

What Is Behavioral Anthropology?

Introduction

With the maturation of anthropology as a social science our research interests are inevitably broadening. To a traditional interest in describing the typical culture of various groups we are increasingly adding efforts to plot and explain the range and distribution of their internal variations (Pelto and Pelto 1975; Boster 1987).¹ For “shared meanings” are not totally shared, cultural knowledge is not uniformly acquired, practice frequently falls short of ideal, and even the “ideal” differs from individual to individual. This developing research interest acquires further importance as the pace of culture change increases and the pressures for modernization thrust new problems upon us, problems whose understanding rests in within-group variation.

Shifts in research objectives require concomitant shifts in research strategies. With increasing frequency anthropologists are finding it necessary to adapt and employ in their research the tools of their sister disciplines—sampling procedures, survey instruments, structured interviews, systematic observations, psychometric and sociometric tests—together with multivariate statistical techniques for analyzing and presenting large amounts of quantitative data (Bernard 2002).

By chance circumstances I became a pioneer in this process. In 1969, when I was only 37, the Department of Anthropology at UCLA made me an offer I could not refuse: a tenured full professorship to direct their NIMH-funded graduate research training program. I was young and ambitious, eager to stake out a distinctive professional identity for myself,

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for this training program, and for the anthropology department at UCLA. So, to distinguish our approach from the rest of the field, we called it “Behavioral Anthropology.”²

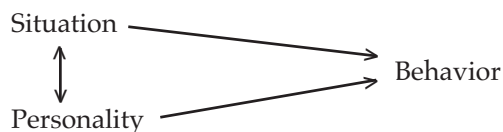
For the previous decade I had been working in the Institute of Behavioral Science at the University of Colorado, first on the inter-disciplinary Tri-Ethnic Community Research Project, and then as director of a graduate Research Training Program in Culture Change and an affiliated study of Navajo Indian migrants to Denver.³ From this experience I had developed a strong commitment to systematic, quantitative, interdisciplinary team research—not what most anthropologists do. But this is what I strongly believe we *should* be doing, at least some of us, if we want to participate as full members in an on-going and cumulative “science of human behavior.” And it is what I wanted us to be doing, and training students to be doing, in our Research Training Program in Behavioral Anthropology at UCLA.

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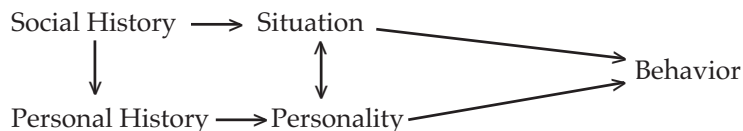
In simplest terms, cultural anthropology studies norms of appropriate behavior; behavioral anthropology studies what people actually do. Currently, there is a growing trend within anthropology towards this focus on “praxis” (Ortner 1984). But behavioral anthropology is much more than that. By “behavioral anthropology” I mean *the formulation and systematic testing within cross-cultural field settings of theoretically grounded hypotheses concerning the causes, correlates and consequences of variability in human behavior*. Traditionally “cultural” anthropologists have focused attention on *within-group regularities* and *between-group differences* in beliefs and behavior; by contrast, our approach requires that we pay attention to *within-group variability* in these as well, that we seek explanatory models which can cope with and help us to understand this within-group variation, that these models be capable of systematic empirical test, and that they be examined within a variety of contrasting cultural groups. The growing body of empirically grounded research which results will then constitute the first steps toward an understanding of what is humanly universal and what may be culturally idiosyncratic.

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This change of focus from descriptions of a group's "culture" to explanations of behavioral variability within even the most homogeneous cultural group does not make me a Skinnerian "behaviorist." All we have to work with as social scientists—our data—are observations of overt human behavior, even if these take the form of verbal responses by our subjects to interviews, questionnaires, psychometric tests, or experimental procedures. But human behavior (and that of many experimental animals as well) is determined not just by situational cues—a simple Pavlovian Stimulus → Response sequence—but also by how these situations are *perceived* and *evaluated* by the actor: Stimulus → Perception → Response (T. Graves 1973, reprinted as Article 7 in Volume II of this series). Furthermore, actors bring to every situation their own habits and predispositions, beliefs and expectations, values, goals, objectives and plans, all of which serve as important determinants of their behavioral responses. The model I use in my work looks more like this:⁴



