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Ethnographic text formation processes

Abstract. Although the textualist critique of ethnography has challenged the possibility of science in cultural anthropology, insights provided by that critique are crucial for the further development of a scientific approach to the discipline. The value of the textualist critique of ethnography for the development of scientific ethnology can best be seen through an analogy with archaeology. Just as archaeologists' ability to reconstruct the past has been enhanced, not undermined, by a detailed understanding of archaeological site formation processes, so can ethnologists' ability to understand patterns within and among human societies be enhanced through a better understanding of ethnographic text formation processes. Key elements of the textualist critique of ethnography, including an emphasis on reflectivity, multisocality, and the process of writing ethnography, are great aids in the elucidation of ethnographic text formation processes.

Key words. Ethnography – Ethnology – Postmodernism – Science – Textualism

A naive realistic approach to anthropology would assume that cultural differences were objectively directly available for any and all observers to "see".

There would be no awareness of methodological problems, no recognition of the problematic character

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of knowing about another culture. Such an innocent epistemology could survive neither the experience of extensive fieldwork nor the comparison of interpretations between students of "the same" culture. As in other fields, the rejection of naive realism is made first by a move to complete subjectivity. At this stage, there is recognition of the fact that knowing involves the knower, is shaped by his characteristics. This recognition goes to the extreme of rejecting any other component, i.e. of denying all objectivity to knowledge. The stage of critical or hypothetical realism shares with radical empiricism an emphasis on the subjective relativity of all knowing. It shares with naive realism the aspiration to objective knowledge, invariant over points of observation or instruments. But it recognizes that such constructions will be fallibly known, through a process of hypothetical models only indirectly confirmed. The conditions of confirmation will never provide certainty and will often be totally lacking. From this point of view, scientific knowledge is not immediately available for the asking, but requires very special settings (Donald Campbell, 1970: 69–70).

The cultural past is knowable, but only when the nature of the evidence is thoroughly understood (Michael Schiffer, 1987: xx).

The attraction of interpretive anthropology . . . is . . . its sophisticated inquiry into the nature of ethnographic reporting . . . the basis of all anthropological knowledge, pursued in whatever theoretical direction (George E. Marcus and Michael M. J. Fischer, 1986: 27).

In recent years, self-described postmodern anthropologists have offered a powerful and wide-ranging critique of the authority of ethnographic texts. Because ethnographic texts are seen by many as the ultimate foundation for all of cultural anthropology’s claims to knowledge, this critique has, in the minds of many (e.g. Tyler, 1986), laid waste to the idea of a scientific cultural anthropology. Following Auinger (1995), I will refer to this as the “textualist critique” due to its foundations in the hermeneutic tradition of textual criticism in the humanities. Those anthropologists who are unhappy with the idea of anthropology without science and science without anthropology have for the most part chosen to try, in one way or another, to resurrect the authority of ethnographic texts. This has been attempted by, for instance, redefining science as

“pattern recognition” (Carrithers, 1990), pointing out the mundane, bureaucratic details behind the ethnographer’s craft (Dauber, 1995) and incorporating both formal and interpretive methods in ethnographic fieldwork (Auinger, 1995). Another approach would be to recognize that cultural anthropologists are not alone among scientists in having to contend with data that come to them as the product of processes that may be poorly understood and that may impose on the data confusing patterns of their own. One field that shares this characteristic with cultural anthropology is archaeology. This article explores this similarity, focusing on an analogy between processes of archaeological site formation and those of ethnographic text formation.

The central contention of this article is ironic: although the textualist critique has been instrumental in tarnishing the image of scientific cultural anthropology, the further development of scientific anthropology depends in a crucial way on the constructive incorporation of selected textualist insights into anthropological metatheory. Just as archaeologists have found that in order to do reliable scientific studies of archaeological sites it is essential to consider the processes that formed the sites, so must anthropologists intent on scientific analyses of ethnographic data pay close attention to the processes that formed the ethnographic texts. Thus, the study of ethnographic text formation processes should have a position in scientific ethnology analogous to the position of the study of site formation processes in archaeology. Much of the textualist critique of ethnography, particularly the emphasis on reflexivity, the emphasis on writing, the challenge to ethnographic authority, and the emphasis on intracultural variation and multivocality, can be seen in this light as an effort to elucidate the processes of ethnographic text formation.

Locating science in anthropology

In this article anthropology is referred to as a “science” in the sense of a “historical” science, not in the sense of a “Newtonian” or “theoretical” science (Aberle, 1987; Hayek, 1955; Kroeber, 1963; see also Hammersley, 1992: Ch. 2). Other historical sciences are astronomy, meteorology, and geology. The “Newtonian” (Aberle, 1987) or “theoretical” (Hayek, 1955: 66–7) sciences deal with general rules of very general phenomena, like the behavior of
elementary particles, chemical reactions, or the laws of genetic inheritance. Historical sciences do not seek such broad principles, but instead use the insights of the theoretical sciences to explain the specifics of local, particular situations. Anthropology, even when it involves sweeping cross-cultural analyses, is a historical science, using the insights of the theoretical sciences to explain human behavior, beliefs, and social organization in specific contexts, past and present.

This is not to say that all of anthropology is a historical science or that it must be the goal of all anthropologists to make it a science. On the contrary, humanistic approaches are crucial to the full development of the whole discipline, even, paradoxically, to the full development of the scientific part of it.

