EPISTEMOLOGICAL IMPLICATIONS OF ANTHROPOLOGICAL FIELD WORK, WITH NOTES FROM NORTHERN ONTARIO[1]

Edward J. Hedican
University of Guelph

Abstract: This article engages in a critical discussion of the epistemological implications—how we know what we know issues—of the interpretive approach in recent social anthropology. Primary among these issues is the role of paradigms, or the lack of them as the postmodernists suggest, in anthropological field work and how these paradigms might be made to be more commensurate with one another. The problems of methodological verification and subjectivity—ethnography from whose point of view?—are related aspects of cross-cultural interpretation. The central point is that the main epistemological issues in anthropology are intrinsically tied to field work. Our field-work activity is the basis of all debate, it is at the centre of the interpretive endeavour and is the final arbiter about methodological issues. Some examples from the author's field work among the Ojibwa (Anishenabe First Nation) of northern Ontario are utilized to illustrate issues raised in the article.

Résumé: Cet article entame une discussion critique des implications épistémologiques – comment nous savons ce que nous savons – de l'approche donnant une interprétation telle qu'elle est utilisée depuis peu en anthropologie sociale. Dans ces questions nous trouvons premièrement le rôle des paradigmes, ou leur absence comme le suggèrent les postmodernistes, dans le cadre du travail anthropologique sur le terrain et comment ces paradigmes peuvent être établis pour être à la mesure l'un de l'autre. Les problèmes de vérification et de subjectivité méthodologique – l'ethnographie, mais de quel point de vue? – sont des aspects similaries de l'interprétation inter-culturelle. Le point central de cette discussion montre que les principales questions épistémologiques en anthropologie sont dans leur essence liées au travail sur le terrain. Notre activité est à la source même de tout le débat, elle est au centre de nos efforts d'interprétation et enfin c'est elle qui est l'ultime arbitre en ce qui concerne les questions de méthodologie. L'auteur utilise quelques exemples

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de son travail sur le terrain parmi les Ojibwa (Anishenabe First Nation) du nord de l'Ontario pour illustrer les questions posées dans cet article.

Introduction

A cartoon in the *Far Side* series shows several Natives scurrying about inside a grass hut. One clutches a stereo under one arm and a television set under the other looking for a place to hide them, while another looks out the door and exclaims: "Anthropologists! Anthropologists!" At one level the cartoon is humorous because the image of the anthropologist dressed in a pith hat, shorts and safari jacket, accompanied by an entourage of Native carriers, confirms in the public's mind a view of anthropology as esoteric, anachronistic and elitist. For the anthropologists themselves there is a not-so-subtle message that anthropology has a distorted sense of reality such that aboriginal peoples are kept in a permanent state of preservation—a sort of cultural deep-freeze, in which the initial colonial encounter is seen to be kept intact.

There is a ring of truth to all of this, since it is only in recent times that anthropologists have shown any interest in contemporary Native peoples, and the impacts that colonialism, racism and imperialism have had on them. Since anthropologists gather information on their subjects of interest mainly on the basis of first-hand experience through field research, then the interrelationship between field work and the creation of knowledge in anthropology is a matter of some epistemological interest. For, as Ulin (1984:xi) rightly indicates, "Fieldwork or participant observation has led many anthropologists to struggle with epistemological problems related to understanding other cultures as part of a dialectical process of self-understanding." It might have been correctly said for the recent past that anthropologists in their writings have not shown any great interest in the epistemological issues raised by field research, but this is no longer the case today. In fact ethnography, especially its depiction of the point of view of the "Other," is at the centre of discussion in contemporary anthropology.

Anthropologists are currently engaged in a debate of considerable importance to the future of the discipline. The discussions and controversies are multifaceted ones, but at the centre of it all is a call for a distinct break with the past—the postmodern call for the deconstruction of existing paradigms and old-order authorities. It is a matter of what has been called a "crisis of representation," which is to say, "the explicit discourse that reflects on the doing and writing of ethnography" (Marcus and Fischer 1986:16). The challenge is to "all those views of reality in social thought which prematurely overlook or reduce cultural diversity for the sake of the capacity to generalize or to affirm universal values" (ibid.:33). It is issues such as these that we should all be thinking about, and for this reason the purpose of the present article is to engage in a critical discussion of the epistemological implications of the post-

modernist tendency in current anthropology. The central argument of this article is that the postmodernist call for the deconstruction of existing paradigms is not a useful approach to the difficulties of interpretation in anthropology because the centrality of field work is ultimately replaced by an emphasis on literary skills and "constructed truths."

The "Interpretive Quest" of Ethnography

It all began innocently enough with Clifford Geertz's article called "Thick Description: Toward an Interpretive Theory of Culture" (1973). In its essential form, interpretive anthropology explores the ways human beings assign meaning to their lives. It is this "meaning-seeking" feature which is the distinguishing characteristic of human beings. As Geertz explains later, in his book *Local Knowledge*, "the interpretive theory of culture represents an attempt to come to terms with the diversity of the ways human beings construct their lives in the act of leading them" (1983:16). Thus, to those who adopt this theoretical program, "the aim of anthropology is the enlargement of the universe of human discourse" (Geertz 1973:14).

