A Student's Guide To Our Theoretical and Methodological Mouse Traps

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Abstract

The current state of disassociation within the field of cultural anthropology presents numerous difficulties for learning and teaching theory and method. This paper has been written by a graduate student, for students, as a tool for contextualizing and organizing contemporary theory. In addition to providing a historical framework for understanding today's theoretical challenges, the practical implications of these issues are also addressed. Applied anthropology is presented as one significant response to these challenges.

When an academic discipline, such as anthropology, can no longer agree on what it does or should be doing, the ambiguity and inconsistency of its vision is offered to its graduate students as an array of appetizers, served with a heavy sigh and the hope that they will be able to find sustenance in the ongoing prelude to a meal. So varied are the options, the arguments, the content, and the contexts, that it is impossible for today's student to escape exposure to fragmentation, particularity, and relativity. At the same time, many will find themselves, degree in hand, with little more than an anecdote to summarize over a hundred years of scientific research, application, and generalization. Having been enticed into anthropology by the alluring taste of holism, we soon discover that our prize is indeed full of holes, and that we've been left with our tails in a vice, wondering how, and if, there is a way out of this mess?

This article is intended as an introductory map through the methodological obstacles and theoretical problems which have created the environment in which we, as students, find ourselves. Emphasis has been placed on the limitations and potentials of science, as this was the traditionally unifying paradigm, and as this is the subject that remains critiqued, yet largely absent from our education. While the usual approach has been to describe the current fragmentation within the discipline as the result of postmodernist and interpretivist deconstruction, this view is misleading. To understand the decline of positivism in anthropology, we must look back as far as the 1950s, if not further, and consider the difficulties these scientists bequeathed to the generations that have apparently rejected them. The issues we will confront include: the problem of subjectivity, the various roles of explanation, description, and comparison, the rapid pace of global social change, considerations of ethics and power, and finally, the use and application of anthropology.

Scientific descriptions, whether they are intended for explanation or not, depend upon our ability to classify and arrange observations. The systematic ordering of phenomena, material or psychological, provides the means by which observations may be verified as well as compared and contrasted (Kaplan & Manners 1972). A prohibition against comparing apples and oranges, for example, depends upon our ability to distinguish one from the other. Early anthropologists, of both the particularist and comparativist varieties, were immediately beset with the problem of classifying social and cultural activities. In fact, as recounted by Marshall Hyatt in, *Franz Boas: Social Activist*, much of Boas' career was spent refuting spurious racial and evolutionary typologies (1990).

While the problems of classification are many, all may be neatly summarized with the question, "what does a given category tell us?" To this, the naturally evasive response is, "it depends on what it is that we hope to know." Much of the contemporary debate between the loosely grouped "humanists" on the one hand, and the more easily discernible "positivists" on the other, is the result of different answers to this second question. The division is not one of philosophy, but of interest. Those who wish to learn about human similarities will utilize vastly different categories than those who focus their observations on cultural differences. Furthermore, the relative flexibility of a category will reflect the kind of questions that are being asked. Questions about the meaning of a symbol will invite dense "interpretive frameworks," while inquiries into pivotal transitions in cultural evolution may require broad ethnographically shallow classifications. And so we have, in the history of the field, a range of definitions of culture that stretch from the general, "culture . . . refers to the learned repertory of thoughts and actions exhibited by the members of social groups . . ." (Harris 1980: 47), to the specific, "the concept of culture I espouse...is essentially a semiotic one . . . man is an animal suspended in webs of significance . . ." (Geertz 1973: 5).[2]

This is all fine and well at the level of theoretical abstraction, but when we are confronted with the problem of developing cultural typologies which are capable of synthesizing field notes or facilitating the cross-cultural comparison of ethnographic data, we find that our attempts are fraught with obstacles. This became particularly evident during the 1950's, when cross-cultural comparison experienced its brief, but informative, golden age. As anthropologists began to assess their comparative work, they were forced to contend with over half a century of fuzzy definitions and soft concepts. It became clear that much of the earlier data was inadequate and, or, incomplete, and that operationalizing social concepts involved additional difficulties. For example, in describing the inconsistencies between his own residence data with that of John Fischer, Ward Goodenough found that even the acceptance of a common cultural category did not prevent divergent interpretations of the ethnographic facts:

> Even where we agree as to what the patterns are, we cannot agree as to what cases conform to them. In this instance, the same sociological and cultural data were available to both of us. Where we differed was in regard to what aspects of it we considered relevant for classifying a couple's residence. (1956: 27).

