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Ethnography

Charles O. Frake

Introduction

Deficiencies in the way anthropologists practice ethnography stem not only from the very real difficulties inherent in fieldwork, but also from a failure to appreciate that good ethnography is an exercise in the construction of cultural theory. Ethnography is the science — and art — of cultural description. I believe we should eliminate the condescending "mere" that often occurs before "description" in anthropological discourse. Over the past decade the theoretical payoffs inherent in practicing ethnography and in worrying about ethnographic practice have continued to be demonstrated by many investigators with varied disciplinary affiliations and theoretical orientations. The present volume attests to that claim. Although these ethnographic efforts have not resulted in much noticeable agreement on a single coherent theory of culture, they have displayed some critical issues about the nature of culture and the limits of a culture in ways that other approaches to theory construction, dependent on a priori definitions of units and measures, have often obscured.

One who aspires to describe culture cannot avoid the fundamental issue: where to find it. Culture is to be found in the doings (and sayings) of people. The ethnographer goes to "the field" to capture these doings. The finally captured doing is typically enclosed in a phrase of the form: "the so and so's do such and such." The practice of ethnography has shown us that the referents of both terms of the ethnographic phrase, the subject so and so's as well as the predicated doings of such and such, are highly problematic.

How the so and so's get delimited — by the investigator and by themselves — is looming as a critical question for cultural theory, but it is a

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These remarks on ethnography were originally written in 1972 at the request of a National Institute of Mental Health Committee on Social and Cultural Processes. Although I did not refrain from presenting my own particular view of things, my discussion was directed at the Committee's presumed aim of improving the social science research support programs of NIMH. In this essay, with the interests of a different audience in mind, I have deleted sections of the original report dealing specifically with grant review problems and with the ethics of proposal writing, but, otherwise, the original is mostly intact. I use this introduction to expand somewhat on my theoretical arguments and to assess the 1972 review from the perspective of a decade later.

question which I sidestep here in a manner made familiar by ethnomethodologists: The ethnomethodologists' so and so's are called "members." I call mine "natives." I think they would recognize each other.

The basic issue with the description of doings hinges on whether or not they can be captured as *cultural objects* in categories preconstructed by investigators who, in their scientific sophistication, know better than the natives what the natives are doing. This so-called objective reporting of "what's really there" (in spite of what the natives might think) is seen by some investigators as necessary for the cross-cultural comparability upon which grand theory depends. These are investigators who would, in spite of Geertz's classic treatment of the phenomenon (this volume, pages 37–59) still record and count eye twitches as, say, objective measures of energy expenditure — never mind whether, for the natives, they are winks or blinks. Never mind, in other words, what the natives think.

The opposite point of view is that what the natives think is what in fact constitutes as cultural doings and cultural objects the body movements, vocal noises, and material artifacts produced by humans. Culture, the object of our description, resides within the thinking of natives. But thinking — and we must mean thinking in the broadest sense of what people think about, believe, ponder, and wonder, as well as how they calculate and infer — can only be seen through behavioral displays in social contexts. The human thinking that pervades these displays is never transparent; it always must be glimpsed through an interpretation of what some display might "mean." This opacity is as much a fact for natives as it is for ethnographers. Natives, too, must find out what is happening, what is getting done, around them. Natives must do ethnography in the conduct of their daily lives. It is in their social practice of ethnography, in their negotiated interpretations of their doings, that we find displayed the "thinking" that constitutes culture. And it is the natives, not the investigator, who must be accorded the privileged interpretation of their own cultural texts.

This conclusion addresses what remains the critical issue in ethnographic practice: if the success of an ethnography is a test of the cultural theory it implies, then how do we judge the adequacy of an ethnography? Much of the diversity and conflict in current cultural theory stem from varied attempts to meet or avoid this persistent issue. In spite of a number of perceptive recent discussions, the state of the art is little different today than it was a decade ago. Training in fieldwork techniques is, if anything, even more neglected. It is the elegance of the investigator's prose that still counts. The battle lines are drawn between advocates of the rich, juicy elegance of fine literature thickly packaged between hard covers, and those who push the lean, dry elegance of formal logic thinly sliced by Occam's razor. I see little prospect for abandoning this appeal to the rhetoric of our own discourse until we learn how to take the natives seriously as interpretive ethnographers of their own lives.