As Geertz explains it, "our consciousness is shaped at least as much by how things supposedly look to others, somewhere else in the lifeline of the world, as by how they look here, where we are, now to us" (1983:9). Probably it is the theoretical implications of Geertz's interpretive approach which have caused the most concern. Is ethnography to become a matter of deeper and deeper introspection, or will it involve a wide basis of comparison and generalization? "To an ethnographer," Geertz (1983:4) explains, "the shapes of knowledge are always ineluctably local, indivisible from their instruments and encasements." The suggestion for the interpretive theory of culture is rather distinctive, in that it presupposes that "the essential task of theory building . . . [is] not to generalize across cases but to generalize within them" (Geertz 1973:26). Thus, anthropology necessarily becomes "imprisoned in the immediacy of its own detail" (ibid.:24).

It is this point, about the "immediacy of detail" which, I believe, is most responsible for a parting of the way among anthropologists regarding the interpretive approach. It is all about the BIG question for anthropology, which Geertz poses in an incisive manner, to wit: "The great natural variation of cultural forms is, of course, not only anthropology's great (and wasting) resource, but the ground of its deepest theoretical dilemma: how is such variation to be squared with the biological unity of the human species?" (1973:22). The fundamental problematic in anthropology is therefore the simultaneous notion of cultural uniqueness and underlying similarity of *Homo sapiens*.

Geertz "grows uncomfortable," he says, "when I get too far away from the immediacies of social life" (1973:vii). Yet, there are no doubt many anthropologists who feel that this "deepest theoretical dilemma" cannot be resolved

unless we move beyond the immediacy of local detail. It is all probably a matter, as much as anything else, of how one broaches the question, the sorts of training and experiences one has, and whether or not one is predisposed to looking at humanity in a micro- or macro-perspective. As Scholte (1972:438) has suggested, "The ethnographic situation is defined not only by the native society in question but also by the ethnological tradition 'in the head' of the ethnographer."

The sorts of "ethnological traditions" that anthropologists usually have in mind revolve around the question, as phrased in one recent contribution to the discussion, "Is Anthropology Art or Science?" (Carrithers 1990). The essential concern is this: "How are we to represent anthropology as a serious activity to ourselves and to those with whom we are engaged if it is so nebulous" (Carrithers 1990:263). Anthropology could be made less "nebulous," in the view of some, if it became more rigorous in a methodological sense. As Renner (1984:540) explains, "the absence of an empirically convincing theory and methodology has as its consequence the fact that there can be no program for the direction in which research should proceed." It is not a matter of whether or not Geertz's anthropology fails to account for the causes of human thought and behaviour: "Instead, the most significant deficiency . . . is its lack of explicit theoretical and methodological guidelines" (Lett 1987:117).

The problem for many has to do with the belief that the interpretive and scientific approaches are mutually exclusive. This belief prompts the concern, as Shankman (1984:261) suggests, that "the programmatic side of Geertz's work is an attempt to refocus anthropology—indeed all of social science—away from the emulation of the natural sciences and toward a reintegration with the humanities." Similarly, Scholte (1984:542) states that interpretive anthropology "draws its inspiration from the arts and the humanities rather than from the natural sciences." The last word on this matter goes to Geertz himself who clearly has had enough of these repeated attempts to cast the interpretive approach as unscientific. As Geertz explains (1990:274) in his recent comments concerning the article, "Is Anthropology Art or Science?"—"I do not believe that anthropology is not or cannot be a science, that ethnographies are novels, poems, dreams, or visions, that the reliability of anthropological knowledge is of secondary interest, or that the value of anthropological works inhere solely in their persuasiveness."

It is important to point out, though, that numbered among Geertz's supporters are those anthropologists who hold that anthropology should become more explicitly a humanistic discipline. For those who would argue this case, the suggestion is that interpretive anthropology is an admirable alternative to "the reductionism and ethnocentrism of traditional science" (Scholte 1984:542). Ridington (1988) makes a similar suggestion in his study of the Athapaskan (or Dene) phenomenology of knowledge and power. He argues that long-term

field work is a crucial aspect of understanding this phenomenology, since "the careless and uncritical application of ideas from academic traditions to the thoughtworlds of subarctic people may produce bizarre and ethnocentric results" (1988:98). As an example, Ridington points to the "uninformed ethnocentrism" which dominated much of the debate about the causes of the "windigo psychosis" among eastern subarctic Indians.

The next stage in the evolution of the debate about interpretive anthropology concerns the suggestion, by Marcus and Fischer (1986), that anthropology should become a vehicle for cultural critique. The goal here is one of "pushing contemporary interpretive anthropology toward a more politically and historically sensitive critical anthropology" (Marcus and Fischer 1986:xii). It is here that the controversy takes on a more serious tone.

The view of contemporary anthropology becoming "more politically and historically sensitive" would certainly receive a wide basis of support among anthropologists. As Ulin explains, "a thorough historical and political understanding under which epistemology in relation to field work takes place is, therefore, an important part of cross-cultural understanding" (1984:22). Where anthropologists differ has to do with the suggestion that the extreme relativistic stance of the "interpretive-critical" position has precluded attempts to develop these wider political and historical issues. Spencer (1989:145), for example, suggests that "despite its trappings of political and intellectual radicalism, it is in some of its presuppositions a depressingly reactionary document." Another related work of the postmodernist genre, *Writing Culture* (Clifford and Marcus 1986), draws similar criticism about the shallowness of the interpretive approach, such as, "the tendency to read texts with little or no consideration for the social or historical context in which they were written seems an especially barren approach" (Spencer 1989:145).