Michael Moerman encountered similar difficulties in trying to determine who it was that he was studying in Thailand. He attributed his problem to the, "ways in which ethnologists demarcate ethnic units and account for their survival" (1965: 1215), and suggested three obstacles to defining ethnic identity: "1) Ethnicity is impermanent in that individuals, communities and areas change their identification . . . ; 2) Various non-members may use ethnic terms differently . . . ; 3) Members may not always use the same term for themselves" (1965: 1222-23). In his response to Moerman's paper, Naroll cites a letter from Goodenough indicating that while it is possible to categorize the group he studied, "there are no categories that have universal applicability for all problems. They follow from the variables one is examining" (1968: 77). By the 1960's, anthropologists such as Naroll and Moerman were referring the problem of the incompatibility of classifications between nomothetic and ideographic projects as "Goodenough's Rule:"

> Goodenough (1956: 33) holds that concepts suitable for comparative purposes may or may not be the ones most suitable for describing particular societies and vice versa" (Naroll 1968: 74).

This issue, it should be realized, is not yet at the level of observer subjectivity and the Rashamon problem (Harris 1980: 321-322); these scientists were confronting the practical difficulties of actively pursuing a positivist research strategy. *They were trying to measure the same thing*, and were coming up short. In response, cultural anthropologists intensified their efforts to develop methodological rigor, while simultaneously questioning the utility of the existing ethnographic database:

> With the use of the comparative method comes the application of statistical techniques, although much of the information that is available at the present time was not collected with this end in view (Colson 1954: 3).

We doubt that available source materials on most societies, in the Human Relations Area Files or elsewhere, are adequately refined to allow assured judgments...We must, of course, work with material at hand until additional and more suitable data are available; in the meantime a necessary safeguard against misinterpretation in cross-cultural studies is a sharp eye toward the significance of traits within their own cultures (Norbeck, Walker, & Cohen 1962: 481).

Both Colson and Norbeck et. al., propose that data collection techniques must be more contextual and holistic, abandoning the "all-or-none" approach in favor of distribution models which account for variation. This, of course, makes generalizable categories more difficult to develop. To oversimplify and explain, it is more troublesome to define and compare the "usually patrilocal" with the simply "patrilocal." Or, perhaps closer to the issue, it is harder to classify rites of passage with significant variability as opposed to the clumsy separation of "violent" rites from "passive" ones. As a critical aside, Marvin Harris also noted that many typologies, such as those used in the HRAF, are particularly dubious because they fail to distinguish between emic and etic realms (1980: 49). But assuming for the moment, with all of these authors, that theirs' was ultimately a problem of loose operations and insufficient science, there remains yet another, more thorny hedge of obstacles to the classification of social phenomena:

> If type is conceived in the historical-index sense as a means of arranging phenomena in time and space, there is the inevitable dilemma that types cannot define areas or periods nor can areas and periods define types in an absolute sense, each being relative to the others (Ford & Steward 1954: 56).

Having set aside the problems of defining cultures and their constituent parts in terms of space or form (by recognizing the relativity of formal and spatial categories and resolving to intensify research methods and make the type fit the question), we are now vexed with the issue of change across time. This difficulty is apparent in the organization of the Human Relations Area Files, in which the same people, say the Hopi, are given two different files corresponding to different time periods, and then treated as if they were completely separate cultures. Other examples of the time problem emerged in two of the most famous conflicts in the discipline: Margaret Mead's version of teenage sexuality in Samoa versus Derek Freemen's account (Shankman 1994), and Robert Redfield's harmonious Tepoztlan as opposed to the turmoil described by Oscar Lewis' (Hannerz 1980: 70).

Turning to the history of urban anthropology, we find that city typologies, with various degrees of utility and historical accuracy, are also notoriously confined by local and temporal conditions (Hannerz 1980: 60-118). On a larger scale, the concerted effort to isolate the causes of the agricultural revolution resulted, by the 1950's, in Julian Steward's historically and locally situated concept of, "multilinear evolution" (1956). While all of these examples include apparently effective use of cultural categories and lend support to HRAF's time slice treatments, they also beg the question of, "what do our categories tell us?" At the very least, we find that the theoretical range of our classifications has become increasingly limited in time, space, and form.