Ethnography as a Field

Anthropologists do not generally regard ethnography as a field of specialization comparable to areas such as social organization or political anthropology. To respond to a colleague's question "What's your field?" by saying "ethnography" evokes puzzled smiles. One does not ask of an anthropologist, "Is he an ethnographer?" but, "Is he a good ethnographer?" The professional ideal is that every anthropologist¹ does ethnography periodically. Some have built brilliant professional careers relying largely on other people's ethnography, but they lose points for such practices. Ethnography underlies just about every tradition of investigation within anthropology. Labeling the process through which an investigator confronts his or her primary data as "ethnography" is what best distinguishes an anthropologist from companion social/behavioral scientists.

Most research in anthropology somehow involves ethnography, but few studies focus on ethnography itself as a methodological or theoretical topic of investigation. Although the ideal anthropologist is a "good ethnographer," he or she is also not "just an ethnographer." Most anthropologists still equate ethnography with fieldwork, data gathering, and description. The tradition of the profession tells us that data gathering and description are not enough. We must have theory. Anthropology must be science, not natural history. Proponents of what is sometimes called "the new ethnography" have tried in the last decade to elevate ethnography to a theoretical topic in its own right, comparable to the role of descriptive theory in linguistics. But these efforts have had limited success.

Ethnography as Fieldwork

However they regard the nature of ethnography, anthropologists would all agree that when one does ethnography the first step is to "go to the field." Doing fieldwork has a hallowed place in an anthropologist's career. The science of anthropology is built on data obtained "in the field." Given this revered role of fieldwork, two things are surprising: First, it is not at all clear just what kind of work counts as fieldwork; second, very little attention is paid to training people to do it.

What is fieldwork? A sociologist going from door to door with a questionnaire in an American community doesn't get credit for doing fieldwork, but an anthropologist doing the same thing in highland New Guinea probably would. However much the modern anthropologist tries to deny it, the fieldwork image in fact still strongly entails exotic settings, strange cultures, unfamiliar languages, and a certain amount of suffering by the investigator. The ideal fieldworker lives in the exotic setting, adapts to the

¹ These remarks exclude archaeologists as well as linguists who professionally identify as anthropologists (a dying breed).

culture (bravely overcoming the "culture shock"), learns the awesomely difficult language, and thoroughly enjoys the experience in spite of the suffering. Minimally, I suppose, fieldwork requires reasonably prolonged and continuous face-to-face contact with people in the natural setting of their daily lives. Certainly conducting laboratory experiments, mailing questionnaires, and culling census data from government documents do not count as fieldwork.

Once "in the field," what does an ethnographer actually do? Anyone who has sat on grant review panels knows that what he does is to employ "traditional ethnographic methods." Ask an anthropologist to name one. He will say "... uh ... well, there's the genealogical method, for example." It is true that the genealogical method is one procedure which the anthropologist can call his own and take some pride in. It is extremely useful to almost any kind of investigation; the way to ask questions is clear as is the ultimate objective and format. There is very little else in the way of standard field techniques that has these properties. Yet it is also true that, in the seventy years anthropologists have been employing this relatively straightforward and highly productive procedure, there has emerged no single best way to write down the answers to genealogical questions. Everyone figures it out individually. There has been no accumulated tradition of advances in data recording techniques. Advanced techniques such as genealogical recording for personal genealogies, kinship terminology, and household composition developed by A. K. Romney and his colleagues in the 1960s have been largely ignored beyond his circle of associates and students. The essential features of the Romney kin-type notation have been independently invented at least three times, and I have no doubt that somewhere in the Amazon jungle some ethnographer is inventing them again.

Of course there are a variety of devices, techniques, and methods available to the fieldworker (who takes time to learn them) for accomplishing particular tasks, things like cinematography, projective tests, and ethno-semantic eliciting procedures. But, although they have sometimes been advocated as such (or claimed by critics to have been so advocated) none of these techniques are substitutes for prolonged observation, participation in daily life, flexible and sensitive questioning, and skilled use of a wide range of techniques. There are no shortcuts to good ethnography, but there ought to be some way to insure accumulated improvement in the quality of ethnographic work.