The "situation" includes the entire objective *context* within which a person operates, including other people, institutions and artifacts. Measurable characteristics of complex situations obviously have some effect on how they will be perceived. But personal preferences also influence what arenas someone is likely to choose to enter. Thus the double-headed arrow between Situation and Personality. But both also have a direct (often complementary) effect on how a person will respond (Behavior). Finally, whenever possible I like to see the inclusion of an *historical* analysis of how a current situation came into being, and *developmental* research on determinants of a subject's personality predispositions at the time behavioral choices are made. This results in a *process* model which may look something like the following:



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To pursue a model of behavior such as this requires that anthropologists add to their analyses of social history and cultural evolution the concepts and tools of sociology and psychology. Measuring relevant characteristics of the objective situation within which people operate is difficult—for example, the job opportunities available and level of unemployment when they arrive as migrants in some new urban setting—particularly if we recognize that actors may differ in their own situation as well: married or single, living alone or with a group of kin. Measuring their *subjective* personality characteristics is even more problematic. These determinants are *phenomenological*—they exist unseen within a person's head and must be *inferred* from what they say and do. This process is interpretive—*hermeneutic* if you will—and often subject to alternative interpretations. Our job as “psychological anthropologists” is to make this interpretive process overt and public, and our psychometric measures as appropriate to the cultural context within which they will be used and as “reliable” and “valid” as possible. I will discuss these issues at greater length in Chapter Six on “Measuring Psychological Variables.”

The Role of Culture in Human Behavior

What role does “culture” play in my model? The concept of culture grew out of the experience of Western explorers, missionaries, traders and travelers confronting the many strange and wonderful customs of exotic tribes in “primitive” and “underdeveloped” areas of the world. Describing these novel customs before they were “lost” by contact, conquest, conversion and co-option became the first priority of our profession. It served, and continues to serve, as a convenient basis for organizing museum displays and introductory courses in comparative society. Unfortunately, by emphasizing these between-group differences and within-group similarities it undoubtedly contributes to an all-too-common tendency among human beings to stereotype other groups and to ignore the many similarities among us in the ways humans respond to similar conditions.

Furthermore, a descriptive term used to refer to typical characteristics of some exotic group then became transformed into an “explanation” of their behavior. Anthropologists are wont to say that the Bula Bula behave as they do because of their “culture”—the traditional ways of thinking,

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feeling and doing passed down to them by their ancestors. They speak Ki-Bula because they learned it at their mother's knee, and they worship Bulanese gods and goddesses because their people have always done so for as far back as anyone can remember.

This is still true to some degree of course, but it is becoming increasingly less true as the authority of tradition and of the elders is being undermined by Western contact. Furthermore, a group's "culture," when it is presumed to be shared by all its members, has difficulty accounting for within-group *variation* in belief and behavior which anthropologists have long recognized and reported within even the most isolated and homogeneous group. (For early references, see Dorsey in Sapir 1938; Hart 1954.) And since a group's culture is also conceived as basically self-perpetuating from one generation to the next, it cannot account for *change* in their behavior over time. When variation and change become the focus of our research we need new explanatory concepts. These are exactly the kinds of problems behavioral anthropologists typically want to address.

Even as an explanation of what between-group differences and within-group regularities we *do* observe, "culture" is a cop-out. It serves to conceal our ignorance of the underlying processes involved, not only from others, but from ourselves as well. By giving us a handy label to attach to our observations, it satisfies our curiosity before we are stimulated to seek more adequate explanations. Thus it can become a barrier to scientific progress.

In the last chapter of this book I will discuss these problems at greater length, and attempt to formulate and clarify roles the concept of "culture" may continue to play in an interdisciplinary science of human behavior. For now, I simply state that on purely pragmatic grounds I rarely found the concept useful in my work. During the first ten years of my professional life, for example, while participating in the Tri-Ethnic Community and Navajo Relocation projects at the University of Colorado, we were able to explain most of the within- and between-group differences in drinking behavior among our subjects without recourse to the cultural heritage of the Native Americans, Hispanics and Anglos we were studying. Instead, our explanations were lodged in the uneven distribution of economic opportunities and other structural variables among all the groups under investigation, which in turn had psychological parallels or conse-

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quences within the heads of our subjects. Regardless of their ethnicity, measures of these structural and psychological variables alone each successfully “predicted” their drinking behavior, and from a combination of the two types of variables, even higher levels of explanation could be achieved (Jessor, Graves, Hanson and Jessor 1968. For examples, see Chapter Four below.) From this decade of experience I concluded that a focus on psychological and structural variables such as these has far greater explanatory pay-off than attempts to describe the unique cultural traditions of the drinkers. And this non-cultural orientation had substantial empirical success in our research among Navajo Indians. (See Volume II, Part 2 and Chapters Five through Eight below.)