If some part of anthropology can at least hope to be a (historical) science, which part is that? To answer this question, it will be useful to resurrect an old-fashioned but still serviceable distinction between ethnography—the detailed description of the behavior and culture of groups of people—and ethnology—the analysis of such detailed descriptions with an eye toward broader generalizations and syntheses. Ethnology in this sense is not limited to cross-cultural comparisons, but also includes the application of ideas from theoretical sciences and from humanistic disciplines to ethnographic data from a single society. If this definition is accepted, then many ethnographies will be seen to contain a fair amount of ethnology as well. Chagnon’s work on the Yanomami, for instance, while clearly ethnographic, also contains his ethnological attempts to explain Yanomamö behavior using insights from such theoretical sciences as evolutionary biology and ecology (Chagnon, 1992). This distinction is analogous to that made by Sperber (1985) between ethnography and anthropology, but given the de facto structure of the discipline in which ethnography is generally seen as a subset of anthropology, my terminology seems clearer.

The failure to maintain a distinction between ethnography and ethnology has contributed to the confusion and discord surrounding the textualist critique of anthropology. The focus of that critique has been on ethnography and particularly on the possibility of “scientific” ethnography. Some authors have used the textualist critique of ethnography as the basis for a rejection of the possibility of science in anthropology as a whole on the grounds that the impossibility of objectivity in ethnography undermines scientific anthropology’s claims to objectivity (e.g. Tyler, 1986). That radical position has inspired advocates of science in anthropology to refine their arguments, and some have taken the approach of defending the possibility of a “scientific” ethnography (e.g. Auinger, 1995; Carrithers, 1990; Kuper, 1993). This reaction is understandable but, in my view, wrongheaded. The widespread perception that the textualist critique of ethnographic authority challenged the scientific status of cultural anthropology is based on a misunderstanding. Specifically, it is based on the idea that the scientific status of cultural anthropology rests on ethnographic authority. However, the scientific status of cultural anthropology was never truly challenged by the textualist critique of ethnographic authority because it never truly rested on that authority.

What makes a field a science is not to be found in the way it collects its basic data, and ethnography is the wrong place to look for science in anthropology. 4 Meticulous, replicable observations do not make a field a science, and personal, idiosyncratic, and even poetic observations do not make a field something other than a science. As Kuhn (1962) pointed out, before the scientific revolution in the physical sciences sparked by Newton, there were plenty of people making detailed and careful observations of the physical world without their work adding up to something that could rightfully be called “science”. By the same token, however, the reason that the physical sciences were sciences after Newton is not because people continued to make meticulous observations, but rather because they had acquired a scientific paradigm that guided their research. “Science” is located not in the methods of data collection, but in the way questions are phrased, ideas are tested, and knowledge claims are made. 5 To use another analogy, astronomy is not a science because astronomers have sophisticated equipment and make careful observations of the heavens. Astronomy is a science because of how astronomers decide which problems to address, how they phrase those problems, and how they relate their data to them. Research priorities of astronomy and other sciences are typically set by a dominant paradigm (sensu Kuhn, 1962), questions typically are expected to be phrased as falsifiable hypotheses (sensu Popper, 1959) and data are related to hypotheses using canons of operationalization that are fairly standardized within each field.

Similarly, anthropology, or more specifically ethnology, can aspire to be a (historical) science, but ethnography cannot. “Scientific ethnography” is a chimera, not to be made real by redefining
“science” to mean, for example, “pattern recognition” (Carrithers, 1990). A science of ethnology, on the other hand, is possible, and is not made impossible (just more difficult) by the nature of ethnographic data. The contribution of the textualist critique of ethnography to the project of scientific ethnology is to clarify the ways in which ethnography makes that project a greater challenge than once was thought. It also allows us to begin thinking about how we might make ethnography more helpful to ethnology without making it less valuable in its own right (see Sperber, 1985 for a similar point).

Kuper (1993) provides a concrete example of how “science” happens in anthropology not in the details of an individual’s fieldwork and writing, but in the interactions among anthropologists and in how they analyze data. Kuper was one of many prominent scholars to do fieldwork in the Kalahari during the 1960s, and naturally a dialogue developed among Kalahari specialists. He describes how they discussed their findings, checked one another’s facts, compared each other’s interpretations, and so on. It is clear from Kuper’s account that something scientific emerged from this discourse, not because of the details of each individual’s fieldwork methods, but because of the nature of the discourse itself. Science, in this sense, is located in anthropology not in the field or in ethnographic writing, but in the social process before and after those activities and in the analysis of data that happen to be included in ethnographies. A science of ethnology is only possible, however, to the extent to which we understand the source of its data, i.e. fieldwork and ethnographic writing. Just as astronomers must have a clear understanding of atmospheric disturbances and of the workings of their telescopes and other instruments, so must ethnologists have a clear understanding of the process that goes into the creation of ethnographic texts.

Archaeological site formation processes

Particularly among post-processualist archaeologists, it is common to refer to archaeological sites as “texts” and to describe the archaeologist’s task as “reading” such “texts” (e.g. Hodder, 1991: 153; Tilley, 1993: 12). Some sites are indeed deliberately constructed in a way analogous to the way a text is deliberately constructed. The Great Death Pit at Ur, for example, was essentially a work of propaganda, a collection of corpses, objects, and structures carefully assembled and then buried in a way that displayed the “official transcript” (Scott, 1990) of the society (Dickson, 1993: 125). In this article, this metaphor is turned on its head. My contention is not: that archaeological sites are like texts (though they may be in some ways) but that ethnographic texts are like archaeological sites. More specifically, the study of ethnographic text formation processes can play a role in scientific ethnology corresponding to the role played by the study of site formation processes in scientific archaeology.6

It may be a caricature to say that, before the development of a concern with site formation processes, archaeology was dominated by a naive “Pompeii premise” that held archaeological sites can be read in a straightforward manner (Binford, 1981). However, this is a useful caricature for the purposes of this article since it is analogous to the caricature of anthropology’s theory of ethnography before the advent of the textualist critique, i.e. that the process through which ethnographies are created is simple and unproblematic. Although neither caricature accurately reflects the former state of affairs in either discipline, both accurately convey an understanding of the sea change that subsequently occurred in each field.

