanagisako volume, fears of disciplinary fragmentation are a structure of the *longue durée* in anthropology. Far from being a recent conflict that we can blame on postmodernism, in fact anthropology has always felt the weight of its multiple origins and contradictory commitments. Indeed, Sidky does blame much of the current mess, as he sees it, on Franz Boas, and

particularly on the Boasian critique of evolutionism. Sidky's take on Boas as being both "scientific" and "anti-science" is admirably nuanced, but fails to take into account the nature of sociocultural evolutionism in the late 19th and early 20th centuries, (We should be thinking of Frazer [1894] and Spencer [1886], not Charles Darwin.) Indeed, as Herbert Lewis (2001) has argued, Boasian skepticism and empiricism link up with similar qualities in Darwin. What Boas rejected was not so much nomothetic science, as Sidky argues, but the toxic combination of evolutionary ideologies and political interests. That Sidky believes that the "scientific" issues can somehow be divorced from racism, colonialism, and eugenics is both naive and strangely tone-deaf to the major issues of the modern era (see Baker 1998).

Indeed, Sidky is willing to give a pass not only to those such as Francis Galton (1883) (the "father of eugenics") against whom Boas was arguing, but later anthropologists who defended colonialism, such as Steward (1972) and Radcliffe-Brown (1957) (see Feuchtwang 1973:90; Pinkoski 2008). In fact, the ethical lapses of these figures go well beyond politics, and relate to their own conception of "science," which largely consists of what Michael Silverstein (2005:105–109) in the Segal and Yanagisako volume calls "calques"—metaphoric borrowings of theoretical constructs in other (i.e., more scientific) fields. Thus, Steward borrows the idea of ecology from biology, positing the concept of levels of "sociocultural integration," which is a calque of the biological notion of organic complexity. This particular theoretic commitment, in turn, forced him to be selective in his data when arguing, for instance, for the primitive quality of the Shoshonean peoples of the Great Basin (Pinkoski 2008; Steward 1972:101–121). In this way the "scientific" anthropologists have always been caught in this particular (metaphoric, to be sure) feedback loop between an open-ended field of cultural data and calques of scientific models generally produced under rather more controlled circumstances.

Much of the "scientific" lineage of anthropology: Radcliffe-Brown (1957), White (1959), Steward, Harris (1979), Freeman (1983) has been equally obtuse about their own violations of scientific method as they have been deaf to the critiques of the Boasians. The apotheosis of this sort of doubled error is surely the Mead-Freeman controversy, which Sidky feels obliged to resurrect, despite the large amount of commentary already published on it. The casual nature of Margaret Mead's data collection and her overreaching conclusions are both more than matched by Derek Freeman's own selective use of data to support much larger claims about human nature. Whereas Mead used data from an appropriate sample (adolescent girls and young women) of the Samoan population to investigate adolescent psychosexual development to question universalistic assumptions, Freeman employed data culled primarily from an inappropriate sample (male hereditary chiefs, living in a different historical era from Mead's sample) to reassert a universalistic notion of psychosexual development. And yet, for commentators such as Sidky, it is Mead who commits the greater sin against science!

The deafness to the sorts of critiques offered by Boasians, interpretivists, and postmodernists (often lumped together) to science-calquing forms of anthropology by Sidky and others bespeaks a profound divide within anthropology that often approaches the levels seen in American electoral politics. Like Hillary Clinton in the Democratic primary campaign of 2007–2008, Sidky accuses his various enemies of being so concerned with language, specifically writing, that they have lost sight of the world of materiality and action. They do little fieldwork, and have little understanding of what they do see, because they ignore "human *cultural universals*" (Sidky 2003:351, his emphasis). It is interesting that part of Sidky's argument is that we should not bother paying attention to tropes, which are the mere window decoration of fancy writing, when in fact his underlying worldview is revealed in his own reliance on the trope of words vs. action, a key component of Anglo-Saxon "common sense," and not at all a "cultural universal."