In their attempts to resolve the typological conflicts of time and space, Ford and Steward suggested that effective categories should be developed in terms of function:

> The time and place occurrence of cultural features can be defined quantitatively in terms of historical-index types, but the impasse of classifying cultures and reconstructing cultural history can only be resolved by introducing functional criteria into the definition of type . . .
>
> Steward goes on to say that:
>
> Emphasis upon function, however, would give primary importance to selected features and permit historical hypotheses...the effort to establish types of cross-cultural significance has been thwarted largely by the importance ascribed to form. If, however, function is given equal importance, the way is opened for fruitful comparative studies" (Ford & Steward 1954)

This seems plausible enough; if we are looking for comparable categories, we should choose types which are generalizable and amenable to the kinds of comparisons that we want to make. We are back to Goodenough's rule, but now we have descended into the slippery realm of function. There is no need to elaborate on the well established logical problems of functional tautology (Jarvie 1965), for Steward already notes that these types are merely heuristic devices that point the researcher toward testable hypotheses:

> Functional types cannot be established in terms of universal features, nor do they have objective reality. To the contrary, substantive types of heuristic utility must be postulated provisionally, gradually, and always with reference to the historical problem (1954: 57).

However, this move from functional types to testable hypotheses presents a recursive dilemma that returns us to the many pitfalls of categories in general. The following summary illustrates this process: 1) We begin with the effort to describe if not explain similarities and differences in human groups; 2) We attempt to classify phenomena by form, but are thwarted by practical difficulties, poor operations, and ultimately the problem of change; 3) We shift our classifications to reflect more generalizable categories of function; 4) Recognizing the tautology of function, we use its orienting principles to formulate specific hypotheses; 5) Pursuing the methods of hypothesis testing, we find that we must operationalize our variables; and, 6) Operationalizing our variables is isomorphic with classifying and we are returned to the problems which began at step two.

Constrained by the difficulties presented by time, space, form, and the innumerable practical barriers to ethnographic work, Elizabeth Colson concluded that she must redouble her efforts to develop quantitative rigor and limit her research to, *The Intensive Study of Small Sample Communities* (Colson 1954: 3). Taken as a whole, and in the most charitable light, all of these obstacles emphasized the need for anthropologists to contend with particular contexts and historical processes.[3] Whether one is doing descriptive ethnography or comparative evolutionary explanation, the problem of classificatory distortion is primary. Since the 1960's, the fallout from these difficulties has met with various responses. The ascendant view is perhaps best expressed in the work of Clifford Geertz:

> We must, in short, descend into detail, past the misleading tags, past the metaphysical types, past the empty similarities to grasp firmly the essential character of not only the various cultures but the various sorts of individuals within each culture, if we wish to encounter humanity face to face (Geertz 1973: 53)

The scientific version of these extreme conclusions is exemplified by the branch of cultural ecology known as methodological individualism and anti-essentialism, (Vayda 1987, 1994). The response of more traditional positivist scientists provides a quite substantial rebuke to this relativistic retreat, but we will reserve this case until the point at which the full extent of our problems have been laid bare.

Before we may address the more familiar territory of the interpretive critique, let us first confront what is perhaps the most influential force in contemporary anthropology, namely the "reality of change and the certainty of mobility.[4]" This condition, cogently summarized in the first paragraph of Arjun Appadurai's, *Global Ethnoscapes: Notes and Queries for a Transnational Anthropology*, forms the puzzling backdrop for all further anthropological work (1991). Not only are the conceptual boundaries of space, time, and form, pragmatically difficult to isolate, identify, and record, they are also amorphous, overlapping, and permeable. Whether or not this social context is qualitatively or quantitatively new remains at issue, but the globalization and dissolution of fixed territories and bounded cultural groups is a demonstrable contemporary fact, and one that figures prominently in the concerns of modern cultural anthropologists. Robert Hackenberg makes this transition, which he often describes as, "the advance towards vagueness," abundantly clear in, *Reflections on the Death of Tonto and the New Ethnographic Enterprise*:

> Today, the village is a place they pass through en route to or from Mexico City or Los Angeles. We confront an explosion of individual experience which appears to undermine most of our methodological tool kit (1993: 15).

Even where the anthropologist can still find a bounded group, say for example, the college dorm, Hackenberg reminds us that we have lost the most critical of ethnographic necessities- the key informant:

> . . . they will have no successors. I suppose the basic reason is that no informant, no matter how well-positioned within his community, any longer shared enough of his neighbors' experiences to be comfortable in generalizing about them . . . this is another way of saying that even in the most out-of-the-way places, life has become individualized and diversified beyond our wildest dreams (1993: 13)

In *Europe and the People Without History*, anthropologist Eric Wolf presents convincing arguments suggesting that the world never actually consisted of the isolated homogeneous groups of our historical imagination (1982). John and Jean Comaroff have also provided abundant evidence for the complex dynamics of historic cultural interactions (1991). But regardless of the extent of past integration, it is quite apparent that today, anthropology is reeling from the loss of its traditional local subject, not only because of its privileged domain, but more importantly, because old methods no longer suit modern contexts. George Marcus puts this nicely:

> . . . how does one reconstitute modes of ethnographic evidence and authority with nonlocalized subjects, emergent phenomena, not tied to well articulated local history or tradition, in worlds that are not at first take that unfamiliar to the ethnographer? (1994:52).