It is the concept of cultural relativism which provides the basis for the antitheoretical posturing of the interpretive approach. The tacit assumption is that anthropology should have a political mission, such that ethnography would document cultural differences with a view to encouraging a more tolerant Western society. It is for this reason, as Marcus and Fischer (1986:141) explain, that "a major task of the epistemological critique offered by anthropology is to deal directly and in novel ways with the materialist or utilitarian bias of Western thought in explanations of social life."

It is pronouncements such as these that must make the cultural materialists seethe with apoplectic shivers. But beyond this, critics of the new relativism have argued that emancipatory perspectives in anthropology presuppose the existence of some commonality of experience, even if it is only common suffering at the hands of the same economic oppressor, and subservience or resistance against the same economic forces, a presupposition which either subverts or is subverted by arguments of extreme cultural difference. "Marcus

and Fischer seem unaware," Barak (1988: 101) explains, "that the self-determination and emancipation of anthropology's subject peoples is as much (if not more) contingent upon the recognition of their common experiences as of their differences. But this perspective would necessitate a totalizing vision of world history which the authors are loathe to entertain." In fact the organizational efforts of aboriginal peoples, such as the internationally-based "World Council of Indigenous Peoples" comprising Maoris, Samis, North American Native peoples, as well as others, was founded on the commonalities of the colonial experience, as aptly illustrated by one of its founders in *The Fourth World* (Manuel and Poslums 1974).

In summary, the interpretive approach in anthropology has been the subject of considerable discussion and debate in recent years. On the one side are those who are in favour of the relativistic underpinnings of this approach. Their argument is that "traditional science" ignores important cultural differences, and has inherent ethnocentric tendencies. However, in terms of the volume of literature at least, interpretive anthropology has also been the subject of much criticism. Most of this criticism centres about methodological concerns—the approach is too nebulous, it lacks explicit guidelines, and its theoretical implications are not discussed in sufficient depth. But it has been the later manifestations of this approach, in terms of Marcus and Fischer's cultural critique, that has drawn the most severe criticism.

Taken as a whole, what discussion of the interpretive approach has done is to re-open many long-standing issues in anthropology, such as, whether anthropology is to be humanistic or scientific, subjective or objective, paradigmatic or non-paradigmatic, inductive or deductive, etc. The point here is that debate about interpretive anthropology is not simply a recent phenomenon stirred up by Clifford Geertz and his cohorts, but is about a diversity of unresolved epistemological issues in the discipline. Discussion about field work continues to be a central focus in these debates, probably because so much of anthropology's validity depends on the interpretive process of cross-cultural data gathering and analysis. As such it is incumbent on anthropologists to continue these discussions about field work and the basic epistemological foundation of anthropology. Much of this task involves what could be called reflexive understanding, or, in Geertz's terms, "an attempt somehow to understand how it is we understand understandings not our own" (1983:5).

Reflexive Understanding in Field Work: Northern Ontario

There are those anthropologists who might be excused perhaps for feeling that field work does not always leave them full of knowledge, but maybe only a little less ignorant. Field work might be likened to a knock to the side of the head. Our view of the world becomes altered in some fundamental way that is difficult to describe or articulate. We might try to bury our apprehensions

about what we have gone through, but we nonetheless realize that our experiences have left our view of the world a bit off centre. Our vision is now somehow permanently askew, so that we are much less trusting that our previous, comfortably held perceptions should act as a reliable guide. We now tend to look at the world somewhat obliquely, wondering all along whether that particular reality open to us at that particular moment might skip a notch. It is like chasing a ball through the air in the summertime when all of a sudden our eyes make direct contact with the sun's brilliance. Startled and dazed we stumble about, trying to figure out where we are and what has happened.

Field work also has a habit of leaving a permanent record of discontinuous events in our subconscious that keep bubbling to the surface whenever we let our guard down. We could be shaving or driving to work when all of a sudden there is a direct recall of some event or situation which happened years ago that now, for some unknown reason, requires a thorough thinking through. Of course we cannot give it the attention it needs and so, as with a cranky child in the supermarket, we tell it to be quiet for the time being and otherwise try to muddle our way through. A curious thing about these flashbacks or recalls of field-work situations—they do not seem to be the same sort of pressing concerns that we had when we were actually conducting the field work. They are the types of events that we did not pay a whole lot of attention to at the time because they did not seem that important.

Is this the price of field work that anthropologists have to pay in order to make their descriptions and account of other cultures believable? If it is, then it is a heavy burden indeed. We are left as not only the forlorn "strangers in a strange land," but strangers unto ourselves. The selves that we call "me" begin to divide and then dissolve into a large entity. Anthropologists are thought to be the oddballs among social scientists, and at times might even be expected to be so. Some of us even play up to that perception, donned in safari jacket and khaki shorts, but what is more important than these outward appearances is what goes on under the pith hat.