Schiffer (1987) provides a summary of the development and current state of the study of site formation processes. The key development was the realization that not only are artifacts subject to differential rates of decay and degradation, but their preservation is also influenced by processes that impose their own patterns on the data. Schiffer groups these processes into two broad categories: cultural formation processes and environmental formation processes. Cultural formation processes, or c-transforms, are concerned with how people’s behavior helps create archaeological sites: how items are discarded and reused, how the dead are treated, how goods are cached, how structures are abandoned, and so on. Environmental formation processes, or n-transforms, include the ways in which nonhuman factors create patterns in archaeological sites, e.g. geophysical processes like erosion, soil deposition, seismic activity, and biological processes such as the effects of micro and macroorganisms on the arrangement and preservation of artifacts in sites. Some of these “transformational” processes have been
clarified by ethnoarchaeology (e.g. Binford, 1978; Gould, 1980), experimental archaeology, and geoarchaeology (Waters, 1992). Of course, archaeologists already understood that preservation is never perfect and that the archaeological record is subject to systematic distortions and biases (e.g. Ascher, 1968; Collins, 1975; Cowgill, 1970). This transformational approach differed from the earlier approach, which Schiffer labels “entropic”, in its focus on how archaeological sites are reorganized, not just disorganized, by site formation processes (Dunnell, 1990: 223).

The analogy between the role of the study of ethnographic text formation processes in scientific ethnology and that of the study of archaeological site formation processes in scientific archaeology breaks down, however, when we come to how Schiffer and other advocates of the new, transformational approach presented the implications of the new insights for archaeology. What they did not claim is that a realization of the complexity of the processes that go into the creation of archaeological sites makes scientific archaeology impossible. Quite the contrary: they claimed that in order for archaeology to be a true science, it must incorporate an appreciation of site formation processes, for without such an appreciation they will never be able to accurately reconstruct past behavioral patterns. As Schiffer wrote, “My approach is not to underscore the difficulty in reconstructing the past, but to emphasize that this difficulty will decrease as we begin to confront head-on the numerous problems which inher in the study of the past” (1976: ix).

This example holds lessons for cultural anthropologists grappling with the textualist critique of ethnography. Archaeologists’ realization of the need to understand site formation processes made their work more difficult, but it also increased the chances that they might accurately reconstruct the behavior that created the sites at the same time that it made them acutely aware of the limits imposed by their data on knowledge of the past. Similarly, the textualists’ realization that the ethnographic text formation process is far from the straightforward affair it was once thought to be makes the work of scientific ethnology more difficult, but a clearer understanding of that process promises to make it more likely that ethnologists will produce reliable answers to their key questions at the same time that it makes them more aware of the limits imposed by the nature of ethnographic data on scientific ethnology’s abilities to make knowledge claims.

Highlights of the textualist critique of ethnography

Key elements of the textualist critique for the study of ethnographic text formation processes include (1) multivocality/polyphony/heteroglossia and the closely related anti-essentialist view of culture; (2) reflexivity on the part of the ethnographer as one aspect of a broader critical examination of the process of fieldwork, including interview methods and relationships with informants; and (3) a challenge to the authority of ethnographers as writers.

Although it has long been recognized that cultures vary not only across societies but also within them (see Pelto and Pelto, 1975; Vayda, 1994) it was the textualists who brought great attention to this issue and enshrined it as a central problem of ethnographic fieldwork and writing. It contrasts with the earlier view that the job of the ethnographer was to take the diversity of voices found in the field and synthesize them into some internally coherent picture of the culture of this people or that. Some have suggested that this multivocality can be creatively enshrined in ethnographic texts. For example, Tyler (1985) proposes the Bible, a book with many authors with many different goals at many different times, as a possible model for “postmodern” ethnography.

Fieldwork is done by fieldworkers, who then become ethnographers. Who those people are has a major influence on what they do in the field and what they write in their ethnographies. That, in a nutshell, is the reason for the intense concern with reflexivity in the textualist critique of ethnography. As with multivocality, this idea has its precursors, such as the call for all fieldworkers to consider their “personal equation” (Werner and Schoepfle, 1987; see also Lemaire, 1991). But, again, it was the textualists who made reflexivity – the need for each ethnographer to reflect upon the very personal nature of the task and how one’s own characteristics influence how that task is accomplished – a central issue for all those concerned with ethnography. This concern has spawned a large number of very personal accounts of fieldwork experiences, the best of which address the issue of how the ethnographic text formation process is influenced by the ethnographer’s personal characteristics (e.g. Okely and Callaway, 1992; Rabinow, 1977). Reflexivity is, in a sense, just one aspect of a broader concern with the details of how individual fieldworkers approach fieldwork – with whom they talk, how they live, how they talk to people, how they record their observations and conversations, and so on. These sorts of concerns
are key elements of an understanding of ethnographic text formation processes, and they will be discussed in more detail below. Although reflexivity can easily deteriorate and become self-indulgent, the idea has garnered praise even from so strong a critic of textualism as F. G. Bailey, who applauds the "deconstructive habit of scrutinizing other people's ethnographies for evidence of bias, intellectual or otherwise, [which] reinforces an entirely warranted demand for reflexivity, self-examination, self-criticism, and by implication, the production of something better" (1991: 113).