Still, Sidky's book reminds one of an important point, albeit one that is perhaps less burning now than in the 1990s, which is that postmodernist anthropology primarily talks to itself. For all of its theoretical sophistication and literary innovation, it has a limited audience even within anthropology. Eschewing the language of science forfeits the larger audience it might have had in the general public and the political realm. Although the high tide of postmodernism has receded, many of the most prominent American anthropologists, such as those who contributed to the Segal and Yanagisako volume, share this basic orientation and genealogy. If we cannot talk to colleagues within our own departments, then with whom can we talk?

ANTHROPOLOGY AND THE INTELLECTUAL ECOLOGY OF THE ACADEMY

For several of the contributors to *Unwrapping the Sacred Bundle*, the solution to the problem of audience is to forge connections with neighboring fields, primarily within the humanities. If cultural anthropology is freed from its burdensome attachment to the other subfields, it will be able to recreate the intellectual ecology of the modern university, with alliances to other fields that make use of the culture concept. Indeed, the master concept of culture has become invested with far more explanatory power than it had even in the heyday of the Boasians. At the same time, as James Clifford (2005) argues, the concept has lost ground in the other subfields, replaced by various forms of non-cultural determinism, such as Chomskian linguistics, human behavioral ecology, sociobiology, and so forth. Clifford sees other components of the anthropological configuration diminishing, including its object, the idea of savage, primitive, or archaic society. In fact, one could well argue that the only part of Clifford's schema that is still somewhat intact across the subfields is the ethnographic method. Perhaps even more than the notion of culture,

the method of ethnography has spread far and wide, well beyond the boundaries of anthropology itself.

While this will not lead to a reconfiguration, which would be "utopian," one can hope for "a thorough decolonization of research and a rebirth of hermeneutically sophisticated ethnography," according to Clifford (2005:40, 42). Well, one can always hope, but it seems that this, too, is rather utopian. One wonders what conditions would have to be met for ethnography to be fully "decolonized"—refusing grant money from NSF?; publishing only in open source formats? It seems that anthropology is caught in a rather peculiar Foucauldian trap. Aware that scientific discourse is inextricably intertwined with webs of power, political and otherwise, it at the same time depends on being taken seriously, that is to constitute its own scientific discourse, for its very institutional existence. So really, the option of full decolonization does not exist except perhaps as a final cathartic act of self-destruction. What Clifford seems actually to have in mind is a new set of alliances with fields in the humanities and, particular, those interdisciplinary "niche" fields, such as gender studies and cultural studies. By "niche" I do not mean to impugn their own significant contributions to important questions, but rather to draw attention to their institutional (and, what amounts to the same thing) discursive position in the academy and society more broadly. Like a naively ethical person who suddenly becomes aware of the moral dimensions of eating meat, Clifford is suggesting that anthropology move down the food chain—or, to use a more dynamic metaphor, attach ourselves to a sinking ship (see Donoghue 2008).

There are a number of pragmatic reasons why this is not a realistic option, not the least of which is that anthropology is in fact a moderately expensive science, which will always be reliant on government and private grants, and so could never as a discipline survive on the level of support provided to a field like gender studies (or even less, cultural studies, which seems to be fading from the American academic landscape altogether). I will talk more about this in the context of the public university below, but for the moment it is clear that although certain anthropologists, particularly those at points in their careers where long-term fieldwork is not possible, have chosen to make alliances with other fields, such an arrangement would never work on the level of the discipline as a whole.

Beyond pragmatic considerations, it would seem an odd choice for anthropology to make, to blend back into the humanistic milieu from which a large part of it came. The efforts of Franz Boas in the United States and Claude Lévi-Strauss in France to extract anthropology from the humanities would have been in vain (see Harkin 2009). In renouncing anthropology's claims to be a "human science," in Lévi-Strauss's words, we fall into what could be called the humanistic trap. The problem with humanism, as the French giants of the last century were particularly aware, is the implicit, unexamined metaphysical assumptions always contained within such discourse.

