

# Introduction

The social sciences are currently marked by substantial controversy within the disciplines. This controversy concerns fundamental questions such as: What are the relevant questions to be asked? Is there an objective reality "out there" that can be perceived and studied? What are the appropriate methodologies that should be used in studying social phenomena? On one side are those researchers and practitioners who see social phenomena as subject to systematic regularities and therefore discoverable through systematic procedures of data collection and analysis. On the other side are those who question whether "objective" or, more precisely, "intersubjective" study of social phenomena is possible, arguing that the appropriate role of researchers should be to engage in careful analysis of the interplay of the observer and the observed, and of how the understanding of cultural phenomena is generated by that interaction.

In anthropology, the current debate over appropriate epistemologies, theoretical perspectives, and approaches harks back to many previous divisions. Indeed, the discipline of anthropology still straddles the fence that exists between the humanities and social sciences.

The approach advocated in this volume is certainly that replicable and systematic procedures of data collection and analysis are a requirement for the building of useful cultural theory, cultural theory that is an aid both to understanding social phenomena and in changing existing social conditions. At the same time, it is our contention that conventional social scientific procedures—those exclusively relying on the social survey or on ethnographic techniques—are not as productive as is needed for building cultural theory. Rather, a theoretical and methodological approach that is eclectic in its orientation is likely to be more useful, as Pelto argued some twenty years ago (Pelto, 1970). We have brought together in this volume the work of anthropologists who are committed to using appropriate and innovative methodologies to build better theories. Also, in keeping with the notion that "nothing is more useful than a good theory," this body of work is relevant to the application of ideas. These essays are forward-looking in terms of their contributions both to methodology in anthropology, and to efforts in formulating public policy. Before examining the theoretical and methodological themes unifying these papers, we will briefly put them in the context of discussions of methodological issues in anthropology.

### A Historical Perspective

The history of anthropological research has been characterized by periods of intense interest in the methods by which that research is conducted, as well as by periods in which methodology has generated little thought or debate. Methodological debates were important in the development of anthropology early in this century. In response to what he saw as the excesses of armchair anthropology, represented by such people as Lewis Henry Morgan and Edward Tylor, Boas (original 1896, reprinted 1973) argued forcefully for the collection of primary field data in anthropology. He maintained that it would only be through the careful and systematic collection of primary ethnographic data that anthropologists could inductively arrive at lawlike statements concerning social and cultural processes, which in turn could be used to explain cross-cultural regularities.

Malinowski (original 1922) also was critical of a lack of systematic methods in ethnography. He stated:

The results of scientific research in any branch of learning ought to be presented in a manner absolutely candid and above board. No one would dream of making an experimental contribution to physical or chemical science, without giving a detailed account of all the arrangements of the experiment. . . . In Ethnography, where a candid account of such data is perhaps even more necessary, it has unfortunately in the past not always been supplied with sufficient generosity, and many writers do not ply the full searchlight of methodic sincerity, as they move among their facts but produce them before us out of complete obscurity. (Malinowski, 1961:2-3)

Later in this same work Malinowski, in opposition to an inductive logic, argues that the ethnographer is best prepared for work in the field by a thorough grounding in theory, for "problems are first revealed to the observer by his theoretical studies" (1961:9).

This vision of a scientific anthropology was not universally shared or adopted. Throughout this century there have been anthropologists who have argued that the discipline cannot—indeed, should not—be regarded as representative of the natural sciences, but rather is better regarded as a humanistic endeavor. Evans-Pritchard (original 1950, reprinted 1973) represented this point of view. His basic objection to the natural science model was that it led ethnographers to force their observations into rigid and inappropriate categories, in an attempt to generate the lawlike statements of science. Rather than formulating their observations to fit inappropriate models, Evans-

Pritchard argued that ethnographers should be immersed in the "social reality" (his term) of the culture studied, to understand it as completely as possible without reference to other natural law or generalization. Foreshadowing more recent writings in phenomenological sociology, Evans-Pritchard pointed out the paradox in his suggestion:

The concepts of natural system and natural law, modeled on the constructs of the natural sciences, have dominated anthropology from its beginnings, and as we look back over the course of its growth I think we can see that they have been responsible for a false scholasticism which has led to one rigid and ambitious formulation after another. Regarded as a special kind of historiography, that is, as one of the humanities, social anthropology is released from these essentially philosophical dogmas and given the opportunity, though it may seem paradoxical to say so, to be really empirical and, in the true sense of the word, scientific (Evans-Pritchard, 1973:368).