Ethnography is a kind of writing. This observation, trivial at first glance, has taken on tremendous significance thanks to the textualist critique. The insight that ethnographers are authors has been the starting point for some of the most important and insightful textualist work in anthropology (e.g. Clifford, 1988; Clifford and Marcus, 1986; Geertz, 1988; Marcus and Cushman, 1982; Marcus and Fischer, 1986; Van Maanen, 1988). The concern can be traced back to seminal works on authorship by Michel Foucault (1979) and Roland Barthes (1982) and Bateson's pioneering Naven (1958 [1936]) and it takes issue with the matter-of-fact, nonreflective style of writing that Marcus and Cushman (1982) label "ethnographic realism". The ethnographic realist style involves such techniques as generalizing particular instances to overall patterns - not "they did this ritual with these details on this day", but "this ritual goes like this: . . .". This style, though superficially "authoritative", just obscures the ethnographic text formation process:

For better or for worse, this discontinuity has meant that it is impossible to work back from final account to original fieldwork enterprise in anything like the way a chemist can work back through an experiment reported by another chemist. The recent spate of experimentation has tried to bridge this gap essentially by shaving down the style of writing to the presentation of the particular evidence obtained from the field, accompanied by a self-conscious working out of generalizations. (Marcus and Cushman, 1982: 35)

The question arises, then, of what kind of writing ethnography is. Clifford (1986: 6) responds that it is "artisinal" (arguably a better term than Geertz's "fictional") in the sense that it creates artifacts. This implies that ethnographic writing is, or can be, a craft as much as an art, meaning that ethnographers are not so much "writers" as "wrights", analogous to wheelwrights, cartwrights, and playwrights. Ethnographers as craftsmen are free to experiment with their methods, and so the textualist critique has led to important experiments in ethnographic writing, such as texts in which interview transcripts and the voice of key informants take center stage away from the no longer authoritative voice of the ethnographer (e.g. Crapanzano, 1980). Of course, it would be wrong to say that there has emerged any consensus about how exactly to proceed with reforming ethnographic writing. Some (e.g. Hastrup, 1992) advocate retention of the convention of the ethnographic present, while others oppose it. One group reflexively foregrounds the ethnographer, while others (e.g. Krieger, 1983; Lizot, 1985) foreground the statements of informants and attempt to sublimate as much as possible the voice of the author. Clearly, not all of these experiments are equally helpful to the effort to elucidate ethnographic text formation processes.

Deconstructing for science: the study of ethnographic text formation processes

Although the analogy between archaeological sites and ethnographic texts is new, the idea that ethnographic texts are the result of a process of formation is not. One approach that resembles in some ways the study of ethnographic text formation processes is Raoul Naroll's data quality control method (Naroll, 1962; see also Divale, 1976; Naroll, 1970; Rohner et al., 1973; Rohner et al., 1982). As Naroll himself wrote, the philosophy behind the approach was "simple. Field reports are classified by conditions of observation. Circumstances are selected that seem likely to affect the reliability of field reports and to produce either random error or systematic bias" (Naroll, 1962: 14). Reports are then coded according to the favorability of the circumstances in which they were gathered, and statistical tests are made to look for differences between those thought to be more or less favorable. For example, are there differences between reports according to how long the ethnographer spent in the field? Naroll found that there are: fieldworkers who spend more time in the field are more likely to report the presence of witchcraft. This approach is analogous to long-standing concerns in other behavioral and social science with experimenter and interviewer effects (e.g. Hyman, 1954; Rosenthal, 1976).

Similar in spirit to the data quality control method of Naroll is Donald Campbell's triangulation method (Campbell, 1970). Campbell notes that the traditional one observer—one culture method in anthropology is inadequate since it makes ambiguous whether any
particular thing included in an ethnographic report is the product of the culture or of the fieldworker. As an alternative, Campbell proposes that ethnographies be prepared in groups, with ethnographers from two different cultures both studying the same two other cultures. Anything that one ethnographer records in both societies that the other records in neither society can be attributed to the ethnographer, not the society. Anything that both ethnographers record about a particular society but that neither records about the other society can be attributed to the culture "as objectively known" (Campbell, 1970: 70). Campbell's method is different from Naroll's in that it approaches the issues at the onset of fieldwork rather than after the fact, but the underlying spirit is the same, i.e. that there is a basic, shared truth about each society that can be known if enough careful studies and statistics are done. A more fine-grained approach is taken by the informant accuracy approach (Bernard et al., 1984; Freeman and Romney, 1987; McNabb, 1990) which looks at the accuracy (in terms of their ability to recall the past) not just of fieldworkers but of individual informants.

Although all of these methods do shed light on the ethnographic text formation processes, they are somewhat different in spirit from the approach proposed here. To understand in what way they are different, let us return to the analogy with archaeological site formation processes. Prior to Schiffer and the development of a transformational approach to site formation, the emphasis was on entropy, i.e. on decay and degradation. Although decay and degradation remain important elements of archaeologists' understanding of site formation, what Schiffer et al. added was the realization that sites also were subject to forces, both behavioral and environmental, that create their own patterns in the deposits. Analogously, the approaches of Campbell, Naroll, and so on, while getting at some of the problems inherent in ethnographic text formation, leave intact cultural anthropology's equivalent of the "Pompeii premise", i.e. the idea that ethnographic texts are straightforward accounts of what actually happens in specific settings, even if they are sometimes missing details or are inaccurate in statistically specifiable ways. What the textualist critique of ethnography has made clear is that there is more to the ethnographic text formation process than this. In particular, it is now well understood that the particular characteristics of fieldworkers, their experiences in the field, and how they write up their findings do not just introduce "sampling error" and "bias", they introduce patterns of their own making.