Here is an example of the sort of situation that is encountered by the field worker. The scene is a small log cabin village of Ojibwa Indians in the far reaches of northern Ontario (Hedican 1986). There are no cars or roads or televisions here—just the people and the all-encompassing jack pine forest. My own cabin was a mere 12-feet square but nonetheless comfortable enough. I had a small desk in one corner accompanied by an overturned garbage can with a pillow on top, which served as a seat for my visitors. There was also a bench near the door so that at first people who happened to feel a bit nervous about visiting could keep their distance. Most everyone eventually moved over beside the desk because the candles and coal oil lamp made it hard to see even at 12 feet. The can beside the desk left us "cheek by jowl," and the people

would bend over farther when they thought that they had something important to say.

This cabin was the place where much of the information about village life was gathered, as just about everyone showed up beside the desk at one time or another, often spending long hours recounting their memories, perceptions, frustrations and insights. At times it all seemed a jumble of field work, entertainment and relaxation to fill in the long winter evenings. Late at night the flickering lamp made the shadows and shapes flow back and forth as if they were made of liquid matter. On one occasion a middle-aged man grew serious as he pointed to the corner of the cabin where the wood stove was and asked if I had seen it yet. The bottom of the stove had begun to rot out, and the glowing embers inside cast eerie twinkles of light across the ceiling. "What?" I asked. "You know, the cheebuy (ghost)," he said.

The cheebuy was apparently that of Ed Pidgeon, the one-armed former occupant of the cabin. Now he had made me nervous, because while I had to admit that Ed Pidgeon's apparition had not revealed itself to me, I also realized that through the long winter months I had to spend many hours alone in this cabin, and one never knows what tricks the mind is apt to play as we work our way through the manifestations, real or imagined, of the so-called culture shock experience. This cabin and the people who came and went were part of my life nearly 20 years ago, yet the scene remains with me, sometimes in vivid detail. The cabin itself has long been torn down, the result, it is said, of the drowning of Sogo, a later owner. The cabin had to go because they did not want his ghost wandering about the village. Without the cabin, the reasoning went, Sogo's apparition would not have a place to settle down and would move elsewhere.

It would not be true to say that events such as these have had any sort of permanent impact on my psyche as an anthropologist, but it is the cumulative nature of such happenings that have a subtle moulding effect. They are a touchstone to an alternate reality that is after all these years still only poorly conceived and apprehended on my part. It is as if in the beginning of our field work we are not allowed a true glimpse of the magnitude and scope of the reality enveloping us. In any event we are usually too naïve, young and immature to fully appreciate what is going on, so our mind's eye secretly files various occurrences away for safekeeping as it were, with the possibility that at some later stage of our development we might be in a position to make a more profound sense of these happenings. So these little snippets of time are hauled out on occasion and presented to us for some sort of closer scrutiny and analysis.

The trouble with all of this is that we are now years down the road, and the accuracy of our recollections, even with the aid of written field notes, logic tells us, should be regarded with some degree of scepticism. We have a seemingly clear grasp of the detail of some events, but other aspects have been for-

gotten altogether. What this means is that we are faced with the task of trying to reconstruct the reality of the original field work, and all the other "realities" that have emerged over the years as we reflect on our experiences and what they mean in some wider, objective sense. How merry, we are led to think, must be the life of the logical positivist for whom the content of observation tends to be free of conceptual contamination. It is no wonder that Nietzsche called this "the dogma of immaculate perception."

To give Geertz and his interpretive brand of ethnography its due, there is a considerable problem with the "immediacy of detail" in field work, and the way that anthropologists come to the sorts of understandings and explanations that they do. One facet of the problem is what Barrett has referred to as "the illusion of simplicity," which is to say,

> The interplay between the contradictory nature of social life and the mechanisms that conceal it indicates the vast complexity in the midst of which our lives unfold. . . . But anthropologists . . . spend their lives trying to prove that order exists. This mistake is not restricted to anthropologists or to their analysis of primitive society. It is probably intrinsically related to the attempt to establish a positivistic science of society. (1984:195)

It is no doubt true that many anthropologists perceive of themselves as "doing science." However, science can be thought of in terms of a broad range of scholarly activity. For, as Pelto (1970:30) indicates, "No sharp lines can be drawn to differentiate the so-called hard sciences from other disciplines . . . somewhere in the middle of this conceptual domain is the matter of methodological verification—the sets of rules whereby useful knowledge can be accumulated and pyramided into a more powerful understanding of the universe."

It is this issue of "methodological verification" that has become the focus of controversy concerning the believability of anthropological research and the basis on which anthropologists accept generalizations of human behaviour. There are several well-known examples of theoretical debate about this issue in the anthropological literature. The surprisingly large area of disagreement between Goodenough (1956) and Fischer (1958) concerning the classification of residence patterns on the Island of Truk is one of the more prominent instances. Using census data Fischer concludes that there is a matrilocal tendency; Goodenough derives a somewhat different interpretation from indigenous decision-making models. What is peculiar about this case is that anthropologists thought that they had clear-cut definitions of the various residence patterns, so it would appear to be a rather simple matter of tallying up the number of different cases of each.