This focus on structural and psychological *variables* rather than on a non-differentiated and over-deterministic concept of “culture” has also enabled me to work closely with economists, sociologists, and psychologists who also may find “culture” a difficult concept to incorporate in their work (Bennett 1954).

Distinctive Features of Behavioral Anthropology

I would like to turn now to a more detailed examination of behavioral anthropology, its roots in more traditional anthropological method and theory, and the contribution I believe it can make to an emerging interdisciplinary science of human behavior.⁵

The distinctive features of behavioral anthropology are to be found at all stages in the research process: (1) in the selection of an appropriate problem for investigation; (2) in the type of explanatory theory invoked as a framework for research; (3) in the research design; (4) in the measurement of variables and data collection; and finally (5) in the analysis and interpretation of these data. Some of what follows is implicit in what has already been said; most will receive further elaboration in the rest of this book.

The Type of Problems Investigated

“Cultural” anthropologists typically select for investigation areas of belief and behavior in which there is *high consensus* among group members (D’Andrade 1987 and 1995). This is in line with, and helps reinforce, a

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concept of culture as a group's non-biological, shared heritage, and "culture as consensus" continues as a focus of inquiry (Romney 1999). The group's *language* and other forms of *symbolic communication*, which are public expressions of group identity, require interpersonal agreement for their success, and provide constant opportunities for corrective feedback. These serve as prototypical "cultural" artifacts, and the rules of appropriate behavior which constitute a language's "grammar" provides a "model" for the concept of "culture" as a whole: the "grammar of interpersonal relations."

Among other typical topics chosen for investigation by ethnographers, *kinship terminology*, *marriage rules*, and related *kin-governed behavior* stand out as clear favorites. There is also a variety of similar *classificatory behavior* dealing with taxonomies of other things than people: like firewood, for example (Metzger and Williams 1966). *Social stratification* and associated *deference and demeanor* are other pet topics, and the more rigid the system (India's caste system and caste-like systems elsewhere) the better we like it. *Ceremonies, myths and rituals* are also favored, usually being highly formal and tradition-bound. (They also make good cocktail conversation. And they generally occur too seldom during our fieldwork to challenge our assumptions of uniformity.) *Trading relationships* and other types of reciprocity also may assume near ritual form. These are the safe topics, the ones an ethnographer may choose when the rich diversity within a new setting overwhelms.

For this traditional interest of cultural anthropologists in the description of normative patterns and group regularities, behavioral anthropologists typically substitute an effort to explain within group *variation* in observable behavior. Behavioral diversity is to be found within even the most stable and isolated human groups, and its sources within these groups can certainly be usefully studied. But most examples of behavioral anthropology with which I am familiar are concerned with *culture change situations* and the diversity generated by contact with an alien way of life. (Historically, the roots of behavioral anthropology can probably be found in the emergence of *acculturation* as a problem, and the inadequacy of traditional anthropological approaches for dealing with that problem.) This does not involve simply the free adoption of alien patterns of behavior by some and not by others, though this is an important problem (Maher 1960;

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Woods and Graves 1971). It may also entail explaining different *reactions* to the contact situation itself, including hostility and group conflict (Parker 1964), drinking and deviance (Graves 1967; 1970), changes in childrearing practices (Boggs 1956; N. Graves 1971; Whiting 1996) and their effects on children's personality and values (Boggs 1958).

Despite the multi-culturalism implicit in any acculturative situation, this emphasis on explaining within-group variation makes our work appear superficially like that of sociologists and social psychologists studying differences in beliefs and behavior within our own society. But closer examination reveals many distinctive anthropological features, which will emerge more clearly below.