Fortunately, these patterns themselves can be revealing, just as in archaeology. For example, an understanding of the role of reuse and refuse disposal in the creation of archaeological sites can reveal a great deal about the way a particular people lived and something about how they viewed their world. Similarly, some of the things that make ethnography such a problematic business, such as Rashomon effects among informants, introduce patterns into the data that are themselves potentially revealing of underlying cultural and social patterns. The label "Rashomon" comes from a film by Akira Kurosawa in which the same event is retold several times, in strikingly different ways, by different witnesses. Rashomon effects among ethnographers, too, can be revealing, and not just of the ethnographers themselves, as Campbell's triangulation approach would have it. On the contrary, sometimes different ethnographers report different things because more than one thing is "true". Incorporating and using this sort of insight, rather than just statistically controlling for such problems, is a challenge for the ethnographic text formation processes approach.

The idea of studying ethnographic text formation processes shares with the textualist critique of ethnography a belief that ethnographic texts are not straightforward mirrors of reality, but rather representations of a reality that we can no longer experience directly. Such texts are all we have to go by, and so it is imperative that we examine more fully the process that creates the texts. To do this, we need to make the ethnographic text formation process itself the focus of our research. This echoes a call made long ago for "an ethnography of ethnography", "making explicit and public the procedures by which research is accomplished and interpretations are derived", and resulting in "a description of exactly how ethnography is done, how insights are derived, how judgments about data are made" (Berreman, 1968: 368-9).

Ethnographic texts begin with fieldworkers, and much good could be done by further developing Naroll's work on the effects of variations among fieldworkers and fieldwork experiences on ethnographic texts. Interesting issues might include whether the fieldworker lives with the local people or apart from them, whether (and how well) the fieldworker speaks the local language, uses an interpreter (see Pool, 1991) or uses an intermediary language, and
so on. Unlike the data quality control method, the goal of ethnographic text formation process research on this topic would be not to identify "good" and "bad" methods in terms of how accurately they represent a refined cultural reality, but simply to try to understand the effects of such methodological variations on the resulting texts. For example, it is not possible to say that either using an interpreter or relying on one's own linguistic abilities is always the correct choice since the two methods will reveal different things, but it may be possible for fieldworkers to keep track more carefully of which insights are gained with and without interpreters, and that may shed light on how ethnographic texts are created. Similarly, the choice of whether to live intimately among the subjects of one's study or somewhat apart from them is one that most fieldworkers must face, but there is little existing literature on the effects of that choice on the resulting ethnographic texts. The existing bias in the discipline would be in favor of greater immersion, a bias based on time-tested understandings of how, for example, immersion increases the likelihood of unexpected insights. But surely it is not simply the case that living intimately with the locals produces a higher resolution image of the same basic ethnographic reality. Rather, how one chooses to live in the field must produce changes in the ethnographic reality itself, and this complicates the choice of living situation: depending on one's goals in the field, it may well be that living somewhat separate and thereby, perhaps, providing a context for more private conversations with informants would be more revealing of some aspects of ethnographic reality than would living intimately among them.

Another way to reveal ethnographic text formation processes would be to experiment with the method at the heart of most anthropological fieldwork: interviewing. Briggs (1986), Mishler (1986) and others have challenged the assumption in most western social science research that interviews are an appropriate and legitimate means of gathering data. Briggs (1986) notes that although interviews are a familiar form of discourse in our society, in many other societies they are but one highly artificial and strange form of metacommunication and may amount to a form of "communicative hegemony" (1986: 121). The imperialism of our reliance on interviews was also noted by Pratt (1986: 41), who said of Evans-Pritchard and Maybury-Lewis, "Both writers complain, for instance, about the impossibility of having private conversations with informants, as if private conversation, once baptized as field method, ought to be culturally possible everywhere".

Briggs argues that the components of interviews, "interviewer, respondent, audience, message form, reference, channel, space, social roles, interactional goals, social situation, and type of communicative event", "should be examined in terms of their role in shaping the meaning of what is said by both parties" (Briggs, 1986: 100–1). Out of this list, the audience present at the interview may be an element that is easy to record, often manipulable, and likely to have major and clear effects on what is said and how it is said. So-called audience effects are sometimes noticed by ethnographers, but usually they are seen as problems, with the "perfect" interview being seen as one conducted, in keeping with the social science's ideal of the interview, in complete privacy. Chagnon, for example, found that only by being the sole member of the audience when conducting genealogical interviews with his Yanomamö informants could he hope to get accurate information from them (Chagnon, 1992: 20 et passim). Occasionally, however, ethnographers notice that audience effects can provide an important window on social and cultural reality. For example, Scott (1985: 284–9; 1990: ix) put the audience effect to a constructive use in his efforts to understand the discrepancies between statements made by Malay peasants in private settings (the "hidden transcript") and those made in settings in which members of the elite are present (the "public transcript").

To more fully understand audience effects and to exploit them for the insights they provide, it may be fruitful to make it a more standard practice among fieldworkers to record the audience present at interviews and more informal settings when notes are taken, and to systematically vary the audience present at interviews in ways that are significant given the particular research question. Auinger (1994) accomplished this by varying the identity of the interviewer, with the same 65 individuals being interviewed two and sometimes three times on the subject of food taboos by both Auinger himself and a native assistant. Another technique would be to conduct group interviews while systematically varying the composition of the groups, formalizing the method used informally by Scott (1985, 1990).