The lesson is that even when there exists some degree of consensus among anthropologists concerning problems of definition, the subjective interpretations by the anthropologists about what patterns he or she actually sees in the field is a matter of some variation. The Oscar Lewis-Robert Redfield contro-

versy about life in the Mexican village of Tepoztlan is an even more poignant case of the problem of subjective interpretation. Redfield (1930) had originally studied the community in the 1920s and wrote about a harmonious village life. In a subsequent restudy of the same community, Lewis (1951) reported quite different conclusions, emphasizing "the underlying individuality of Tepoztecan institutions and character, the lack of cooperation, the tensions between villages within the municipio, the schisms within the village, and the pervading quality of fear, envy and distrust" (Lewis 1951:429). However, to the extent that we can regard Redfield's account as documenting the ideal or formal belief system of Tepoztlan, and Lewis as describing the actual system, it is possible for us to view the ideal and actual system of beliefs as complementary, rather than opposing, modes of analysis.

In sum, the construction of ethnography is largely a matter of organizing our "reflexive understandings" of the field-work experience. It is a process fraught with difficulties of interpretation as we attempt to grapple with the accumulation of "realities"—ours and that of the "other"—that have built up over time. The fact that we are able to provide a plausible account of this experience is perhaps a minor miracle in itself. Our success depends pretty much on how we are able to organize our understandings. We group them together in various ways, by discussing issues and problems, and thereby building up larger spheres or facets of the account we seek to portray. Thus, this process of constructive understanding becomes central to the problem of verification in field work. The debate about residence patterns on Truk, or the portrayal of Mexican village life highlights the possibility, even probability, that quite different accounts of the same "reality" can be expected in anthropology. However, it would be a mistake to conclude that ethnographies are by their very nature unreliable documents because of the matter of methodological verification. It is true that Lewis and Redfield provided different accounts of Tepoztlan, but these accounts enlarge our understanding of the whole because they are largely complementary to one another. Complementary understandings raise a central epistemological question in anthropology—the matter of paradigmatic commensurability.

Paradigmatic Commensurability: Fact or Fiction?

One facet of the problem called the "crisis of representation" is a certain postmodern tendency to challenge the authority of all the older paradigms of social science. Marcus and Fischer, for example, are critical of any form of large-scale theorizing for anthropology. Instead, they propose an interpretive anthropology that is critical of "a persistent tendency to drag all discussions back to the classic work of the first generation of modern fieldworkers" (1986:viii). What they advocate could be termed "radical inductivism," which is to say, "the process of reconstructing the edifices of anthropological

theory from the bottom up" (ibid.:ix). In addition, a further facet of this postmodern approach is the claim that "Interpretive social scientists have recently come to view good ethnographies as 'true fictions' " (Clifford and Marcus 1986:6). While there is much hidden in the claim that all truths are constructed, the excesses of the postmodern trend are made evident by the further claim that "all constructed truths are made possible by powerful 'lies' of exclusion and rhetoric" (ibid.:7).

Granted, despite our best intentions and preparations the world has a tendency not to appear as we intend or would like, but at times as a potpourri of incongruous situations and events. We strive in various ways to grasp the threads of understanding that make such situations intelligible. We interview, we participate, we observe and we formulate hypothetical constructs. There is a tendency, the more our comprehension and skill are strained, to delve more into the intricacies of methodology. We begin to think that the big barrier is a methodological one, that if only we had a more appropriate or more sophisticated methodological approach our problems of comprehension would be solved. This is what Kaplan (1964:24) has called the "myth of methodology," which is to say, "the notion that the most serious difficulties which confront behavioral science are 'methodological,' and if only we hit upon the right methodology, progress will be rapid and sure."

As such, the problem for the ethnographer is less a methodological one, that is, in the more narrow sense of various research techniques; rather it is the bigger issue of the sorts of models, paradigms and theoretical orientations that are brought to the interpretation of field-work data. However, one is also struck by the apparent tendency for some anthropologists to avoid the task of articulating their assumptions and underlying analytical constructs. All too often ethnographers are timid about revealing the theoretical perspective which informs their writing, and the reader is left to extract it from the text.

While we might not always admit it, anthropologists probably also do hold their own personal views about the general characteristics of human society, and about the role of human beings in the natural scheme of things. After all, it is impossible to conduct meaningful field work without some sort of guideline or orientation, no matter how dimly recognized or articulated. The problem is that theoretical positions are often presented as polar opposites. We do not feel comfortable with either of the extremes, but nonetheless develop a certain affinity with the main tenets of one side or the other.

If some anthropologists are apt to keep their theoretical perspectives well hidden, there are those who are aggressive in championing one cause or another. In such situations we might feel the pressure to make allies and choose sides. As Manners and Kaplan have aptly noted, in anthropology there is a tendency for theories to "function as ideologies, and the reaction to them is often in terms of their ideological rather than their scientific implications"

(1968:10). In such situations there is also a corresponding tendency, one could add, to avoid the question of paradigmatic commensurability in anthropology. In sum, as Lett (1987:132-33) indicates, "unfortunately, most anthropologists who have participated in the debates have failed to recognize the incommensurability of the paradigms involved."