We associate this critique especially with Foucault and Derrida, but it is Lévi-Strauss (1966:258) who offers, to my mind, the most withering critique of anthropology's closest intellectual neighbor, history: "The price so paid for the illusion of having overcome the insoluble antinomy (in such a system) between my self and others, consists of the assignation, by historical consciousness, of the metaphysical function of the Other to the Papuans." On one level, this may be read as a simple indictment of ethnocentrism in Western thought. The more we affirm the commonality of a European, Western, or "Judeo-Christian" identity, the more we emphasize our separation from those on the outside. In a more sophisticated reading, Lévi-Strauss seems to be suggesting that by denying the other within (that is, the absolute separation among individuals even within the same community, and even more so among different communities), we project it onto the category of the other, always present, always waiting to be filled with content. This operation—a sort of Freudian totemism—is the opposite of sublimation. If we repress the differences that really exist in the social and natural world and which, in the structuralist account, constitute the very basis of consciousness, we are forced to be blinded to our common humanity with the Papuans, or the Bushmen, or the Kwakiutl, or whoever fills the current role of primitive other, whether our currently fashionable narratives about them are ostensibly positive or otherwise.

One could surely multiply such examples many times over, the point being that the intellectual and ethical price to be paid for the comforting illusions of humanistic discourse is great indeed. Moreover, Lévi-Strauss's line of thought is in fact very much in the Boasian mainstream. Much to the dismay of Sidky and others of his ilk, Boas and Lévi-Strauss after him offer radical skepticism in the face of attractive narratives, coming from either "science" or the humanities. Surely, Boas would have recognized the critique of implied metaphysics as it applied to his own rejection of evolutionism. Indeed, as Rosalind Morris (2007) argues in her masterly review essay on Derrida, the extreme Derridean skepticism evident in his critique of the metaphysical assumptions in linguistics and related fields resonates with many of the concerns that anthropology has traditionally held. Nor is it absurd to see some elements of the Boasian reduction in poststructuralist and postmodernist thought, as Herbert Lewis (2001) argues.

ANTHROPOLOGY AS META-SCIENCE

It would be tempting to argue that this strain of Boasian skepticism and empiricism is what unites modern American anthropology, but of course we have seen that in fact nothing, other than institutional arrangements, does. Still, imagined somewhat differently, less as a totalizing structure than a particular sort of intellectual space, anthropology has been remarkably

successful and looks forward to a bright future. The future mapped out by Rena Lederman in *Unwrapping the Sacred Bundle*, as well as many of the contributors to *Holistic Anthropology*, seems more productive than the anthropology as science–anthropology as humanity posturing discussed above. As Lederman (2005:63) argues, anthropology has long been a pragmatically oriented discipline, in which "rationales for moving around and among the subfields were specific, improvised, and topic- or argument-driven." This is equally true of those who find second homes in other fields. What is more, both the scientific and humanistic strains of anthropology share an important feature in the larger intellectual context; anthropologists tend to be anti-reductionist, at least in so far as strong versions of reductionism are concerned. In summing up, Lederman (2005:73) paints a fairly optimistic picture of anthropology, if one can get past the problem of holism:

> Fusion or fission are not our only options. Over its history, "four-field anthropology" as a pragmatically institutionalized discipline has produced humanist/scientist hybrids of various sorts; it has been the ambivalent guardian less of a "sacred bundle" than of a rare nesting ground—a condition of possibility—harboring anti-essentializing evolutionists, hermeneutic realists, and other third kinds.

This valediction, which closes her essay, captures what is in fact extraordinarily valuable about anthropology. It is in this way that calls by the first wave of postmodernists for anthropology to be more innovative and "experimental" have been answered. Anthropology functions less as an established science than as a meta-science, with new patterns of connection among theories, ideas, data sets, methods, undreamt of in other disciplines, arising with stunning regularity.