The Type of Explanatory Theory Evoked

Many cultural anthropologists take as their model of all behavior the linguists' model of verbal behavior. The basic proposition underlying this model is that *all behavior is rule-governed*; the aim of anthropological inquiry therefore becomes the specification of the underlying "grammar" of "appropriate behavior" which serves as the guide for any observable act. Consequently, if you describe their culture—the norms and rules which they learned from their elders—you will understand their behavior. Some time ago Clifford Geertz provided an articulate example of this point of view: "culture is best seen . . . as a set of control mechanisms—plans, recipes, rules, instructions (what computer engineers call 'programs')—for the governing of behavior," and mankind is "precisely the animal most desperately dependent upon such extragenetic, outside-the-skin control mechanisms, such cultural programs, for ordering his behavior" (Geertz, 1965: 57). Many may rebel at this view of culture as a computer program, with human beings fed in like stacks of IBM punch cards and their behavior spewed forth on reams of print-outs. And to be fair, Geertz soon backed off this definition and adopted what he refers to as "an interpretive theory of culture" in which "culture is not a power, something to which social events, behaviors institutions, or processes can be causally attributed; it is a context, something within which they can be intelligibly—that is thickly—described" (Geertz 1973:14). Current theorists have recognized that there is no one-to-one relationship between "cultural models" and human behavior, and seek to investigate how action is "culturally informed"

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(D'Andrade and Strauss 1992). "In current anthropological theory there is no clear relation between *culture* and *action*. Of course, one can say 'people do what they do because their culture makes them do it'" D'Andrade observes. "The problem with this formulation is that it does not explain anything" (page 23). Nevertheless, these authors and many other anthropologists continue to expend inordinate time and effort in seeking ways to link shared "culture" to individual motivation (Jessor, Colby and Shweder 1996).

Behavioral anthropologists have taken a different tack. Typically we focus on observed behavioral *diversity* and seek its explanation in the interaction between actors' personal attributes (both physical and psychological) and attributes of the environment (both material and social) within which they find themselves. These situational factors, in turn, have their roots in the history of the group, which also affects—through biological and social inheritance—many of the individual's personal characteristics as well, *but not uniformly or universally*. Finally, modification in any element within this system of concepts changes the context within which the other elements operate, producing waves of linked changes which can profoundly alter the environment of experience at both the individual and group level. This model is thereby capable of accounting for both continuity and change, both variation among actors at any point in time as well as individual and group variation through time.

This explanatory model is holistic and eclectic, in traditional anthropological fashion. Human beings are endowed with varying biological attributes, they are the product of differing historical circumstances and personal experiences, and they live out their lives within a multitude of changing physical and social settings. Most anthropologists, I believe, would find this explanatory model compatible with their traditional view of the world. The only element which is missing is the concept of "culture."

For interdisciplinary research on within-group variation in behavior and its correlates, an analytic approach which breaks the concept of culture into less global elements capable of being distributed unevenly within a population turns out pragmatically to be a more useful theoretical strategy, and one which other behavioral scientists can understand and work with. Thus we look at differences in the degree to which a community's members are mapped into certain social control structures, the degree to

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which they hold certain widely shared beliefs about the nature of the world, the degree to which they value certain commonly sought goals, and so forth. Not only does this strategy make possible within-group analyses such as we commonly pursue, it also permits more meaningful, because more differentiated, comparisons *between* groups, as in the Tri-Ethnic Research Project (T. Graves 1967a, Jessor, Graves, Hanson and Jessor 1968), N. Graves' comparative study of parental feelings of personal control among Anglos, Hispanics, and East African Baganda (1971), and differences in consumption and violence between groups of Polynesian and Pakeha drinkers in New Zealand pubs (Graves, Graves, Semu and Ah Sam 1981 and 1982; reprinted in Volume II, Articles 13 and 14).

Research Design

When I was a student the typical anthropological research design could be summarized as follows: Pick a spot, the remoter the better, and then go live there for a year. Since there are now more anthropologists than remote unstudied spots, we have been forced to reassess that approach. Behavioral anthropology is distinguished by far greater attention to research design. Usually the problem has emerged out of field investigation, as in traditional ethnographic inquiry, but strategies are then carefully specified for its systematic investigation. (See Chapter Ten.) Eventually, as more of the world becomes better known (and we shed our need to conduct our inquiries among the least known groups available) we may frequently reverse this order, first specifying a problem, and then selecting a spot where this problem can fruitfully be investigated (N. Graves and T. Graves 1978, reprinted in Volume II, Article 10). But even then preliminary exploratory work should precede the formulation of a research design, and then be followed by more systematic hypothesis-testing studies.