These are noble sentiments, especially if they are meant to bring about a rapprochement between disputing parties such that harmonious relations might be re-established. But these sentiments are misguided because Lett himself emerges as a champion of Harris's cultural materialism and positivism, and a harsh critic of phenomenology. It is all a matter of "epistemological responsibility." Lett (1987:19) says, "the phenomenological argument is doomed." The conclusion is that phenomenology is not an epistemologically responsible position for anthropologists to hold.

It is a curious matter that the attacks on phenomenological approaches to sociocultural studies have been so virulent, especially given that ethnography could be regarded, in Goldstein's (1968:102) words, as "the model of phenomenological social science." In fact Goldstein's position is worth reconsidering, inasmuch as he finds that "far from opposing one another, the two approaches [phenomenological and naturalistic] are complementary and both of them necessary if we are to have a full account of the phenomena in question. Each does a different job, and there is no reason why we cannot have both" (1968:98). Goldstein articulates the reason for his position further by stating, "even naturalistic social science rests upon the phenomenological standpoint. Our problem, then,... is to sketch the purposes and domains of each and to pay attention to their possible points of contact" (1968:100). In the final analysis we are led to conclude that the naturalistic (positivistic) standpoint cannot be made entirely independent of the phenomenological.

The philosopher of science, Abraham Kaplan, reaches a similar conclusion in reviewing debates among colleagues, and noting, in a general sense, that "both positions have important contributions to make to methodology, and more is to be gained by treating them as supplementing one another than by pushing either to exclusion of the other" (1964:388). After all, what was eventually gained by the often acrimonious debate over the conflict-consensus issue? It just sort of fizzled away over the years under the overwhelming evidence that human societies are characterized, in varying degrees, by states of harmony and conflict. In a similar vein the same can be said to be true of the formalist-substantivist controversy in economic anthropology. In a somewhat exasperated tone, Salisbury noted in the *Annual Review of Anthropology* that "unfortunately, the debate has continued, but materials presented in strictly dialectical terms of substantivist-formalist have yielded little of value.... The two approaches need to be fused—we need total models of economic systems" (1973:85).

When we begin to take stock of the various debates over the years in the anthropological literature, a rather interesting pattern begins to emerge, which lends credence to the idea that there is probably not a multitude of small issues that is the problem, but several large ones. Consider this for a moment. Is not the position of the substantivists quite similar to that of the phenomenologist in that with both there is a tendency to restrict the scope of cross-cultural generalizations? On the other hand, the formalist emphasis in this debate on the universality and rationality of economic decision making across cultural boundaries has a decided positivistic bent to it.

The same might be said about whether one is predisposed to the use of an inductive or deductive model in anthropological field work. Barrett's (1976) work in a wealthy Nigerian village is an interesting test case of the utility of models in anthropological field research. Initially he was convinced that anthropology was in need of more rigorous approaches which suggested that the use of deductive models would have a beneficial effect. This approach, he points out, was originally thought to be more acceptable than the usual practice of carving out empirical generalizations and then comparing the findings rather haphazardly with other similar studies.

The question he began with was why one particular village had advanced economically, when others did not. Various explanations are then discussed, such as the personality of the leaders, the communal social organization and religious reasons. The conclusion, somewhat surprising given his initial assumptions, was that "the theoretical framework adopted prior to fieldwork resulted in a distorted view of reality, and only when this framework was discarded was it possible to solve the major research problems" (1976:161).

As a general rule anthropologists are not in the habit of using a deductive approach in field work. The more commonly utilized inductive procedure, however, has some serious consequences for field work in anthropology, the most important of which is that it tends to encourage the separation of research and theory. Effort is wasted on the collection of a wide range of data, most of which is eventually found to be superfluous to the empirical generalizations that are finally formulated. In addition, adopting an inductive approach means that one's research lacks the sense of direction that a hypothesis testing procedure engenders, and suffers from a lack of an initial "built-in" theoretical significance. In all, Barrett's Nigerian study is particularly enlightening because he is not only aware of the potential benefit of deductive paradigms for field work, but in the final analysis he nonetheless argues for the orthodox procedure characteristic of most social anthropology.

While not all anthropologists would agree with this conclusion (cf. Lett 1987:41-47) the enduring nature of the inductive approach in anthropology probably has less to do with the rejection of "science" than with the realization among anthropologists that long periods of field work in cultural settings

quite different from their own leave open too many relevant possibilities, most of which would be left out in the conventional hypothesis testing procedure. This does not mean of course that anthropologists are able to carry a "clean slate" to the field, or that they do not think about theories even in an implicit sense. What it does mean is that anthropology's unique position with regard to research material is not benefitted by presumption.

The overall problem with the deductive paradigm in anthropology is that an explanatory framework is forced upon the inquiry before the research even begins. The anthropologist is also led into making assumptions on the basis of existing theory and research that could be totally inappropriate for the problems that eventually emerge in the field study. Some would argue that this is not harmful because at least we have a starting point or guideline to begin with, but it is apt to become a major difficulty if the initial assumptions play a role in distorting the study in ways unknown to the researcher.

