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The Rashomon Effect:
When Ethnographers Disagree

Disagreements between ethnographers often arise because of the particular circumstances of field-work or attributes of the ethnographers. A positivist search for truth versus error may be less fruitful than a constructionist examination of the research itself. This article suggests a conceptual framework for such a constructionist approach.

DISAGREEMENTS BETWEEN ETHNOGRAPHERS POSE A CRUCIAL METHODOLOGICAL PUZZLE: How are we to understand or resolve such disagreements? This article is an attempt to pull together a conceptual framework to deal with the puzzle. As it happens, ethnographers rarely disagree with each other's interpretations of a culture, and when such disagreements do arise they are usually handled by discreet avoidance or confused partisanship. Only recently, in confronting Derek Freeman's 1983 attack on Margaret Mead's picture of Samoa, have many American anthropologists been pushed to think deeply about the meaning of ethnographic disagreement in general and, more important, to discuss it in print.

There are several well-known disagreements in anthropology. The classic disagreement was between Robert Redfield (1930) and Oscar Lewis (1951, 1953, 1960) over the nature of the Mexican village of Tepoztlan. Other notable disagreements in the ethnographic literature include whether or not the Arapesh had war (Mead 1935 versus Fortune 1939), and the exchange between Ward H. Goodenough (1956) and John L. Fischer (1958) on residence rules in Truk. In reaction to Ruth Benedict's famous capsule ethnographies in *Patterns of Culture* (1934) came John Bennett's discussion of alternative interpretations of Pueblo culture (1946), and Helen Codere's reanalysis of Kwakiutl (1956). There are also Beverly Gartrell's article (1979) contrasting her view of the Nyika of Tanzania with that published by Miriam Slater (1976), and Joseph Reser (1981) and Arthur Hippler (1981) on the Australian Aboriginals; and of course, most recently, Freeman on Mead's Samoa (1983).

One's approach to these disagreements reflects one's basic position on truth, reality, and the scientific method. In philosophical discourse, lines are clearly drawn between rigorous logical positivism/empiricism and subjective metaphysical meaning-dependence (cf. Kaplan 1968 and Achinstein 1968:67). This dichotomy surfaces in anthropological scholarship as a positivist-constructionist dispute (see Harris [1979:ch. 1] for positivism, and Peacock [1986:68–72] for constructionism). I find both these presentations nicely equivocal and I, like many ethnographers, draw freely from both camps in my own research.

Here, in dealing with ethnographers' disagreements, I take an intermediate modified constructionist position: in important ways, ethnographies are made, not found. Redfield, one of the parties to the classic Tepoztlan disagreement, put it well:

> An account of a little community is not something that is given out of a vending machine by putting in the appropriate coins of method and technique. There is no one ultimate and utterly

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objective account of a human whole. Each account, if it preserves the human quality at all, is a created product in which the human qualities of the creator—in the outside viewer and describer—are one ingredient. [Redfield 1960(1953):136]

While Redfield's position may have sounded hopelessly equivocal in the 1950s, by the late 1980s it is hardly novel. Landmarks in this change might be the influential work by Thomas Kuhn (1962) in the natural sciences, who argues that research is shaped by the particular paradigm of its time, and also the book by the psychologist Robert Rosenthal (1976) on the "Pygmalion Effect" and other factors creating observer bias. By now some form of the constructionist view is held by scholars in many disciplines (see Kemper 1981, Davis and Mitchell 1985, and Shweder and Miller 1985).

The purpose of this article is to suggest that ethnographic disagreements present puzzles of the greatest importance. And there is an irony here that Pollner (1974, 1975) has pointed out: it is only with the assumption of a shared reality ("mundane reasoning") that these disagreements ("reality disjunctures") take on significance as puzzles to be solved; there is a shared reality, true, but differing truths may indeed be said about it.

The charter image of this present enterprise is from a 1950 Japanese film made by Akira Kurosawa based on two short stories by Ryunosuke Akutagawa (Kurosawa 1969). The film is set in 12th-century Japan and concerns the encounter in the forest between a bandit and a samurai and his wife. The mystery of the film comes from four quite different accounts of the same event (a sexual encounter that may be rape, and a death that is either murder or suicide). Each account is clearly self-serving, intended to enhance the nobility of the teller. Each account is presented as a truth at a trial by the bandit, the samurai's wife, the samurai (who, having died, testifies through a spirit medium), and a passing woodcutter who may have been an onlooker. As each of the four testifies, we see that particular version of the events on film, so that the apparent truthfulness of the visual image supports each testimony in turn. But unlike the familiar detective story on film, where accounts that are later impeached are given only verbally, *Rashomon* commits itself to, and convinces us of, the truth of each version in turn. And unlike the detective story, we are not given an explanation wrapped up nicely in truth at the end.

I do not propose to take *Rashomon* as more than an allusion to the idea of contradictory truths.[1] It is at best a charter image for us, and certainly not a charter myth—deeper consideration of Kurosawa's film leads us too far afield, into consideration of art versus flim-flam versus paradoxical koan.