Anthropology as a whole is a *field* science, and behavioral anthropologists share this tradition. Our commitment is to studying behavior in its natural setting. This raises severe problems of "control" over the influences of mutually interacting forces that have operated over long periods of time. The effects of such forces can sometimes be "controlled" in the laboratory (Bruner and Rotter 1953). But since they are not controlled in nature, this procedure raises questions about the "external validity" of laboratory research findings: Would similar results actually occur in real-

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life settings? (See Campbell and Stanley 1963/1966.) Because of our dedication to understanding how people behave in fact rather than in theory, we are generally willing to sacrifice laboratory controls for fieldwork reality. But we haven't yet devoted sufficient thought to methodological strategies for achieving alternative forms of control over the inferential ambiguity which natural settings introduce into our work (Jessor, Graves, Hanson and Jessor 1968, pp. 137-149). This is a challenge which confronts not only behavioral anthropologists but all of behavioral science, and I hope that our research strategies will help contribute to its solution. (See Chapter Five on "Research Design.")

As in traditional anthropology, the usual focus of attention of behavioral anthropologists is a single, non-Western group. Increasingly, however, I believe we will be adopting a *comparative* perspective. Because we are examining relationships between within-group variation in behavior and within-group variation in other attributes, and because the content of our measures (but not our analytic concepts) may be specific to a particular setting in which they are employed, direct comparisons between groups on the same operational procedures are often inappropriate. Such efforts by ethnologists have evoked justified criticism that the units of comparison are not truly comparable. Rather, I would hope that we can increasingly examine whether the same *pattern of relationship* between variables holds within two or more distinct ethnic groups (Jessor, Graves, Hanson and Jessor 1968; N. Graves 1971; T. Graves, N. Graves, Semu and Ah Sam 1981 and 1982; T. Graves and N. Graves 1985). This would provide us with cross-cultural validation of within-group processes, as well as making possible a *systematic* examination of the role group membership variables may play. (See T. Graves 1967a and the sociological literature on "structural effects.") This also turns out to be a useful enterprise for generating new theory or refining old ones, by specifying the conditions under which they apply (N. Graves 1971, T. Graves and N. Graves 1985).

Finally, behavioral anthropologists, like traditional ethnographers, are interested in creating explanations firmly grounded in the specifics of the situation we are studying. This is in line with a growing concern within social science to break free from the constricting influence of "inherited" grand theory, and to be actively engaged ourselves in the process of theory-building. Traditional anthropological methods of par-

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ticipant observation and key informant interviewing are useful “discovery techniques” for generating new explanatory theory, but we should be exploring others. (See Chapter Eleven on “The Interplay Between Theory and Research.”)

Behavioral anthropologists are concerned both with *generating* appropriate theory and systematically *testing* its explanatory worth. Thus we want to go “beyond ethnography,” and our reports often begin just at the point where traditional ethnographic accounts conclude. (See Chapter Ten for an example of “A Different Kind of Ethnography.”) Fairly explicit hypotheses and their empirical examination are the typical result. But be warned: A theory to be of any value must be both testable and refutable. Thus behavioral anthropology is risky business, and our pet theories may crumble in the face of empirical reality (T. Graves 1974—reprinted in Volume II, Article 6.) But that, too, is part of the joy of the enterprise.

Measurement and Data Collection

An extended period of *participant observation* and interviews with a handful of *key informants* remain the major data collection procedures employed by cultural anthropologists. These methods may work reasonably well if our research problem involves the description of cultural knowledge and beliefs in which there is “high consensus” among informants. But when our focus of inquiry is on *within group variation* in beliefs and behavior, these methods are totally inadequate. “What sort of scientists are they whose main technique is sociability and whose main instrument is themselves?” Geertz has recently asked (2000:94). Indeed.