Like audience effects, the Rashomon effect is another phenomenon often seen as a problem for fieldworkers intent on obtaining "accurate" or "reliable" accounts of events that could instead be
fruitfully exploited as a window on both another culture and ethnographic text formation processes: how does the content of ethnographies differ according to who is interviewed? A promising way of exploiting the Rashomon effect constructively might be to manipulate systematically the identity of informants so as to preserve the nonessential nature of the culture under study and the multivocality of its bearers. A more expensive way to create Rashomon effects would be to follow Campbell’s (1970) suggestion to have different ethnographers study the same society. Of course, there have been many famous instances in which ethnographers of the same society have disagreed (e.g., Mead and Freeman on Samoa, Mead and Fortune on the Arapesh, Mead and Gewertz on the Tchambuli/Chambri, Malinowski and Weiner on the Trobrianders, Redfield and Lewis on Tepotzlan, Goodenough and Fischer on Truk). Heider (1988) suggests, in very much the same spirit as the study of ethnographic text formation processes, that in such cases “a positivist search for truth versus error may be less fruitful than a constructionist examination of the research itself” (for more on the Rashomon effect, see Condron and Bode, 1982; Crissman et al., 1989; Frankel, n.d.; Weingrod, 1981).

Another variable in field methods that could be profitably manipulated in order to shed light on ethnographic text formation processes is the extent to which the data come from interviews versus observations. The “ethnographic realist style” (Marcus and Cushman, 1982) was to blend the two together, e.g., by phrasing information derived from informants’ statements as if it came from observations (“they do X” rather than “So-and-so told me that they do X”). That style obfuscates the ethnographic text formation process and rests on an ignorance of sizable literatures on discrepancies between what people say and what they do (e.g., Cancian, 1975; Cronk, 1991; Deutscher, 1973; Hrdy, 1990) and on lying informants (e.g., Anderson, 1986; Blimes, 1975; Bleek, 1987; Chagnon, 1992; Chen and Murray, 1976; Dean and Whyte, 1958; Freeman, 1983: 289–90; Frelich, 1970; Gilsenan, 1976; Montandon, 1981; Nachman, 1984; Passin, 1942; Salamone, 1977).

More systematic attention to the fit between statements and behavior and careful attention to the sources of ethnographers’ generalizations would contribute a great deal to the study of ethnographic text formation processes. The results might be surprising. Nachman (1984: 537–8), for example, found that his longevity in the field and familiarity with his subjects did not necessarily decrease the frequency of lying. On the contrary, his assistants informed him that people he seldom dealt with tended to give him accurate information but people he frequently dealt with got tired of his way of asking questions and tended to give him increasing amounts of misleading information. One wonders how many figments of the imagination of bored informants have been enshrined in ethnographies as “facts,” with each such instance representing an unexploited opportunity for insights into the culture of the informants even greater than that falsely afforded by the lie itself.

The bridge between fieldwork and ethnographic writing is formed by fieldnotes and, following Ottenberg (1990), “headnotes,” or remembered but unwritten observations. The exploration of how fieldnotes are created, organized, and turned into the raw materials of ethnographies is already well under way (e.g., Dauber, 1995; Sanjek, 1990a, 1990b; Silverman, 1972; Wolf, 1992). In the past, the anthropological attitude toward fieldnotes has been rather like men in a locker room: the polite thing is not to stare, and certainly one never asks for a peek. The whole topic engenders a certain self-consciousness and doubts about the adequacy of one’s own fieldwork (Bond, 1990). The new attitude exemplified by, for example, the volume edited by Sanjek (1990a) needs to spread if we are to make open and clear the ethnographic text formation process. The recent work on fieldnotes invites us, essentially, to stare and to be stared at.

In some circles, there is already a long-standing tradition of openness about fieldnotes. For example, Melville Herskovits required his students to send him copies of their typed notes for him to read and comment on, and at least one student of Herskovits, John Messenger, reportedly followed the procedure with his own students (Ottenberg, 1990). Although it is not feasible to publish an appendix of raw fieldnotes after every ethnography, partly due to the sheer expense and partly due to the ethical problem of being open with information often collected with promises of confidentiality, quantitative data do lend themselves to public dissemination because they are both compact and easily made anonymous. Examples include Chagnon’s detailed appendix to *Studying the Yanomamo* (1974) and the series of monographs edited by Allen Johnson on time allocation (e.g., Johnson and Johnson, 1989).

Through a careful comparison of an ethnographer’s fieldnotes, the resulting ethnography, and perhaps also a personal account of the fieldwork experience, it should be possible to reconstruct the
“life history of a ‘fact’”: where it began, presumably in an informant’s statement or an ethnographer’s observation, how it grew and took shape, and how it finally was deposited in the soil of the ethnographic text. Silverman (1972) provides us with just such a life history, and in so doing provides an exemplary account of the ethnographic text formation process. He recounts the history of his efforts to understand postmarital residence patterns among the Banabans of Fiji, including the history of his fieldwork on the islands, his fieldnotes, and his various attempts to write up accounts about the phenomenon, including very candid assessments of how at times he has overstated his case, ignored variability, and so on. More recently, Wolf’s *A Thrice-Told Tale* (1992) has presented the life history of an ethnographic observation by publishing fieldnotes and a professional article side by side with the even more experimental form of a fictional short story, all about the same event. Although it is clear that Silverman’s and Wolf’s examples cannot be followed for every single detail published in ethnographies, they do clearly demonstrate the value and the possibility of being more keenly aware of the ethnographic text formation process. As Silverman observes, “within and behind those tedious, ethnographic one-liners – the X’s are patriarchal, the Y’s plant taro, the Z’s appease their ancestors – there is often more than meets the eye” (1972: 204).