In other words, by giving up the false illusion that anthropological research can be conducted along lines similar to those employed in physics, biology, or even psychology, ethnographers will in part become more scientific, being freed to pursue social reality in the way best fitted to the particular context studied.

In the 1960s there was a remarkable spurt of interest in the development of more reliable and systematic methods for anthropological research. One source of this interest was ethnoscience or ethnosemantics, in which emphasis was placed on reliable techniques for eliciting culturally acceptable linguistic categories. It was also in this decade that explicit attempts to bring together anthropological methods in single sources were made, including Naroll and Cohen's (1973) handbook of methods, and Pertti J. Pelto's *Anthropological Research* (1970; Pelto and Pelto, 1978). These works differed from earlier contributions (e.g., *Notes and Queries on Anthropology* [Royal Anthropological Institute of Great Britain and Ireland] and the *Outline of Cultural Materials* [Murdock et al., 1982]) that were mainly collections of the kinds of topics that ought to be addressed by anthropology.

Both the Naroll and Cohen volume and Pelto's work were explicit in detailing a philosophy of social science that acknowledged the unity of scientific inquiry. Also, both were explicit in recommending a blending of the traditional strengths of anthropological inquiry—ethnographic methods—with quantitative methods.

Despite these positive moves, there was a countervailing trend in anthropology to reject a natural science or—to use a term that has gained the

logical status of ethnic slur—positivist model of research in favor of an approach that emphasized not the formulation of lawlike or causal statements, but rather the uniqueness of human cultures, an essential noncomparability resulting from the creative expression and manipulation of symbols by members of society. There are actually many varieties of this orientation, which could be labeled symbolic, interpretive, hermeneutic, phenomenological, deconstructivist, or even postmodernist anthropology. Whatever the label, there is a tendency, variably doctrinaire in its expression, to reject a natural scientific model of inquiry in favor of a human scientific, or even literary approach.

The degree to which the natural science and symbolic approaches can be opposed is exemplified by two articles, both published in the 1980s, by two distinguished anthropologists. Clifford Geertz, in the published version of his 1983 Distinguished Lecture at the American Anthropological Association, takes the concept of cultural relativism as a vehicle for making clear his mission for anthropology. His aim is not to defend cultural relativism, but rather to criticize what he terms anti-relativism approaches in anthropology. His main criticism is of orientations that seek to illuminate regularities in human behavior across cultures—orientations that employ notions of “human nature” or the psychic unity of mankind. He characterizes these approaches, in the published authors chosen by him as representative, as assuming a kind of neofunctionalist view of human behavior and society. Any kind of functionalist theory assumes a structure, or a set of elements and the relationships among those elements. At least a portion of the relationships can be described as functional, in the sense of contributing to the continued integrity of the system. These functional relationships regulate the values of system parameters within broad limits, and can be described in terms of central tendencies and deviations from those tendencies. Inputs to and outputs from the system—in this example a human social system—can be described in causal terms. The theoretical orientations criticized by Geertz employ concepts of “human nature” and the psychic unity of mankind as limiting conditions on the range of diversity in human social systems.

Geertz argues forcefully (and always eloquently) that these approaches represent a return to an outmoded view, one which violates the very essence of a human science.

“Looking into dragons, not domesticating or abominating them, nor drowning them in vats of theory, is what anthropology has been all about . . . We have, with no little success, sought to keep the world off balance; pulling out rugs, upsetting tea tables, setting off firecrackers. It has been the office of others to reassure; ours to unsettle . . . Merchants of astonishment” (Geertz, 1984:275). In other words, it is far from the job of anthropology to search for lawlike regularities that describe human behavior within and between cul-

tures. It is, rather, the job of anthropology to make explicit how much and how far cultures diverge, and to revel in that which is novel.