At the most superficial level, a confrontation between two ethnographers has all the attractions of a good fight, and nothing attracts attention quite like the sound of a colleague's mistakes being nailed to the wall. But the question of who is right and who is wrong in these confrontations is the least interesting one that we can raise. Certainly there are some senses in which an ethnographer may be just plain wrong, but even the mistaken ethnography has potential use. That is, even "mistakes" may be made to reveal something of importance about the culture concerned as well as about the background of the ethnographer.

Another proposition: Those realms of culture that generate disagreement are likely to be those that are most problematical and interesting. What these disagreements reveal about individual ethnographers is of ethnographical importance to the extent that the disagreements arise as the result of the ethnographer's membership in a group (as representative of his or her own culture, theoretical school, or the like).

But most important, the value of thinking about the Rashomon Effect goes far beyond the relatively few cases of ethnographic disagreement that we shall be able to turn up. The sorts of influences, biases, or predilections we can examine here are at work in all ethnography, even when it is unchallenged. And so what we learn from the special case of ethnographic disagreement can help us understand ethnography in general.

The following is a brief discussion of some reasons for disagreements between ethnographers.

1. Someone is wrong

Probably most disagreements are not clearly resolvable (in the film *Rashomon*, someone *did* and others *did not* plunge the knife into the samurai's chest). The resolution may not be one of the two answers offered but some more complex mix (again, taking an example from *Rashomon*, there was probably sex between the bandit and the samurai's wife, but it may have been somewhere between rape and seduction). At any rate, even if we can satisfactorily determine that someone is wrong, we must go further to understand why.

I do not at all intend this to be a cavalier dismissal of truth or denial of the possibility of falsehood. Ethnographies can contain information that is wrong, whether through deliberate falsification or otherwise. Although Raoul Naroll and other hologeistic anthropologists working with the Human Relations Area Files have not been primarily concerned with ethnographic disagreement, they do deal with many of the same influences under discussion here. They generally focus on "the problem of ethnographic error" (Naroll 1970:928), and do not treat it as a puzzle of interpretation. They have been trying to identify and so control for ethnographer bias which results in errors in the ethnographies, because these errors compromise cross-cultural correlations. By their emphasis on error, they take a positivist position. Not surprisingly, the sorts of questions they ask of the ethnographies are especially vulnerable to false answers, in my view. For example, Naroll's most-cited finding concerns the presence or absence of witchcraft attribution (1962:153): ethnographers who spend longer in the field are more likely to report that deaths are attributed to witchcraft than those who spend a shorter time. One possible explanation of this is that the short-term researchers are simply wrong, that they missed an important fact. And indeed, presence or absence of witchcraft beliefs is about as close to a truly determinable fact as one could ask for.

2. They are looking at different cultures or subcultures

This problem is exemplified by the old tale of the blind men disagreeing about the nature of an elephant because each is touching a different part of the beast.

Confusion may arise from the use of one name for peoples who are quite different in important ways. But generalizing to an entire society on the basis of data from one subset of the population also happens often. I think particularly of gender differences (see McGoodwin 1978), but in many societies there is enough class or occupational differentiation to create different views of the situation. This presumably would only result in disagreements if the source of the data was not specified and the generalizations were carelessly made.

3. They are referring to the same culture at different times

Surely no anthropologist can be unaware of changes over time, but sometimes when we create an ethnographic present we obscure the temporal origin of the data; Divale (1975) has emphasized the significance of this "temporal focus." I would suggest that part of the disagreement between Mead and Fortune about Arapesh warfare can be attributed to differences in time periods. Ember has addressed these two points, saying that "the main reason we should reject Freeman's attack on Mead is that his so-called evidence does not deal with the time and place that Mead described" (1985:906). And we are not just talking about linear time change, as exemplified in the Pacific by the landmarks of pacification and missionization. We also need to consider the possibility that the different ethnographers stepped into the culture at a different phase of a cultural cycle. The philosopher John Ladd (1957) was forced by the exigencies of his academic duties to do his Navajo fieldwork in the winter instead of the summer, when most ethnographers had worked. As a result he learned of many matters of Navajo ethics that are only spoken of in the winter. I saw the great Pig Feast of the Grand Valley Dani for the first time after nearly three years of fieldwork, at the end of my fourth visit, after I had begun to formulate my ideas of Dani as a low-intensity culture. An ethnographer who

began fieldwork with the Pig Feast might well come up with quite a different view of Dani culture.