The sorts of paradigms that are used in anthropology is an issue without a ready solution. It might be suggested that the deductive-inductive debate is a misdirected one because the real problem has to do with the unsophisticated frameworks that are adopted by anthropologists, an uncertain knowledge of what constitutes a model and faulty applications of the deductive procedure. On the other hand, it may be that anthropology will never be so methodologically and theoretically "sophisticated" as to allow for the application of deductive paradigms.

In summation, we can say that the use of one particular paradigm or another in ethnography is probably a result of a multiplicity of factors, such as the researcher's previous training, personal inclinations and the characteristics of the field situation. Whether one subscribes to a phenomenological point of view or not, the researcher is always faced with the problem of how one apprehends the perceived "reality" of this field situation. We all realize that different points of view are possible, even probable, given the diversity of cultural settings in which anthropological research is conducted. Not only must ethnographers deal with the issue of the extent to which their perceptions coincide with the people in other cultures, there is the added facet of the researcher's changing viewpoints as material is evaluated, assessed and analyzed. Thus, what is at stake here is an important epistemological question about subjectivity in field work.

Relativism: From Whose Point of View?

The postmodernist ethnography of interpretive anthropology is characterized by the pursuit of a central goal which is to adequately portray the point of view of other cultures. Geertz (1983:56) articulates this problem of representation in the following way: "The issue is epistemological. If we are going to cling—as, in my opinion, we must—to the injunction to see things from the

native's point of view, where are we when we can no longer claim some unique form of psychological closeness, a sort of transcultural identification, with our subjects?" From Geertz's perspective culture is characterized primarily as a social phenomenon and as a shared system of intersubjective symbols and meanings.

For other writers of this genre, such as Marcus and Fischer, "the contemporary debate is about how an emergent postmodern world is to be represented as an object for social thought in its various disciplinary manifestations" (1986:vii). In a related work, *Writing Culture*, the hermeneutic philosophy of the interpretive approach is even more pronounced, such that "interpreters constantly construct themselves through the others they study" (Clifford and Marcus 1986:10). We are told that anthropology is in an "experimental moment" and, furthermore, that the solution to the problem of representation lies in creating new forms of ethnographic writing. It is all founded on the belief that "writing has emerged as central to what anthropologists do both in the field and thereafter" (Clifford and Marcus 1986:2).

The debate about how important writing is to ethnography takes off on a whole new tangent from here, prompting one critic (Spencer 1989) to refer to Geertz as that "American literary dandy." What is peculiar about the discussions in the literature is that they were initiated by Geertz's quite proper suggestion that we examine the portrayal of the Native's point of view as an epistemological issue. The issue is then recast as a problem of representation, and finally as a matter of literary form, all the while moving further away from Geertz's initial query.

There are several important concerns here. For one, if anthropological knowledge is to be seen as interpretive and hermeneutic, is it necessary to place so much emphasis on the relative spatial and temporal orientation of the author? Is knowledge to be regarded as forever tentative, rather than conclusive? Where are we being led by this "post-positivist" discourse? The suggestion, by Marcus and Fischer (1986:15), that we adopt a "jeweller's-eye view of the world" would appear to be a particularly myopic one when so many interesting epistemological issues are raised by the problem of representation. The issue of the relative importance of the researcher's subjective stance is of course central to the representation problem, suggesting that the focus of discussion return to this most basic of epistemological concerns for anthropology. We have been led astray by the postmodernist discourse. A return to a discussion of the inherent dilemmas of field work is therefore called for, founded on the view that the central problems in anthropology are intrinsically tied to field work. Our field work activity is the basis of all debate, epistemological or otherwise. Field work is at the centre of the anthropological endeavour; the final arbiter about methodological issues.

Field work by its very nature is an ambiguous experience for the anthropologist. Part of the ambiguity stems from the ethnographer's attempt to bridge the gap between objectivity and subjectivity in field work. The general lack of discussion of this problem in field studies has meant that anthropologists have become the subject of criticism on this account, such that "ethnographic accounts are by nature one-sided, although based on dyadic interaction" (Manyoni 1983:227). Whether attempts to reduce or change the "one-sidedness" of field work are possible, or even desirable, is a difficult question to answer. For example, there are those who have been critical of anthropology for not searching for that elusive "objective reality." As a proponent of this stance Lett (1987:19) suggests that "The problem of objective reality is a genuine problem in philosophy, but it is not, as the phenomenologists argue, a necessarily insoluble problem."

It is all a matter of "epistemological responsibility," Lett (1987:18-22) argues, that we reject phenomenology and, by implication, embrace the positivist approach. When the issue is in such terms, as a matter of polar opposites, we are unwittingly, and perhaps wrongly, persuaded by the view that other possibilities do not exist, or are not logically possible. Furthermore, we are led to ask why must an "objective" account—the "real" or "true" one—be provided by only one spectator: can there not exist a number of valid subjective accounts? This is a matter of what Kaplan (1964:128) refers to as "The methodological importance of what is called . . . intersubjectivity. A scientific observation could have been made by any other observer so situated: nature plays no favourites, but exposes herself promiscuously."