There are a number of reasons, some of them inherent in the nature of fieldwork, for the poor development of ethnographic techniques. The conditions of fieldwork are so varied that what works well in one situation may be impractical or even dangerous in another. Being a good fieldworker depends on qualities of sensitivity, adaptability, and insight that are difficult to train for or to identify in advance. Nevertheless there are many techniques of general utility which can be taught but rarely are: ways of re-

ording and organizing field notes, elementary mapping, procedures for collecting plant and animal specimens, interviewing strategies, sampling techniques, etc. No amount of sensitivity, adaptability, and insight is of much use when the research problem requires knowing the areas of agricultural fields and the fieldworker has no idea how to find this out. Even techniques for recording and learning unfamiliar languages are not routinely taught to all students in most graduate programs. In fact systematic training in field linguistics is much less common now than a decade ago. These deficiencies in the development of ethnographic techniques are widely known, frequently lamented, and unremedied to this day.

Ethnographic Theory and Method

The common attitude that fieldwork is data collecting and analysis and "write up" take place after the fieldworker comes home underlies much of the inadequacy of ethnographic field technique. There is a lack of integration of data recording techniques, methods of analysis, and theory. Although they must be integrated in practice, these three phases of ethnographic work must be kept conceptually distinct (much of the controversy over "ethnoscience methods" arose through the confounding — both by practitioners and critics — of technique with method). Ethnographic technique comprises the procedures through which the researcher compiles a record of events. Ethnographic method comprises the procedures through which he or she arrives at an ethnographic statement from an ethnographic record. Ethnographic theory is concerned with such questions as criteria for evaluating competing ethnographic statements, motivations for decisions about what to record, units of analysis, and the relation between elicited and observed data. These questions of course entail a theory of culture — a theory of what it is that is being described. Doing ethnography is a way of testing and building cultural theory independent of the particular theoretical concerns that motivated the investigation. There is plenty of bad description in the ethnographic literature, but there is no such thing as good "mere description." A good ethnographic statement is an assertion about the nature of culture.

Unlike American linguistics, which tends at any given period to be dominated by a single paradigm that defines the nature of the phenomenon to be accounted for (e.g. as behavioral or mental), states the basic questions to be asked, specifies procedures of research, and demands common standards of evaluation, American anthropology exhibits little agreement among practitioners on these issues at any one time. A diversity of descriptive theory, and consequently a diversity of methods and techniques, can be expected. The problem for the immediate future is not who has the right theory but, in particular research projects, explicitly integrating method and technique with an ethnographic theory of some sort.

The tradition within anthropology that has given the most explicit attention to these problems is the one commonly called "ethnoscience" or the "new ethnography." The unity of this tradition is mostly in the eyes of its critics. Certainly it is not a coherent paradigm of research. At least three kinds of interests get labeled as ethnoscience: concern with ethnographic method and theory; an interest in cognition and decision models motivated by the notion that culture is a mental phenomenon; and a methodological interest in lexical semantics. Some investigators manifest all three interests, but many do not.

The impetus for the concern with ethnographic theory was expressed in the 1950s by structural linguists, whose field was then in its glory of paradigmatic dominance. Three aspects of the linguistic paradigm most attracted anthropologists. First was its insistence on strict relativism; the units of analysis and their arrangements had to be discovered in the systems being described. Second was its faith in operationalism. Third was the fact that the data on which the analysis was based (a set of texts) could be made public. This meant that a linguistic statement could be evaluated by referring back to the data. Ethnographers, on the other hand, used pre-defined units of analysis (as exemplified by the categories of the Human Relations Area Files) and nonexplicit and intuitive procedures, which were never based on a public set of data and could only be evaluated in terms of the persuasive power of the ethnographer's rhetoric.

In one respect, however, early ethnoscience radically revised the structuralist assumptions. Ethnoscience was avowedly mentalistic both in their interpretation of structural linguistics and in their conception of culture. In what is probably the closest thing to a charter for the ethnoscience tradition, Ward Goodenough (1957) argued that culture was not observable behavior, events, and artifacts but the knowledge of these elements in people's heads. He also stated that linguistics was the best method ever devised for getting inside people's heads. He was talking at that time about behavioristic structural linguistics. This mentalistic reinterpretation of structural linguistics provided the basis of the ethnoscience argument: culture is knowledge, and the way to get at knowledge is to apply linguistic methods both to cultural behavior and (especially) to meanings of words (structural linguistics eschewed the study of meaning as mentalistic). Theory and method were to be tested by doing ethnographic description. The joker in this package is that there must be some criteria for judging the success of an ethnographic statement. This problem has never been resolved.

This type of ethnographic theory arose in large part as a reaction to the standardization of ethnographic descriptive categories advocated by cross-cultural methodologists using the Human Relations Area Files. The most vocal attacks on the ethnoscience position came, however, from behavioristically oriented theorists. In the polemic and programmatic literature, a contrast was set up between those who believed in using verbal data to

make inferences about what is going on inside people's heads and those who believed in the objective recording of "real" events. Fortunately few ethnographers have followed either extreme in practice. No ethnographer with any sense would purposely ignore either what people do or what people say about what they do.