Examples of this sort of connection abound in the excellent volume *Holistic Anthropology*. This book is remarkable less for the innovation of the topics covered—many are fairly familiar by now—than for its systematic use of examples such as ethnomedicine, landscape studies, and cognitive anthropology to demonstrate the immensely rich ways in which a cultural orientation can meet various kinds of science. The authors of *Holistic Anthropology* echo Lederman's plea for anthropology as an intellectual space in which such connections can be made, but without imagining it as a structure generating a *doxa*. Although the situation is somewhat different in the U.K., which has never had a four-field model in the first place, the comments of Tim Ingold (2007:209) seem very much in the spirit of the anthropology Lederman argues for:

> I believe it is crucial to dissociate holism from a concern with wholes. Holism is one thing, totalization quite another, and my argument *for* holism is, quite directly, an argument *against* totalization. Or, more precisely, it is against the particular conception of part-whole relations that totalization implies.

Ingold goes on to list three ways in which it is different: open-ended, emergent, and processual rather than structural. His specific project that constitutes his chapter is something of an abstraction of his earlier work on movements across the landscape (prototypically of hunter-gatherers). He employs the image of the line as a central trope in attempting to create a phenomenology of place, movement, life history, and genealogy. Drawing on De Certeau's (1984) famous analysis of walking and Derrida's (1976:86) borrowing of Leroi-Gourhan's (1982) graphism, as well as Bergson (1911), Darwin (1959), and sundry other sources, Ingold sketches out ways of looking at movement and life history that would be useful for anthropologists working on a broad range of topics. It would have benefited from more engagement with network theory, especially as a demonstration of the way such a model could handle empirical data, but this is a clear implication of his work.

Howard Morphy (2007) argues that anthropology has been too quick to jettison older theories, which are understood in exaggerated caricatures. Thus a concern with structures (either in Lévi-Straussian or other models) has been replaced by a focus on agency, event, and relationship (Ingold's chapter being a case in point). He demonstrates through his analysis of a Yolngu ritual sand sculpture representing an origin story that structure and event are both necessary analytic moments, and that ethnography is in fact a complex interpretive process. The evident need for such a demonstration shows how quick is anthropology to forget the lessons learned by earlier generations, and to engage in the narcissism of small difference.

In the most compelling example from this volume, Laura Rival (2007) in "Domesticating the Landscape, Producing Crops and Reproducing Society in Amazonia" begins with the "new" cultural ecology practiced by those such as Tim Ingold and Philippe Descola (1996), and engages with various strains of materialist approaches, including human behavioral ecology and cultural evolutionism. She argues not that Amazonian adaptations to the environment can be explained solely by either cultural-cognitive models or by materialist-determinist ones, but that a hybrid model of some sort is necessary. At the same time, she takes quite seriously the evolutionary argument, especially in the context of debates in Amazonian archaeology over evolution and devolution in the wake of European contact. What she produces is a nuanced portrait of the diversity of Amazonian people's responses to, inter alia, the Columbian invasion, histories of intertribal conflict and cooperation, and traditions within tribes. She echoes Ingold's discussion of lines as being at once pedestrian movement and genealogical lineage: "Trekking [for the Huaorani] is not simply a mundane activity relating to the pragmatics of subsistence, but, rather, a fundamental way of reproducing society" (Rival 2007:73).

Choices on subsistence are not dependent solely on the resources available, nor on knowledge of technology, but first and foremost on a social identity that is constructed over time, often in contradistinction to other groups. Thus, certain groups such as the Huaorani choose not to intensify

horticultural production, although they practice limited horticulture, mainly for ritual products (reminiscent of many native North American groups who cultivated only tobacco). At the same time, other Amazonian groups maximize horticultural production, using it as a marker of their own (to them superior) identity. In this sense, to consider only the evolutionary dimension of horticulture is to miss the point that food production fulfills many functions in addition to supplying nutrition. Echoing the ultimately Boasian call for anthropology to be pragmatic, empirical, and open-ended, Rival argues that the development of horticulture is conditioned by many cultural and historical factors:

> Plant domestication is an evolutionary, historical and cultural process, which needs to be viewed through the holistic lens of the new ecological anthropology paradigm. Its proper analysis requires the development of a unitary analytical framework that integrates relations between biology, ecology, economy, material culture, language and identity. (Rival 2007:87)

Such pleas to move beyond various sorts of static dualisms of the nature-culture sort are the common currency of this volume, and remind us that anthropology is by its very nature inclined to reach out to other fields, but always to reinterpret what is found there in new, holistic contexts. It is partly the case, I believe, that anthropology, never having developed into a "mature science" with broadly accepted methodological-theoretical paradigms, has had this luxury, and has made good use of it.