Behavioral anthropologists are particularly distinguished from our colleagues by the field methods we employ. Hypothesis testing requires systematic quantitative data. Developing clear “operational definitions” of our key concepts has therefore become a focal concern of behavioral anthropologists (Pelto and Pelto 1978; Bernard 2002). And so we have been forced to look beyond the limited methods learned from our teachers. First, we have had to give up *inappropriate* uses of participant observation and key informant interviewing, and when testing our hypotheses, pay careful attention to sampling problems. (See Chapter Eight on “Samples and Surveys”.) Then we have become methodologically eclectic, borrowing

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field methods freely from sociology, social psychology, economics, etc. (See Chapters Six and Seven on “Measuring Psychological Variables” and “Social Channeling of Behavior.”)

The easiest technique to borrow and apply cross-culturally is a survey interview of a random sample of the entire group we are studying (Graves 1966b). There are many anthropological purposes for which the survey interview is indispensable, and despite doubts of some colleagues, it clearly *can* be employed with success. But there are a host of other procedures which deserve attention: non-reactive techniques such as have been suggested by Webb, et al (1966); the use of self-anchoring scales (Kilpatrick and Cantril 1960) and content analysis which permit our subjects’ own categories and dimensions to emerge (Graves and VanArsdale 1966); rating procedures (Chance 1965); projective tests designed for and tailored to the situation being studied (Goldschmidt and Edgerton 1961; Parker 1964; Michener 1971); and direct, but systematic observations of on-going interpersonal behavior (Boggs 1956; Whiting 1963; N. Graves 1971— See Chapter Nine on “Direct Observations of Behavior.”)

The use of quantitative procedures cross-culturally creates a new set of methodological problems peculiarly anthropological. But their solution, I believe, will make an important contribution to behavioral research within our own society as well. Sociologists and social psychologists, when they construct a measuring procedure, can draw on their own experience as members of the group they are studying to assess the appropriateness of their instruments. They are their own anthropologists. But when working cross-culturally, the “face validity” of a scale is far less dependable. Behavioral anthropologists need to devote more attention, and imagination, to the problem of *validating* the procedures we are using in the field. Here the traditional methods of participant observation and key informant interviewing can again be invoked, but for systematic ends. When conducting social science research cross-culturally, nothing substitutes for personal familiarity with the field (Woods and Graves 1973, reprinted in part in Volume II, Article 9), or for involving a few key members of our subject population in the construction of our instruments (Graves, Graves, Semu and Ah Sam 1982, reprinted in Volume II, Article 15 and discussed in Chapter Thirteen below.)

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Analysis and Interpretation

One of the major limitations of conventional anthropological inquiry, particularly “interpretive anthropology” made popular by Clifford Geertz and his followers, is that it is highly dependent on the skills and insights of the ethnographer, these insights and interpretations must be taken “on faith” by others, and are frequently not subject to verification. This problem was noted early in our history:

It can be taken as a general rule that intensive research upon the same preliterate people by a variety of ethnologists gives rise to considerable controversy and disagreements over the nature of fundamental institutions and cultural expressions. Things which are accepted verbatim about groups reported on by single field workers are subject to considerable scrutiny and argument when the area is opened up to additional members of the profession. (Bennett, 1946)

“Replicability” and “verifiability” stand at the very heart of science. A major critique of “interpretive anthropology” as practiced by Geertz and his followers has been its lack of replicability or verifiability (Spiro 1986). It is all too easy to project more uniformity and structure into our ethnographic observations, Homer Barnett warned us long ago, than may actually exist:

All of us search for patterns in our information, and sometimes we find them when they are not there; or, if they exist, they are amorphous and protean. We are inclined to discern form and structure partly because our concept of culture leads us to believe they must be there despite the vagueness and vacillation of our informants on crucial questions, and partly because we need a basis for comparability between our findings and those of other ethnographers. (Barnett 1960)

Because at least some of the data collected by behavioral anthropologists will be quantitative in form, they are amenable to mathematical and statistical manipulation. (See Chapter Four on “Simple Data Analysis” and Chapter Twelve on “Multivariate Analysis and Causal Inferences.”) This

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brings both advantages and risks. The obvious advantage is the grounding our interpretations then have in an objective and replicable body of empirical data. We may not agree on our conclusions, but we know with clarity wherein lies their basis. Furthermore, with a rich store of quantitative data to draw on, it is clear that we can discover relationships which are far too subtle to come to the attention of an ethnographic observer, no matter how sensitive (Woods and Graves 1971, reprinted in part as Article 9 in Volume II). If these empirical relationships can then be interpreted through the aid of a rich store of personal experience within the setting being examined, representative case studies, and other more qualitative material, we would seem to be on pretty sound grounds. Basing our reports on carefully collected quantitative data narrows considerably potential grounds for controversy. It also provides a basis for later observers to build on earlier work by others with greater confidence.