We see, therefore, that the physical and practical obstacles to a science of culture are many. From the difficulties of classification to sampling problems, change over time, and the increasingly blurred boundaries of human populations, resolving the question of what, or about whom, our categories or generalizations tell us, is no easy task. It is these problems, which now lie buried beneath the more fashionable flash of interpretive and post-modern critiques of positivism, that present the most significant challenge to anthropology. Whether one is interested in meanings or behaviors, explanation or description, comparison or the historical particular, we all face the methodological quandary of procuring verifiable data from a complex field of activity. For those who continue to believe in scientific research strategies, the burden of proof lies not only in resolving and mitigating these issues, but also, in communicating the scope of science as it is actually practiced, rather than as it is ideally conceived.

Having briefly summarized some of the pressing structural obstacles to developing or continuing a science of culture, we now turn to the familiar critiques of ethnographic objectivity, authority, and responsibility, and finally to note the extreme implications of these positions. At the heart of the shift from scientific method to interpretation and critique, lies the nagging suspicion that we are incapable of objective observation and description of social life beyond the most rudimentary material elements. The works of Said (1978), Clifford (1983), Geertz (1973; 1983), and Marcus (1994), to name but a few of the more influential critics, assert, if they do not quite demonstrate, that ethnographic description and explanation is, to varying degrees, historically constituted, politically situated, and individually distorted by every anthropologist from Lewis Henry Morgan to Paul Bohannon.

Between the seminal arguments of Clifford's original article in 1983, to George Marcus' summary assessment in 1994, we find that anthropology's refusal, or some would say inability, to resolve the Rashamon problem (Harris 1973: 321-322), the problem of subjectivity, has resulted in the following perspective:

> Discourses of the 'real' have been demonstrated to be of a piece with the rhetoric of fiction and to possess the fully literary character of language as narrative, subject to tropes, figuration, and self-consciousness (Marcus 1994: 40)

The Comaroffs add:

> . . . realism and rhetoric do not stand opposed. Just as the latter is not a mere aesthetic embellishment of a truth that lies elsewhere, the former is but one among many modes of constructing the past and present, with no greater claim on authenticity, no less attention to aesthetics (Comaroff & Comaroff 1991: 36).

Without wasting more space than is necessary dwelling on the already well worn pages of this ascendant paradigm, the key methodological difficulty is that the intersubjectivity of participant observation in all its forms, is thought to render descriptive replicability impossible. Ethnographic validity is passé, and cultural description and explanation has been replaced by representation. Or, perhaps more in line with the proponents of these views, the fallacy of realist description has been exposed and the *reality* of representation excavated, exhumed, and raised to high art. The paradox of relativity as reality is inherent in this perspective, but while it may be cumbersome to the formulation of a systematic methodology, it does not deny the heuristic value of this powerfully skeptical view.

In turning towards considerations of ethnographic representation, issues of power and authority have naturally become central themes in the academic discourse. Beginning with the four modes of authority offered by Clifford (1983: 126-142),[5] and extending into the work of Foucault, Bourdieu, Gramsci, and the synthesis of these efforts provided by the Comaroffs (1991: 13-36), we find that the entire enterprise of observation, scientific or otherwise, is afflicted with layer after layer of politics and power (Sass 1986: 53-54). This being the collective assessment of more than a decade of interpretive critiques, George Marcus concludes that:

> . . . at the present moment- in which social theory and philosophy have powerfully critiqued the very process of representation- the remaking of great traditions of theory is indeed a problem of the redesign of form. Simply put, this is what the critique of social scientific rhetoric, beyond the function of critique itself, gestures toward (1994: 42)

Indeed, if the failure of positivist social science is in fact a foregone conclusion, and representation is the essence of description, then ethnographic "form" is the primary methodological concern of today's anthropologists. And so the floodgates are wide open, and all fingers have been pulled from the positivist's dike. To those unfashionable stragglers who remain committed to "realist" principles, the conflation of science with power has tended to result in a denial of the critique in general. The baby and the bath water are confused, and we are left with the tragically comical practice of speaking at, across, and through, rather than *with* each other.