Much of the turning point for debate in anthropology centres on the methodological issue of how knowledge is created by the researcher in the field. However, when we discuss participant observation we tend to become apologetic, hoping that no one will seriously challenge what might appear to others to be an unsophisticated, crude and maybe even an inappropriate method of gathering information. When we talk about unstructured interviewing, we realize in our hearts that it requires considerable skill to conduct, and that it is probably our greatest aid in the field, yet we have difficulty in describing what we are doing in a manner that does not appear to be rambling, undisciplined and lacking purpose. Of course at times our methods do have some of these characteristics, which may not always be such a disadvantage, since it could leave open the possibility of serendipitous discovery. After all, Weber (1949: 115) once warned that methodology "can only bring us reflective understanding of the means which have demonstrated their value in practice by raising them to the level of explicit consciousness; it is no more the precondition of fruitful intellectual work than the knowledge of anatomy is the precondition for correct walking." And so it is with the task of anthropology; the cultivation of methodology is neither sufficient nor necessary for a successful anthro-

pological endeavour, since there is much more that goes into the making of a successful field trip than an armful of specific tools for eliciting information.

Indeed, the anthropologist's duty is not to make a fetish or myth out of methodology (see Kaplan 1964:128) but rather to reconsider basic epistemological issues concerning whose point of view, if any, is actually presented in ethnographic interpretations. Certainly one of the main lessons that ethnographic research has for anthropology is that the search for general statements of the structure of knowledge may be precluded by the very facts of cultural variation. There would appear to be few cross-cultural generalizations that could be made concerning such things as proper conduct, morality and, ultimately, truth. There is then an epistemological dilemma or contradiction in the task of anthropology. If our concern is with epistemology as the theory of knowledge, with the pursuit of basic questions concerning the search for truth, then we might have to be prepared for the development of a "culturally embedded" methodology that would be capable of dealing with the sorts of variations in points of view, in accepted traditions and "truths" that anthropologists have to deal with. What this all suggests is that there is some validity to Peter Winch's argument that it is not empirical verification that confirms what is in agreement with reality, but rather it is intersubjective communicative competence that constitutes reality in each social matrix. As he explains, "A man's social relations with his fellows are permeated with his ideas about reality. Indeed, 'permeated' is hardly a strong enough word: social relations are expressions of ideas about reality" (1958:23).

Conclusion

We started this article with Geertz's idea of society as a shared system of intersubjective symbols, and ended with Winch's notion of social relations as inherently communicative, intersubjective events with respect to their form. In the process we have been led back to the essential role of field work, since it conditions the anthropologist's world view in a general sense. What emerges from this view is an appreciation, even an affection, for the diversity of human cultures in the world—Geertz's "great (and wasting) resource." This diversity enters into the anthropological value position which maintains that human cultures are valuable by the mere fact of this diversity.

It is field work that is primarily responsible for the anthropological emphasis on the uniqueness of human cultures, and for the relativistic view that in its own way each human culture has a view of the world in its own terms. It follows then that field work poses certain epistemological problems for anthropology about how knowledge in a general sense is to be studied. It also follows that a central problem for anthropology is that field work tends to induce us into the belief that all knowledge is relative, or even further, that reliable knowledge is not possible.

It is field work which nonetheless poses broad, comparative questions, even when new orthodoxies tell us they are obsolete. It is field work which brings surprises, such as ghosts in a northern Ontario cabin. My field-work encounter with the Ojibwa man in my cabin, and our discussion about whether or not I was aware of Ed Pigeon's ghost near the stove, was a problem because I had not initially come to the community to study ghosts, religion or other such phenomena. My central concern at the time was with politics, leadership and economic development, so I did not pay particular attention to what the man was talking about. It was only later, when I began to reflect on my field-work experiences, that this ghost episode would creep into my consciousness. When it did so, I was belatedly forced to ponder some very fundamental issues concerning my field-work experience, such as: to what extent did our concepts of "ghost" coincide, if at all? what "message" was he actually trying to communicate to me concerning the ghost phenomenon? It was these sorts of questions that also posed problems for me later in my field work when I began to realize that the man was not just posing a rhetorical question about ghosts, as to whether or not I was able to see one, but that he was actually "seeing" it and wanted me to be party in some way to this experience. As the months went by, I began to realize, in ways that were not immediately obvious to me in the initial stages of the field work, that the people took the existence of "ghosts" as pretty much a routine matter, like dogs, trees and so on. For them it was sort of belabouring the obvious to have to point out to the anthropologist the existence of ghosts in my cabin. Over the years this experience academically haunted me—a piece of field-work flotsam that was not part of my research plan. The issues raised were issues of comparative epistemology—the translation of *cheebuy* as "ghost," Ojibwa belief in and experience of cheebuy, the rationality or irrationality of their "knowledge" of such phenomena.

Since the question of evidence is also central to the pursuit of epistemology, we are thus confronted with the challenges that our sources of knowledge brings to us from the field-work experience. All debates and controversies are ultimately reducible to the epistemological question about how do we know what we know? From the anthropological perspective there does not appear to exist any one single interpretation of phenomena that is any more valid or real than any other interpretation. In sum, the relative nature of the human experience means that the existence of an objective reality is a problem without a readily available solution. The postmodernist call for the deconstruction of existing paradigms does not appear to be a useful approach to this difficulty of interpretation, since it simply replaces one orthodoxy with another.

Note

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