But the risks of employing quantitative data are also clear. First, you can be proven wrong, and have to reformulate and retest your theories. This can be a slow and painful process (N. Graves 1971). Second, the more public your procedures and your data, the more susceptible your reports to challenge by your critics. But for those whose goal is understanding, these risks, too, will be cherished as priceless advantages.

Finally, there is a more subtle risk, and one to which it is easy to succumb. This is the danger that mathematical and statistical manipulations become ends in themselves rather than paths to valid understanding. Given the limitations inherent in anthropological field data, no matter how carefully collected, and the many resulting violations of the mathematical assumptions upon which most statistical inferences are based, wise researchers will not take their statistics too seriously. Here again traditional anthropological wisdom has something to offer, though in new form. Our claim to the validity of our interpretations should rest not on the level of statistical significance achieved, but on the consistency and interpretability of the *pattern* of interrelationships obtained (Many examples from my own work are provided in Volume II of this series.) The more variables which are mapped into the analysis, the less likely that this pattern of relationships is the product of chance factors. The more operationally distinct the procedures and different the samples of people which yield similar results, the more confidence we can have in our conclusions.

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Traditional ethnographers analyzing “cultural patterns” or “themes” also looked for the implications of their interpretations within a variety of distinct social contexts. And their analyses gained in compelling quality the more of these they could claim to have found. The same procedure is applicable here, but the firmer empirical grounding available should help us avoid charges of subjectivism, projection, and the systematic (if unconscious) omission of negative instances.

The Structure of This Book

This is an intellectual autobiography covering my thirty years of research experience as an anthropologist from 1954 when I began work among a group of hospitalized Navajo Indians until 1983 when our last research grant came to an end and Nancy Graves and I decided to leave the University of California at Santa Cruz and devote full-time to applied problems: training teachers in cooperative small group classroom methods. My goal in this book is to explicate as clearly as I can from a review of my own intellectual history what I see as the theoretical and philosophical foundations of Behavioral Anthropology, some of its research implications, and the contributions I believe anthropology and anthropologists can truly make to an integrated science of human behavior.

In the following chapters I will illustrate how my own approach to studying human behavior in cross-cultural perspective introduced in this chapter evolved over the years and how it differs from that of most anthropologists. The power of this approach to reveal subtle relationships which typically escape the attention of field anthropologists who concentrate on describing a group’s “typical way of life”—no matter how “thick” that description—should thereby become apparent. Each chapter is devoted to a particular research issue that I was addressing at that time, from “Measuring Behavior” (Chapter Three) to “Research and Application” (Chapter Thirteen). In the final chapter on “Anthropology in the Twenty-First Century” I will discuss the ways I believe Behavioral Anthropology can contribute both to a “new agenda” for our field, and to an integrated science of human behavior.

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Notes

1. See also the entire issue of the *American Ethnologist* (Volume 2, Number 1, 1975) and the entire issue of the *American Behavioral Scientist* (Volume 31, Number 2) on intra-cultural variation which these two essays introduce.

2. William B. ("Pete") Rodgers, who had been instrumental in luring me to UCLA, was the first to suggest the name, and I adopted it as providing a nice contrast to "cultural anthropology."

3. For some representative publications emerging from this work, see Parts 1 and 2 of Volume II of this series.

4. Physical anthropologists, socio-biologists and evolutionary psychologists may want to add *biological* factors—genetic predispositions, for example—as a third direct determinant of behavior (and perhaps of certain personality predispositions as well). Ideally we would want to include such factors and deal with them; I am simply not competent to do so.

5. This section draws heavily on an essay written shortly after I moved to UCLA in 1969, which served as a "charter" for our new program in "Behavioral Anthropology." With its strong critique of the "poverty of culture," this essay (Graves 1972b) provides a good representation of the thinking of this brash young man at mid-career.