Regardless of the verifiability of reflexive analyses of anthropology in general, and ethnography in particular, at least three elements of the critique are essential components for any further development of the discipline in both its humanist and positivist forms. The first, as stated earlier, is the simple recognition of observer bias. The second, is the awareness of the political implications and manifestations of power in culture, including the culture of anthropology. And finally, the liberation of anthropological interests by the unrestrained application of relativity, has provided much needed sensitivity to, and appreciation for, the more subtle qualities and flavors of diverse cultural life. Geertz' proposal that anthropology might concern itself with, "the enlargement of the universe of human discourse," is an expression of this idea (1973: 14). Through the inclusion of emotion and art (Resaldo 1984), as well as the rich accents and ideas provided by diverse native voices and authors, the sometimes sterile *form* of our rhetoric may now become as human as its subject. But this is not the exclusive territory of either humanist or positivist anthropology, and like the first two, this third revelation is equally applicable to both perspectives. Frederick Barth, who has conducted quite a bit of what may today be looked upon as old fashioned social science, captures these insights particularly well:

> . . . it is sobering to notice the attitudes that have been nurtured by our reflexivity. They include irony, elitism, aestheticism, and at best indignation over the conditions

imposed on the less powerful. But why should love come across so weakly: love for people, for the problems, and wonder and love for the various forms of 'otherness' with which we engage? I believe this reflects the narrowing of focus, and the limited uses to which cultural 'otherness' (or alterity) is currently put in anthropological theorizing (1994: 350)

Turning, at this point, to the present and future of anthropology, it should be abundantly clear that even without reference to the politics of academic careerism, the desiccation of social science funds, the glut of anthropologists, or the declining practical value of a liberal arts education, the theoretical and methodological problems inherent in the study of human society are themselves sufficient to provide a daunting challenge to the field. Borrowing from our neighbors down the hall, we might say that cultural anthropology finds itself in the process of a punctuated cladistic event, as three major intellectual adaptations respond to the noted environmental pressures.

The first alternative, represented by the broadly based humanist interests, hardly requires elaboration; no one is questioning whether or not it is *possible* to interpret cultures, deconstruct oppressive hegemonies, or, "see the heaven in a grain of sand" (Geertz 1973: 44). These things are, have been, and will continue to be done as long as they pay the bills and anthropologists, cultural studies scholars, comparative literati, historians, communications visionaries, native intelligencia, popular journalists, speculative fiction writers, fledgling college intellectuals, movie stars, and homeless social critics continue to find them interesting. The opportunities for investigation are nearly limitless, and so are the kinds of people who consider themselves qualified to investigate.

On the other hand, as has been demonstrated here, the possibility of a scientific alternative to the study of culture stands or perhaps wobbles, under the relentless siege of a substantial critique and limited prior success.[6] Fundamentally, scientists offer two deceptively simple responses: 1) regardless of past failures or methodological obstacles, science remains the single most reliable way of developing probabilistic knowledge of human thought and behavior.

The answer to earlier mistakes and misinterpretation is not less science, but more and more finely crafted science. The problem, they say, is not one of a chaotic world, but of chaotic methods; 2) in terms of acquiring verifiable knowledge, the alternatives to science are ineffective, incomparable, and in danger of moral bankruptcy.

Before encountering these two responses directly, let us first ask, following Robert Carneiro, whether or not a science of culture is inherently impossible? This conclusion would require that either one or both of the following conditions are true: first, that the things and events studied by social researchers are not subject to cause, effect, and patterned regularity; And second, that the actual practice of controlled methodical study of these phenomena is simply too difficult to be profitable (Carneiro 1989: 3). Regarding the first of these, Andre' Köbben asks, "Do we *create* order in what is *factually* chaos, or do we *describe* order in what is *apparently* chaos" (Köbben 1970: 581). It is all too tempting to simply dismiss the chaotic view of social life as nihilistic, impractical, or just plain imperceptive, but this, I believe, is a fine example of talking past the critique.

The generalization of social incoherence or relativity is too broad to be anything more than an orienting statement. When scientists direct their attention towards patterns in human thought and activities, they restrict their categories of analysis to phenomena which seem amenable to their explanatory interests. Similarly, when humanists explore the particularities and inconsistencies in social life, they also confine the scope of their work to aspects reflecting their concerns. It is critical to note, however, that both perspectives require the use of generalizations; the former assumes a significant degree of order, while the latter assumes a high degree of chaos.[7] The short answer then, to the first criteria for the possibility of science, is that human social life is both chaotic and ordered, and that both are possible subjects of study, interpretation, explanation, and description.