Melford Spiro (1986) also uses the concept of cultural relativism as a point of departure in an important article discussing the future of anthropology. He begins by distinguishing several varieties of descriptive relativism. A moderate form holds that cultures are indeed different and that all cultures can be regarded as equivalent in some evaluative sense. Spiro refers to this as the familiar Boasian form of normative relativism, one that is consistent with a science model. In the 1960s there developed a strong form of descriptive relativism that Spiro terms epistemological relativism, which serves as the foundation for symbolic anthropology.

“Epistemological relativism, by contrast, not only espouses the theory of wholesale cultural determinism, but it also holds a maximal view of cultural diversity; a combination, so it contends, that precludes both nonvacuous cultural generalizations as well as the axiom of psychic unity. Hence, that combination . . . constitutes its character for the claim that anthropology is an interpretive (‘hermeneutic’), not an explanatory (‘scientific’) discipline” (Spiro, 1986:265). In other words, because human behavior is both widely culturally variable and totally culturally determined, as anthropologists we can only describe and interpret, rather than explain in causal terms, that behavior. Spiro goes on to argue that this view is misplaced, that the fact of cultural diversity is what makes genuine causal (“scientific”) explanation truly possible. Therefore, it is not the fact of cultural diversity, or even a strong form of cultural determination, that precludes a scientific anthropology; rather, it is a prescriptive rejection of scientific approaches. He concludes the article with a systematic examination of the principles on which the hermeneutic critique of a scientific anthropology is based—principles that he shows are logically unsound.

It is worth noting here that although we have phrased this debate in the terms chosen by Geertz and Spiro, which are specific to the discipline of anthropology, there is no necessary reason to do so. The interested reader is referred in this regard to recent discussions by the French sociologists Raymond Boudon (1988) and Pierre Bourdieu (1988). Taking slightly different tacks on the subject, these sociologists agree that a fundamental issue dividing social scientific researchers is an “objective-subjective” dichotomy; that is, Can social science reach objective knowledge of social reality in research, or is it by its very nature forced to provide descriptive accounts of what various players in the game (researchers and research subjects) offer as accounts of reality, with no hope of verifying any account? Boudon concludes that, for some problems, an objectivist, hypothesis-testing, quantified social science is appropriate. Bourdieu concludes that a truly useful social science (useful at least in terms of theory building) must incorporate both of these perspectives.

The important point, however, is that although the debate can be centered around cultural relativism in anthropology, this is but one manifestation of a larger epistemological issue in social science in general.

In a nutshell, this is the modern history of a discipline in which some practitioners say that anthropology is a natural science, whereas others say that not only is it not a science, but indeed it should not be one. If this strikes the reader as a fairly quaint conclusion, that is because it is. This debate is obviously of long standing, and no clear resolution appears to be at hand. The terms in which the dispute is carried on have gained considerably in sophistication, but it remains a continuation of previous disagreements.

Our aim in this volume is not to resolve this controversy, for the simple reason that it cannot be resolved. Rather, our aim is to contribute more grist for the mill, but in such a way as to clarify some of the issues involved. All of the contributors to this volume come down on the scientific anthropology side of the debate. Furthermore, all of the contributors have been influenced in one way or another by the specific methodological approach advocated by Pelto and Pelto (1978). By providing a series of empirical examples of the use of that approach in a variety of substantive areas, we hope to show what the nature of a scientific approach in anthropological research really can be. This is no small contribution. By providing these examples, we show that the "positivist" characterization of explanatory/causal approaches in anthropology is the classic case of the straw man. It would be far beyond the scope of this essay to attempt an in-depth analysis of that term; it will suffice to note that, as used by many social scientists, "positivism" denotes a theory of knowledge in which social facts exist as objects, independent of social actors, their meanings, and the action of the observer. Explanation consists of creating propositions relating those objects, which in turn can be subjected to empirical verification. A truly positivist approach would consist of the development of objective measurements that could be used at all times and all places. All theoretical statements would then be verified by the use of those objective measurements, much as survey research is construed by some in sociology. Such a program has rarely been proposed in anthropology, but examples do exist (Moles, 1977).