Fieldnotes are the first step in the ambiguous and mysterious process known as “writing up”. How one “writes up” one’s headnotes and fieldnotes depends upon one’s goals, and the goals of ethnography are many. In addition to the recently neglected goal of providing information for ethnological analysis, the goals of ethnography include interpretation, both in the sense of translation and in the sense of creating an understanding, and evocation (Tyler, 1986), a quality especially important when choosing texts for classes. One’s approach to writing will depend upon one’s goal, but here I will focus on the goal of providing information that will be of some use in ethnological analysis, whether the ethnographer’s own or someone else’s. In order for such analysis to be possible, the processes contributing to the creation of ethnographic texts need to be made more plain, open, and obvious.

Recent years have seen a lot of experimentation in the strategies for achieving these various goals of ethnography. Some of them, like ethnographic fiction, may create texts that aid cross-cultural understanding and evoke in the reader a vivid sense of experiencing another society, but such a style, unless weighted down with annotations, would be unlikely to shed much light on ethnographic text formation processes. Another experiment that at first glance might appear to lay open text formation processes is Krieger’s attempt to deal with the problem of ethnographic authority by removing the author’s voice as much as possible: “I paraphrased my data even more closely and used my interview notes as the only source for writing up my account . . . I therefore became almost absent as a narrator” (Krieger, 1983: 191). Although this appears to remove the author as an element in the text formation process, of course it does no such thing. In fact, although it produced a very readable and moving book, Krieger’s elimination of her own presence in the text formation process, as an interviewer, writer of fieldnotes, and organizer (if not author) of the text itself simply makes it all the more difficult to understand the details of the creation of the text. This does not mean that all ethnographic texts must be little more than fieldwork accounts, but making the textualists’ concern with reflexivity an everyday thing would go a long way toward making it clear how ethnographic texts are created. Whether this is to be done by including the author’s personal voice in the main text, in a separate text, or in a running commentary on the text is something that will depend on the specifics of each case.

There is a great deal of confusion regarding the presence of the author in the text and how “scientific” or “humanistic” the text is. Adherents to the “ethnographic realist” style of writing might argue, for example, that an objectively worded account, with few or no personal observations and where the author is little felt, is more “scientific” and hence more helpful to the development of scientific ethnology, while more personal, more reflexive accounts are more “humanistic” and less helpful to those attempting to do science. This is not the case. Reflexivity can be an important way to clarify the ethnographic text formation process and as such can greatly enhance the usefulness of an ethnographic text for scientific analysis. Furthermore, humanistically trained researchers do not always employ a very reflexive writing style, while scientifically oriented ones may include much of their own personality and personal experiences in their writing. Witness, for example, Jacques Lizot and Napoleon Chagnon on the Yanomamö. Lizot’s interests and approach are decidedly humanistic, but he makes a conscious decision to “recede into the background as completely as possible” (1985: xiv). Chagnon, never one to recede into the background, includes much about himself alongside many fine examples of
scientific analysis of ethnographic data (Chagnon, 1974, 1992). Chagnon's personal presence in his ethnographic writing, far from undermining its value for scientific ethnology, enhances it.

The textualists' concern with multivocality needs also to be made part of the ethnographer's routine in order both to better represent cultural reality and to make clearer the ethnographic text formation process. This can be a difficult goal to accomplish in writing ethnography since the limited length of the form itself requires ethnographers to trim away the babel of voices enshrined in fieldnotes. As mentioned above, Tyler (1986) has suggested the Bible, a book with many authors written over a long period of time, as a possible model for a "postmodern ethnography" that would better preserve the many voices of the field. The development of multivocality and intracultural diversity, coupled with a nonessentialist concept of culture, as themselves the focus of study for ethnographers will contribute to a clearer understanding of the ethnographic text formation process.

Sanjek (1990b) offers three "canons" or criteria for judging the validity of an ethnography which would also be useful in clarifying the ethnographic text formation process. First, theoretical candor: ethnographers must be open about their theoretical perspectives, which presumably guide the choices they make before, during, and after fieldwork. Second, the ethnographer's path: ethnographers should make explicit the pathway they take through the societies they study, including such details as who their informants were and how they were selected. Third, fieldnote evidence: it is important that the relationship between fieldnotes and ethnography be made explicit. Sanjek points out that in some cases this has been accomplished by including many of the fieldnotes themselves (e.g. Condoninas, 1957; Kluckhohn, 1944; Mead, 1977) but this is not necessary as long as it is somehow made clear how fieldnotes were turned into ethnographic text. This last idea is similar to Sperber's (1985: 12) argument that in order for ethnography to be useful to ethnology (what he calls "anthropology"*) it must be accompanied by a "descriptive comment", which "identifies the object represented and specifies the type of representation involved". Such a comment is similar, Sperber writes, to the legend on a map, which allows the map reader to understand what the various marks on the map mean. Descriptive comments in ethnographies allow readers to know what sort of statement has been made and what its epistemological status is, allowing them "to draw empirical inferences from a non-descriptive representation", an ability crucial to any sort of ethnological analysis.