ON THE GROUND: THE POLITICS OF TETRALOGY AT THE PUBLIC UNIVERSITY

Despite my general agreement with the point made explicitly in *Unwrapping the Sacred Bundle* and implicitly in *Holistic Anthropology*, that anthropology can be a dynamic field without the tetralogy, which has over the years sometimes stood in the way of innovation and made us guilty, especially in our communication with students, of a certain amount of *mauvais foi*, it seems to be up to me, as one who leads such a department myself, to offer some context and nuance to this discussion. Segal and Yanagisako argue, I think correctly, that the tetralogy is "a contingent formation," contingent that is on historical factors (both discipline-specific, and the larger ideological history of settler colonial societies) and institutional arrangements (Segal and Yanagisako 2005a:3). To point out what would be utterly unremarkable in an anthropological study of any sort (for instance in ethnographies of corporate cultures, or Amazonian villages) and endow this point with such significance seems peculiar and more than a little narcissistic.

Anthropology did not emerge fully formed from Zeus's (read Boas's) brow, but struggled through various sorts of political and institutional

obstacles to be born. Further, the fact that anthropology *did* develop into such a form in the United States, while not meaning that it cannot be changed, does suggest a certain adaptive fit to the political and institutional structures of American society. In particular, the evolution of four-field anthropology can be seen within the context of federal policies, especially those governing education, research funding, Native Americans, immigration, and historic preservation. Indeed, one can understand a great deal about anthropology in the modern public university by taking into account three seminal acts of the U.S. Congress: the Morril Act of 1862, the Antiquities Act of 1906, and the National Historic Preservation Act of 1966. The Morril Act, establishing land grant universities throughout the United States, but especially in the South, Midwest, and West, dictated that universities provide tangible services to the landowners of the state. The Antiquities Act and the National Historic Preservation Act both sought to protect lands and specifically archaeological treasures on those lands. Thus, by the early 20th century, at about the time that Boas was establishing his version of professional anthropology, archaeology was situated at the center of questions having to do with the disposition of lands and artifacts associated with Native Americans. The linkage to sociocultural anthropology, which in that day focused on living American Indian societies, was a natural one. Biological and linguistic anthropology could be called in to assist as needed. From the institutional standpoint this placed anthropology propitiously within the academy.

Having a connection with landowners in the state second only to agricultural extension agents gave anthropology departments access to one vector of power and influence to which few other departments on campus had access. A second vector was big-money federal funding to science, going largely (but by no means exclusively) to archaeology and biological anthropology. The third vector was publication markets occasionally including the general reading public (Margaret Mead, Ruth Benedict) but quite often an academic audience well beyond the confines of our discipline (Clifford Geertz, Claude Lévi-Strauss). This combination of political connections to the state, access to research funding, and prestige among our humanistic and social science colleagues has generally served us well, especially at public universities, where resources (both material and symbolic) are often scarcer than they are at the sort of institutions represented by the contributors to *Unwrapping the Sacred Bundle*. Add to this the fact that, because anthropology is generally not taught as a subject in public secondary schools, nearly all of our students come to us via an interest in archaeology (to which they often have exposure locally) or, in recent years, biological anthropology (via popular television programs featuring forensic anthropology). Many of these students find an intellectual home in sociocultural anthropology precisely because it does honor the contributions of materialist science while attempting to wed them to cultural questions, in flexible and innovative ways.

The reality is that in many public universities the subfields of anthropology make, if somewhat awkward bedfellows, natural allies. With different and complementary sorts of capital at their disposal, the subfields can, if they cooperate, build significant institutional presence. With the leadership of archaeology and cultural anthropology (realizing that in some departments biological anthropology plays a major role), anthropology departments can be a large presence in the intellectual ecology of American campuses, asking new questions in provocative new ways, and carrying on the Boasian mantle of skepticism, empiricism, and anti-racism.

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