Ethnography is the classic corrective to a proposal that a research program of this type should be instituted in anthropology. Familiar examples of how behaviors thought to represent one domain (e.g., religion) turn out actually to represent another (e.g., population regulation) abound. Therefore, what the critics of a scientific anthropology like to label as "positivism" is something we eschew as a naive empiricism. Instead, we seek to probe the limits of our own naiveté by the systematic integration of the ethnographic methods that represent the traditional strengths of anthropology with quantitative methods and formal research design, which will enable us to refine our



theories in every substantive area. This was the promise of the methodological programs proposed by Pelto and Pelto (1978), Naroll and Cohen (1973), and others (Johnson, 1978; Bernard, 1988). The papers assembled in this volume exemplify the partial realization of that promise, and can serve as guides to future work.

### The Contributions of This Volume

The papers in this volume are unified not by technique or even by method, but by a more abstract bridge that might be called "approach." The approach used in each paper is fundamentally problem-oriented. A question is raised, a puzzle is posed, and it becomes the task of the analyst to arrive at an explanation for that question. This question may involve the psychosocial risks of depression in a specific setting, or how to understand the determinants of successful fishing. Whatever the issue involved, it is first formulated as a question for which an explanation, causal in form, can be constructed.

One important advance in understanding social processes that has come out of anthropological theory is the awareness of how causality cuts across levels of system integration. Very often the behavior of particular individuals in specific communities is influenced in a very real and direct way by corporate decisions or cultural fads occurring half a world away. The incorporation of this understanding of macro-micro linkages has been an important step (see DeWalt and Pelto, 1985).

Once a problem has been formulated or a question asked, it then must be translated into operational terms. This is perhaps where interpretive and explanatory approaches diverge most completely. What is the point of operationalization? The simple point is that operational definitions enable the analyst to distinguish instances from noninstances of some phenomenon of interest (e.g., heart attacks, religious rituals, or fishing success). In other words, instances and noninstances can be compared. As Spiro (1986:265-69) notes, given the extreme relativism demanded by an interpretive approach, such comparisons, even of individuals within a single culture, are precluded. The extreme assumption of epistemological relativism holds that each individual's organization of knowledge and experience is unique, a result of his or her own biography. Under this assumption, at best we can interpret individual instances of belief or behavior, but without recourse to comparative examples related to different forms of experience, causal explanation can never be formulated. This same argument applies across levels of system integration, be it the individual, the social group, or the culture.

There are two crucial issues here. First, fundamental non-comparability flies in the face of the biocultural facts of life. Human beings did evolve as a

single species. Culture evolved in tandem with, and interacting with, evolving human biology. It seems illogical to presume, then, an extreme form of cultural determinism, because certain limits on the variability of human behavior, including "cultural" behavior, are built into the mechanism, so to speak. Cultures and human behavior are highly variable, to be sure. But that fact aids in the development of causal explanation, rather than hindering it. Causal explanations can be formulated precisely because there is so much variability in human behavior, not in spite of it. We only need assume that behavior varies within certain wide limits that enable us to define operationally that behavior, or those beliefs, or those forms of social interaction (Spiro, 1986:265-71).

Second, an assumption of noncomparability can be viewed as applying not to the phenomena under investigation, but rather to the analyst. This really is the familiar "Martian observer" argument. Suppose, the argument goes, that a Martian ethnographer arrived on earth to study human behavior. He/she/it would be unable to understand the behavior because of Martians' inability to comprehend the meaning of that behavior. The argument goes that the Martian could not comprehend a genuflect before a religious icon because of an inability to grasp the meaning of that behavior. As Rudner (1966:5-7) points out, however, this is not an assumption about scientific methods, but rather a hypothesis about Martian observers. If the Martian ethnographer were able to formulate a hypothesis about genuflection, then that hypothesis certainly could be tested by operationalizing terms and using the agreed-upon canons of scientific logic. We are not sure about the psychology of Martian ethnographers, but human ones have shown themselves to be remarkably creative in proposing explanations for human behavior. Indeed, symbolic anthropologists are among the most creative, and their interpretations are fundamentally complex systems of hypotheses arising from the context of discovery of scientific research. The next step in the process is to formulate these hypotheses in systematic terms to be tested in the context of validation, a step not taken by symbolic anthropologists.