The possibility of a scientific treatment of these issues does not, however, resolve the second practical problem of actually doing the work. Therefore much of the positivists' response has been devoted to accounting for past failures and suggesting methods for improving the validity of scientific research (Carneiro 1995; Harris 1980; Kapplan & Manners 1972; Pelto & Pelto 1978; Brim & Spain 1974). Again, it should be emphasized that rather than recalling past success, social scientists generally look to the future for vindication of their paradigm. While there are important exceptions to this, Boas' work on race for example, or somewhat ironically, ethnography's relationship to cultural relativism, social science would generally benefit from a more even-handed and open discourse on its cumulative contribution to modern thought and quality of life. The lack of this kind of shared information is what leads to the popular view expressed by Luis Sass:

> After so many failed prophecies, so much trivial research, and so little progress toward the discovery of the 'laws' of social behavior, the refrain with which conventional empirical studies typically end- 'More research is needed' - is beginning to sound hollow indeed (1986: 50).

So much of graduate students' time is spent juggling the bits and pieces of critique and disassociated theory, that the content of our collective knowledge about humanity, in both it's specific and general forms, is lost in the shouting.

Utilizing an additional explanation for the Mead-Freeman, Redfield-Lewis debates mentioned earlier, Roger Sanjek focuses in on the single most important scientific difficulty facing ethnographers, and by extension, those anthropologists who wish to use ethnography for comparative work:

> The challengers came to different conclusions because they used different methods (more revealing ones, they claimed), not because they failed to get Redfield's and Mead's results by using the same methods (Clifford 1986: 101-3; Lewis 1951: xi-xxvii; Weiner 1983, in Sanjek 1990: 394). It was *validity* that they challenged, 'the degree to which scientific observations actually measure or record what they purport to measure' (Pelto & Pelto 1978: 33, in Sanjek 1990: 394). Validity lies at the core of evaluating ethnography (Sanjek 1990: 394-395).

This move to advance methodology comes as no surprise when we realize that less than fifty years ago, Margaret Mead was willing to assert that one key informant could be a valid sample of culture (Hackenberg 1993: 13). The response therefore, has been and continues to be, the effort to develop better methods for making accurate observations and written representations. It is beyond the scope of this paper to address larger refinements in anthropological methods, from quantitative hypothetico-deductive and comparative strategies to grounded methodology and team research. For all of these, as well as the more restrained interpretive approaches, ultimately depend on ethnographic validity. Without valid representations there can be no meaningful categories, no probable generalizations, no insightful interpretations, and no effective comparisons. To this end, Roger Sanjek offers invaluable advice for confronting the difficulties of observer bias while maintaining a concern with validity. Sanjek proposes that contemporary ethnography requires the explicit presentation of three aspects of research: theoretical candor, a statement of the ethnographer's path, and fieldnote evidence (1990: 395).

The first requirement, theoretical candor, recognizes the political, academic, and personal baggage which influences a researcher's observations and descriptions by shaping the choices he or she makes throughout the course of field work and writing. The ethnographer's path describes the field worker's travels, emphasizing the limitations and opportunities that particular locations and personal contacts had upon ethnographic observations. Finally, fieldnote evidence provides the raw data, and exposes, "the relationship between field notes and the ethnography" (Sanjek 1990: 395-401). These three reflexive elements represent a new *form* for achieving old scientific goals. Unless one is truly resigned to extreme versions of postmodernism, Sanjek's complete article is essential reading.

It is important to emphasize that the effort to make valid observations and descriptions is not confined to material questions. Similarly, both emic and etic realms are amenable to empirical research strategies. To simplify for the sake of explanation, a push for ethnographic validity only means that we attempt to sort the exceptional from the common. Both remain interesting, but we treat the common as probable and address the exceptional separately, as problematic. *We do not ignore the particular*, but we do not speculate wildly about it's significance either. Instead, we redouble our efforts to obtain relevant data and proceed cautiously.