As a final note on writing, although I hesitate to offer a technological fix to any problem, hypertexts might be a way to improve the presentation of ethnographic material, both in terms of its approachability and its ability to convey a sense of another culture to a reader and in terms of its ability to elucidate the ethnographic text formation process. The advantages of hypertexts, which may be contained on a CD-ROM, over traditional linear texts are, first, that they can include not only text but also still pictures, moving pictures, and sound, and, second and perhaps more important, they can be read in virtually any order, with readers jumping from topic to topic simply by "clicking" with a mouse on a key word that then takes them to related parts of the text. This process would keep most readers' interest better than a traditional text (something we teachers of anthropology are always looking for), and imitates more effectively the way one really learns a culture, in bits and pieces that typically do not start with the natural environment and move outwards through economics, social organization, politics, and religion. The amount of data that can be stored on a CD-ROM would also enable the inclusion of much more of the original source material as well as commentaries on the epistemological status of statements made in the text. Ideally, one could click on any given statement in an ethnography and move instantly to the relevant fieldnotes or other source material. Furthermore, a hypertext could include input from a variety of different authors; film-makers, etc., creating a text that would more closely resemble the Bible-as-ethnography sort of experiment advocated by Tyler (1986; see Crane, 1991 for an example of hypertextual cultural representation).

Conclusion

Anthropology, or more properly ethnology, is not an "interpretive science", but a science of interpretations, and ethnography is not a science at all. Just as archaeological theory, properly understood, is not about past cultures or society or behavior, but about archaeological sites (Binford, 1981), so is ethnological theory, strictly speaking, about the content of ethnographic texts, not society, culture, or behavior. Those texts are composed of interpretations just as
archaeological sites are composed of strata, features, artifacts, and so on. The fact that ethnology has as its subject matter interpretations in no way makes scientific ethnology impossible, but it does create some unusual challenges. The further development of an understanding of ethnographic text formation processes, aided by some of the insights provided by the textualist critique of ethnography and by other innovative ideas, is essential to the development of such an understanding. Just as in archaeology it has been essential to develop a systematic understanding of the processes that create archaeological sites, so it is essential for cultural anthropologists to more clearly understand the processes contributing to the development of ethnographic texts. But while anthropologists must take what is given, cultural anthropologists are constantly creating their own "sites," and can experiment with that process with the goal of producing texts that make the ethnologists' job a little easier, and their results more reliable. As Marcus and Fischer write, "every individual project of ethnographic research and writing is potentially an experiment" (1986: ix). The goal of the study of ethnographic text formation processes is to make permanent this "experimental moment" in the history of anthropology.

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Notes

1. By "metatheory" I mean the basic principles governing such broad issues of how data and theory are to be related to one another, issues that concern not just one theoretical perspective or another but the entire discipline.

2. By "ethnographic text" I mean text in the same literal sense in which the phrase is used by such textualists as George Marcus and Michael Fischer, not in the metaphorical sense in which it is used by Geertz and other interpretive anthropologists. In the Geertzian textual metaphor, cultures are like texts and therefore can be analyzed using some of the same techniques one would use to analyze a text. In the textualist literature, including Geertz's more recent work (e.g. Geertz, 1988) the term "text" is used in the literal sense that ethnographies are texts. For the purposes of this article, the phrase "ethnographic texts" can include not only standard monographs but also fieldnotes, articles, reports of various kinds, films and videotapes, still photos, tape recordings, and so on.

3. Aberle (1987) describes evolutionary biology as a historical science, while I am using it as an example of a theoretical science. It is both. The natural history part of evolutionary biology, the part that focuses on the phylogeny of organisms, is essentially a historical science. The part that Chagnon draws on, in contrast, is the systematic exploration of the implications of the concept of natural selection and of other processes that influence evolution, such as genetic drift and isolation, and that part belongs in the category of theoretical science.

4. It should be noted that the scientific status of cultural anthropology is a separate issue from the status of knowledge claims in scientific cultural anthropology. This section is not meant to imply that methods of data collection are irrelevant to the latter. Clearly, some methods make for more solid knowledge claims than others. A cross-cultural analysis based only on the reports of individuals with long residence in the societies in question and good knowledge of the local languages involved will provide more reliable findings than one based only on, say, travelers' reports. Some field methods and some styles of ethnographic writing are relatively science-friendly, some science-indifferent, and some almost science-hostile. Part of the value of studies of ethnographic text formation processes is their ability to clarify the relative suitability for scientific analysis of data from different field methods and styles of ethnographic writing. One of the goals of this article is to show that the results of such studies are sometimes surprising, with superficially more "scientific" methods and styles occasionally being less conducive to scientific analysis than superficially more impressionistic or humanistic ones.

5. This is not meant to imply that there is no relationship at all between theory and methods of data collection. Indeed, methodological changes are often what spark paradigmatic shifts (cf. Hacking, who argues that "experimental science has a life more independent of theorizing than is usually allowed" [Hacking, 1983: viii] and that "some observations are entirely pre-theoretical" [1983: xii]). The point is simply that no increase in the rigor with which observations are made will transform a non-science into a science.

6. The operative analogy in this article is that the study of ethnographic text formation processes is to scientific ethnology as the study of archaeological site formation processes is to scientific archaeology. This is not the same as saying that the details of ethnographic text formation processes have close analogs in archaeological site formation processes, and the analogy probably should not be pushed to that level. In their details, ethnographic text formation and archaeological site formation involve very different processes. Nothing in ethnographic text formation processes, for example, corresponds exactly to the way an archaeological site is transformed by looting (though the wartime loss of some of Edmund Leach's field notes on the Kachin does come to mind [Leach, 1954: 312]) or to the way the orientation of artifacts in soil may be determined by how water flowed through the site (Schiffer, 1987: 270) and nothing in archaeological site formation processes corresponds exactly to the way informants may deliberately mislead an ethnographer (Binfords, 1983: 21). What this means at a practical level is that most ethnologists probably
References


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