This blend of the interpretive and explanatory, of the qualitative and the quantitative, of the context of discovery and the context of validation, holds enormous potential for future research in anthropology. It is a truism in the philosophy for future research in anthropology. It is a truism in the philosophy of science that there is not a logic of discovery. One only need recall Watson's phenomenologic reconstruction of his formulation of the double helix to understand why. At the same time, however, discovery processes can be systematic, and the hypotheses derived in those systematic processes can in turn be scrutinized with the agreed-upon logic of validation. The blend of qualitative and quantitative methods can be viewed in this way. By being sensitive to local context and local knowledge, the anthropological researcher is more



likely to arrive at valid constructs of relevance in a particular setting, and in turn to be able to formulate valid operational definitions for key terms and creative hypotheses relating those terms. Using this strategy, systematized by anthropological methodologists (Naroll and Cohen, 1973; Peltó and Peltó, 1978; Johnson, 1978; Bernard, 1988), the range of cultural diversity again opens itself as a fertile field in which to explore theories of human behavior.

How does this work in practice? Or even, *can* this work in practice? The papers in this volume have been assembled as evidence of how, employing this approach, key contributions to anthropological theory and to the application of anthropological understanding to the solution of policy issues can be made.

Beyond what we have referred to as "approach," there are five specific themes to which these chapters are addressed. These five specific themes represent a less abstract set of principles for conducting research than the general scientific approach described previously. These themes include: (1) the blending of qualitative and quantitative approaches; (2) the search for micro/macro levels of generalization; (3) the importance of intracultural variation; (4) innovative solutions to research problems; and (5) the practical or applied merit of systematically generated and evaluated theory. These principles were first proposed by Peltó and others, and it is our conviction that systematic application of these principles will carry anthropological research forward in a new and fruitful period of theory construction and testing.

The systematic integration of qualitative and quantitative techniques for gathering data holds considerable potential at several different levels of the hierarchy of theory and method. First, it should be made clear that in many instances, these forms of data collection lie on a continuum. In many cases the collection of data in the field consists primarily of talking to people. When data are collected in a manner such that each individual is asked the same questions in the same order, and the responses are constrained to a finite set, we refer to the information as quantitative data. When information is collected in such a way that each individual interview is treated as separate from each other, and the focus of analysis is on the specific content of utterances, we refer to the information as qualitative data. In short, the information itself is the same; it is how we as analysts treat it that varies.

An obvious way to integrate the two is from the standpoint of operationalizing variables. What precise verbal utterances can be construed as an instance of a variable of interest? Careful qualitative work is a prerequisite for quantitative work in many situations. On the other hand, in most cases the results of a quantitative analysis are reliable, valid, and frustratingly opaque. As we struggle to understand the theoretical significance of a correlation matrix, it is often useful to be able to turn to a description in words from an informant of the same reality described by a correlation matrix. Qualitative

data can serve as an invaluable adjunct in interpreting quantitative results. There is thus theoretical as well as methodological value in interweaving these forms of data collection throughout a research project. There are, of course, other advantages to the use of multiple methods of data collection, which will be discussed in more detail in specific contributions that follow.