In relation to ethnographic validity, Clifford Geertz represents something of an enigma, and it is useful to explore his position as it is a last bastion before the wholesale leap into subjective relativity. Geertz has repeatedly denied that interpretive anthropology is not a science. He bases his position on the public status of symbolic meaning, and states that, "Whatever, or wherever, symbol systems 'in their own terms' may be, we gain empirical access to them by inspecting events, not by arranging abstracted entities into unified patterns" (1973: 12,17). It is here that Geertz has separated himself from the idealist strategies that formally began with Levi-Strauss, and instead, chooses to associate his own brand of interpretation with a kind of empiricism, and therefore, validity. But Geertz recognizes that his approach, "raises some serious problems of verification" (1973: 16). In fact, Geertz' critics are often his own intellectual progeny, who take him to task for not carrying the supposed relativity of interpretation to its logical conclusion. But while it is at least possible for interpretive anthropologists to utilize rigorous methods for observation, analysis/interpretation, and representation, Geertz chooses to "appraise" the value of a description, "against the power of the scientific imagination to bring us into touch with the lives of strangers" (1973: 16). As is demonstrated by Shankman, "the criteria for assessment are not clearly defined" (1984: 263). To understand why Geertz might not demand more rigor in interpretive research, I am inclined to take his word for it that he does not believe it is worth the effort, "to go round the world to count cats in Zanzibar" (1973: 16). Yet, as is demonstrated by Sanjek's three requirements for more valid ethnography, counting noses is not necessarily one of them.

All this argumentation would remain quite inconsequential if it weren't for the extreme implications of the deification of relativity and its direct attack on positivism. No one expresses this more clearly than Marvin Harris at his most reactionary. In a response to Paul Feyerabend's condemnation of science, Harris responds with equal, if not greater venom:

> As long as Feyerabend deals with mountains on the moon or quantum mechanics, his views cannot inflict too much damage. But there are other domains of knowledge in which epistemological relativism poses a grave threat to our survival. Medicine is one such domain, and there are many others in the social sciences. One cannot remain indifferent to whether cancer is caused by witchcraft or some defect in cellular chemistry. Nor can one let unbridled imagination determine the causes of poverty, or establish the existence or nonexistence of a ruling class in the United States. It cannot be a matter of taste whether you believe or do not believe that pollution is a menace, that the underdeveloped countries are getting poorer, that the multinationals are promoting a nuclear arms race, that war is instinctual, that women and blacks are inferior, or that the green revolution is a hoax. Let Feyerabend stand before the ovens of Dachau or the ditch at Mylai and say that our scientific understanding of sociocultural systems is ultimately nothing but an 'aesthetic judgment' (1980: 23)

In another, more restrained statement, Harris asserts:

> In order to do anything that can be called ethical or moral, people need to know objectively who did what to whom. Who fired the gun and who got hit by the bullet are questions that cannot be left to the imagination. It may be that one side is right about the objective facts; or that both are right; or that both are wrong. But without

> objective facts it is impossible to claim that you are doing the right thing (1995: 73).

And so we have come at last to applied anthropology, and the third adaptation to the interpretive critique and the assumed failure of positivism. Because anthropology is so entwined in the lives of people, it has been less successful than other sciences at distancing itself from the real world effects of its theories and practices. After years of scientism, elitism, and paternalism, we have now entered an age when, "governments are giving cultural traditions and religious beliefs higher priority than scientific inquiry" (Morell 1995: 1424). And while the positivists and the humanists have continued their debate, the world has transformed and the traditional subjects of ethnographic research are now doing and reading the work themselves (Harrison 1991; Wolf 1992). Racism, classism, nationalism, and the rest of societies violent' divisions are more visible, if not more prevalent, than ever before. From an intellectual standpoint, our conceptual progression from description, to representation, to power, has also pointed towards the relationship between thoughts and deeds, to "praxis," in our academic double-speak (Ortner 1984). In addition, the efforts of politically motivated anthropologists, particularly feminists, who have led the way in exposing the subtleties of power in its many forms, have also been confronted with the paradox of representation, and the realization that change and improvement are impossible without some recourse to a shared empirical reality (Wolf 1992: Leonardo 1991).

Finally, with the nearly incomprehensible transformations of global economics and electronic information networks, combined with the emergence of an illusive ethnoscape, the possibility of a science of man that in any way resembles the older vision, is appearing more and more unlikely. In particular, and in spite of its contemporary celebrity, ethnography is especially vulnerable to dissolution as it becomes increasingly incapable of addressing complex global issues. Faced with all these considerations, and with equal commitments to a variety of moral agendas, as well as to the practical economic realities which make a career in academia improbable at best, many anthropologists are looking to applied issues as an all purpose solution.