A more serious blow to ethnoscience came from the linguists. In the 1960s a bloody paradigmatic revolution took place in linguistics. The new paradigm of transformational grammar was avowedly mentalistic, a welcome change, but it also renounced discovery procedures, operationalism, empiricism, and relativism. A language, when probed deeply enough, was no longer a cultural phenomenon but part of speakers' innate and universal competence as human beings.

The fact that linguists changed their paradigm doesn't necessarily mean that the old paradigm was wrong for the questions it asked or for the ethnographic applications proposed by anthropologists. Paradigms come and go. But it does mean that ethnographic theorists can no longer justify their paradigm by pointing to linguistics, which is probably a good thing.

Problems and Prospects

The vital, yet seemingly insoluble, problem for the field of ethnography is the absence of reasonable means of evaluating ethnographic descriptions. If we cannot assess the adequacy of a description, how can we judge the cultural theory it entails? Ideally, of course, a description, being a theory about a particular case, should make predictions that can be tested. Apart from the fact that there is little agreement about what should be predicted (behavior or native interpretations of events?), there is rarely, if ever, any realistic possibility of systematically testing ethnographic statements. Short of testing one could hope that ethnographers would endeavor to make as much of their descriptive record public as is possible and be explicit about their methods of deriving an ethnographic statement from that record. But this often-made appeal doesn't really help very much. Good ethnographies don't result from routine application of operational methods to a set of data. Like any exercise in theory construction, ethnography requires insight, intuition, guesswork, and reliance on recollections of events beyond what is set down in field notes. The way anthropologists in fact evaluate ethnographies is in terms of the ethnographer's rhetorical persuasiveness, reputation as a "good" ethnographer, and the relevance of his or her case to theoretical problems of current interest. I see little realistic prospect for improving this situation other than increasing the quality of ethnographic training so that readers can have more confidence that any given ethnographer will be a good ethnographer.

It is now clear that neither the structural nor the transformational linguistic paradigm provides an adequate basis for an ethnographic theory. Both

paradigms restrict their domain to sentences isolated from wider linguistic and social contexts; both assume a homogeneous speech community; and both are concerned only with the acceptability, not the frequency, of events. A number of linguists have been giving attention to discourse beyond the sentence, to social context, and to variability and frequency, promising further revisions of the linguistic paradigm, one hopes in ways that will make linguistic and ethnographic theory more relevant to each other. The study of everyday social situations as contexts of behavior is an increasingly popular and promising field of investigation in sociolinguistics (much of which is done by anthropologists), anthropology, and sociology (where ethnomethodology presents what is in effect an ethnographic theory). At the other end of the scale, reconsideration of the macrounit of study, a culture, promises to reshape ethnographic theory. We talk about "a culture-bearing unit" sharing a "cognitive code" which it passes down through the generations; yet in a number of recent studies researchers have shown that cultural boundaries are not simply the outcome of differential transmission of tradition, but are also produced and maintained in interaction between social groups. Ethnographic theory must account for these boundaries. To do so it must assume a perspective that encompasses a number of neighboring "cultures," a regional perspective. Several investigators, especially in the field of economic anthropology, have been focusing on regional systems. This perspective poses problems not only for ethnographic theory, but also for method and technique in a multilingual, multicultural context.

The most telling indictment against the new ethnography is that it has not been notably successful in producing ethnographies. Like current linguistic theory, it has succeeded in revealing the complexities of relatively simple and restricted domains to such an extent that it is difficult to envision anything like a complete description of a total system under the paradigm. For those who see the value of ethnography as being limited to theory testing, ethnography in the old sense of going out and recording for posterity what's there loses its justification. I am enough of an old ethnographer, however, to be reluctant to dismiss our obligation to record the range of human cultural variability in some fashion. Current theorists rely heavily on data from old ethnographies written with very different theoretical objectives. There is no reason to suppose that future theorists will be any less dependent on current ethnographic statements. Yet the ethnographic record of the world is woefully incomplete. Almost every author of a research proposal in anthropology, no matter what the problem or where the field site, claims he or she will make a contribution to a poorly understood problem in a little-known area of the world. In most cases such claims are perfectly true — especially when made in reference to our own society.