The integration of microlevel and macrolevel data and the systematic search for theoretical statements that cut across these levels is a second theme of this volume. Even a cursory examination of this notion of levels reveals the combined theoretical and methodological importance of the concept. In one sense, the notion of level is a purely methodological issue. In macrolevel data analysis, some aggregate of individuals, whether it is a county, a village, or a whole society, is the unit of analysis. This is a favorite form of analysis for policy scientists, since official statistics often come packaged in this form. In microlevel data analysis, the unit of analysis is usually the individual, but sometimes even smaller units (e.g., body movement). If this were the only difference, this would be an uninteresting and unfruitful distinction; but as Robinson (1950) originally pointed out with his concept of the "ecological fallacy," what patterns exist in the aggregate are often not evident in individuals, and vice versa (see also Schweder, 1973). But clearly the individual and the aggregate are not some completely different order of phenomena. Therefore, the relationship between individual level phenomena and aggregate level phenomena assumes theoretical as well as methodological importance. This relationship of levels of phenomena is something familiar to ethnographers as their attention turns from the most isolated and self-contained communities to those communities linked in systematic ways to supracommunity power structures. New emphases such as dependency theory or world systems theory also alert us to the fact that causal influences in the lives of individuals at the local level often emanate from social and economic institutions far removed from that local level. The challenge for theory and method is to develop concepts and measurements that enable us to model those linkages in systematic, not impressionistic terms, a challenge taken up by several contributors here.

A third theme addressed in these papers is the need to pay close attention to the intracultural diversity (Wallace, 1970; Pelto and Pelto, 1975). It is often thought that by intracultural diversity only individual variability is meant, but this is a false equation. As originally formulated, the concept of intracultural diversity was intended to balance the simplistic cultural uniformist notions in anthropology. Stated boldly, a cultural uniformist orientation presumes that there is one culture (i.e., a single set of rules and assumptions) within a collectivity, and that deviation from that culture occurs because, for example, some people do not know their culture very well, or perhaps be-

cause some people are deviants in the sociological sense of the term. In short, variability is treated as noise in the data.

Few people would acknowledge that kind of uniformist thinking these days, but there is still a lack of appreciation of how a single culture can come in different versions, as it were. That is, within a single community that can be described as a single culture in any generally accepted sense of the term, there may be systematic differences in the organization of knowledge and beliefs leading to patterned nonsharing, which in turn is distributed on social structure. If we wish to study some dependent variable and the predictors related to it, it seems likely that any statistical model will be badly misspecified if we attempt to fit it uniformly across these contexts of intracultural diversity. If, on the other, we recognize this patterned diversity within a culture and fit parameters to the model to partition the variance generated by that diversity, the fit of our model will be enhanced. Examples of this approach occur in several chapters here.

Every fieldworker, especially those employing more formal sorts of research design, will have run into these seemingly insolvable problems. It appears as though the data to operationalize a key concept or to test a particular hypothesis simply do not exist; or, if the data do exist, access to them is curtailed by any number of obstacles as various as the social settings we investigate. It is here that the creative and innovative solutions in research come into play. Like the formulation and generation of a hypothesis, these innovative solutions cannot be programmed into use. But the answers provided by other fieldworkers can often stimulate our thinking and lead us to consider a new avenue in research design and analysis, and this again is a theme addressed by the authors here.

The fifth and final theme that unifies this volume is that a systematic and competently evaluated theory will have implications for application. Although not all the contributors to this volume necessarily think of themselves primarily as applied anthropologists, all of them share the conviction that in order to have any salutary effect on the world, any social intervention must be guided by theory that has been tested and found to be empirically useful. Indeed, there is now too much of a division of labor between pure and applied research, between basic science and the policy sciences, between analysis and advocacy. We do not, of course, suggest that if we understood the world better we would automatically change it. Obviously, a refined understanding of the world can only be useful if the mechanisms and energies for application are available and in place. But unfortunately, as many examples attest, the mechanisms and energies for application are impotent if we fail to understand, in precise, measurable, valid, and replicable terms, the substantive phenomena we seek to change. The contributors to this volume take the

business of understanding social phenomena seriously precisely because that understanding may or will be used to alter the lives of individuals.

In summary, this volume is intended for all students of anthropology, no matter what their status (undergraduate major or full professor), as a set of concrete examples of how a set of methodological principles can be applied in diverse settings and to diverse topics to improve our understanding of social phenomena, and our ability to influence those phenomena.