Roy Rappaport writes of this general turn in science:

> . . . even if scientists could be detached, others use their results not only for acting in but for transforming the world in which scientists, among others, live...Whereas modern science has attempted to develop 'theory,' that is, detached intellectual understandings derived from 'objective' or 'outside' knowledge of particular constituents of the world, leaving 'praxis' to farmers, carpenters, engineers, priests, or politicians, a postmodern science, recognizing that participation in the world it observes is inescapable, will incorporate into itself considerations of practice (Rappaport 1994: 163).

Applied anthropologists confront the critique of science and the futility of relativism by employing practicality and pragmatism as their methodological, epistemological, and ethical frame. Their work is by definition contextual and particular, avoiding the pitfalls of grand generalizations, while simultaneously utilizing bits and pieces of traditional science as they deem appropriate to the specific case. They are eclecticists and "bricoleurs," and as Harris predicts about such an approach, their contributions to general theory are minimal (Harris 1980: 287-299). Yet, as Barth notes, "the cutting edges or our theories can often be well tested by their relevance and power in practical matters" (Barth 1994: 350). Regardless of the theoretical implications of this kind of work, science and its premium on validity are absolute necessities in applied anthropology. In a field where practitioners regularly face competing explanations and interests, the ability to sort the wheat from the chaff is paramount.

On the other hand, science in applied anthropology is limited by the same practical constraints which empower it. From restricted budgets and time lines, to the slippery problems of advocacy, the specter of misrepresentation and subjectivity looms as large as ever.

Conclusion

The application of the scientific method within anthropology does not lead to understanding things as they are in some primordial, natural state, but rather, it results in an ability to decipher phenomena at the limits of our conceptual categories. The scientific method allows human beings to understand their world *as they create it*. While some phenomena, such as death, are less ambiguous or constructed than others, social research is necessarily predicated on the massive distortion, reduction, and abstraction of extremely complex processes in uncontrolled environments. The kinds of distortions which researchers impose upon nature through the formulation of their conceptual frameworks and operationalized categories are indistinguishable from the imagined world of objective essences. This amounts to a tautology: we cannot conceive of that which is beyond our conception. We may divide phenomena into a multitude of categories as they suit a given problem, but it is the problem itself which forms the limits of our world view. The meaning of any research project is therefore inseparable from the ideology which defines it. Consequently, it is impossible to divorce science from politics. This realization directs many contemporary researchers towards integration, rather than separation, of ethics and science.

Regardless of our specific interests in the ideographic or the nomothetic, in webs of belief, material infrastructure, masks of power, means of production, global economics, religious nuance, local politics, and so on, valid description, that is, probabilistic description, provides the means by which our research is transformed from individual opinion to shared understanding. Validity is a bridge, not always a comfortable or safe one mind you, but a bridge nonetheless. By sorting disparate, often opposing views, through the criteria of probability, the quest for valid representation guards against the pitfalls of subjectivity and denial, while also offering the opportunity for an understanding that unites divergent experiences.

Notes

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2. Apart from the position of some extreme post-modernists, non-positivist approaches to anthropology still require the use of classifications in their writings; symbols and symbol systems, for example, are at least theoretically meaningful categories. Andrew Vayda writes, "Not only, as is commonly recognized, do generalizations need to be supported by data from case studies but also, as is *not* so commonly recognized, the analysis and explanation of actions and consequences in the case studies require the use of generalizations" (1986: 307).

3. In considering the epistemological pedigree of historical anthropology, the Comeroff's write of structural-functionalism, "for all its ostensible concern with the nomothetic, it came increasingly to rest, as we said earlier, on an empirical scaffolding of life histories, case studies, social dramas of interpersonal conflict, and the like" (1992: 25).

4. This apt phrase comes from Dr. Robert Hackenberg at the University of Colorado, Boulder.

5. Clifford's four types of ethnographic authority are: experiential, interpretive, polyphonic, and dialogic.

6. While scientists are generally willing to entertain the value of interpretation and critique, the more extreme individuals in the non-positivist opposition (primarily post-modernists), deny the possibility of any science at all (Harris 1980: 21-22; 19; Carneiro 1995: 7,8). Edward Bruner suggests that, "what most precipitates polarization in this controversy is the feeling that your subdiscipline or field of anthropological inquiry is under attack. If someone defines the field in such a way that your own work is denied legitimacy or even worse, left out, then it understandably initiates reactions" (1990: 28).

7. With so many contemporary writers invoking new developments in physics and mathematics as justification for their polemic positions on these issues, I cannot help but observe that the most general perspective provided by these new types of understanding is that we find patterns within chaos and chaos within patterns.

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