4. They are looking differently at the same culture

a. What of different personalities of the ethnographers?   There is an old saying that each tribe gets the anthropologist it deserves. Surely this must have some truth to it, but how do we deal with it except anecdotally? For instance, Devereux says:

> Fortune apears to have a special affinity for the glum side of cultures. Hence, among the Dobuans he studied mainly the (glum) *manifest*, and among the Omaha the (glum) *latent* side of culture . . . I appear to have an affinity for the warmly human side of the culture. Hence, among the Mohave I was interested chiefly in the manifest pattern and among the Sedang in the latent pattern. . . . Some scholars implement their subjective need for consistency . . . by emphasizing the manifest pattern at the expense of the latent one. [1967:214–215]

b. What of different value systems of ethnographers?   This was Redfield's (1960[1953]) explanation of the Tepoztlan disagreement, and this is the factor of ideological bias that Precourt raised (1979).

A romantic commitment to harmonious functionalism can lead to overemphasis on the harmonious aspects of a culture (Rohner, DeWalt, and Ness 1973; Carroll 1974; Schweizer 1978).

c. What of different cultures of the ethnographers?   Surely, any ethnographer would agree at first with the proposition that ethnographers are creatures of their own cultures and approach other cultures through their own. Yet I know of no systematic evidence for this (but see Devereux [1967:129–132] for suggestive anecdotes and see Trigger's 1984 attempt to explain archeological approaches in terms of the sociopolitical milieu of the archeologist).

In one of her most stimulating essays, Mary Douglas (1967) talked about the implication of the Nuer having been studied by Englishmen and the Dogon by the French, and she mused about what might have resulted had Evans-Pritchard studied the Dogon and Griaule and Dieterlin the Nuer. It seems very logical. But I cannot offer any support from Oceania. Is it possible that the ethnographic discipline is so strong, and the ethnographic apprenticeship so successful, that all traces of cultural origin are suppressed in the process of becoming an ethnographer?

d. What of other traits of the ethnographers?   This is a more miscellaneous category, but it is necessary in order to be able to include consideration of other personal features of the ethnographers such as gender, age, race, sexual preference, family status, personal health, and perhaps even height, any of which could possibly make a difference in what sorts of information might be made available to an ethnographer. All of this could be taken to a ridiculous extreme. For each ethnographer to present a full confessional autobiography would be an indulgence (and undoubtedly far from full). Yet some of this information may be important (see Devereux 1967:133). There are certainly limits to the extent we need to get into biographies of ethnographers. But I think that it is fair to say that we do not yet know what those limits are.

e. What of different theoretical orientations or research plans?   This is always the most obvious and most acknowledged influence, and should be the easiest to establish. The most dramatic example concerns the effect that the feminist movement has had on recent ethnography. It is not simply a matter of the gender of the ethnologist. Indeed, the data on gender effect are quite equivocal (see Divale 1976, Whyte 1978, Martin 1978). For example, Whyte (1978) found no evidence of gender bias in the specific area of reports on the status of women. But it does seem likely that, in general, male ethnographers (or better: ethnographers unaware of the feminist literature in anthropology) will tend to neglect women's roles in society. Abu Lughod, in her Bedouin ethnography, has tried to account for the effect of ideology and maleness on some of her ethnographic predecessors:

While I would not accuse Meeker, Caton, Evans-Pritchard, Peters, or any others of inappropriately projecting their own interests onto a situation, it strikes me that a felicitous correspondence between the views of Arab tribesmen and those of European men has led each to reinforce particular interests of the other and to slight other aspects of experience and concern. [1986:30]

The straw man gambit appears as a significant subset of these examples. It is immediately acknowledged but is in fact difficult to recognize and difficult to deal with. One cannot help but approach it with a bit of bemused cynicism. There are the studies that proclaim a new theoretical approach and demolish old ones, not so much for pressing scholarly demands but rather because of our need for individual achievement. In her Malinowski Memorial Lecture, Marilyn Strathern has discussed Malinowski's own use of such theoretical "straw men" (1981). But the reason this subject is so difficult to deal with is because it raises questions of scholarly integrity that are not always very accessible, perhaps even to the principal.

f. What of the situation when the same ethnographer changes his or her interpretations over time? As more ethnographers do long-term fieldwork this should come to be more important. I have written about my own changes of thought about the Dani, the earlier stage of which is reflected in Robert Gardner's film *Dead Birds* (1963), or even worse, in my 1965 dissertation and a colloquium that I presented at Columbia University; and in contrast, the latter stages of my thinking on the Dani in my 1979 case study (see also Heider 1986).

g. What of different lengths of time in the field? In his book on Data Quality Control (1962) Raoul Naroll has suggested that witchcraft is more likely to be reported by ethnographers who stay longer than a year in the field than by those studying a shorter time. On the other hand, length of stay has no effect on reports of drunken brawling. Certainly on the whole, length of stay has an effect on the ethnography. But it is often surprisingly hard to pin down from the evidence published in an ethnography.

h. What of different knowledge of language, or knowledge of different languages? Surely this must make a difference, but how? On the basis of cross-cultural studies, Witkowski (1978) reported no effects for language ability of the ethnographer. But in the case of ethnographer's disagreements, relative language fluency would surely be a factor to be considered. I once heard two people who both claimed linguistic competence give drastically different translations of a phrase shouted at a ceremony. One claimed that it was an interesting symbolic reference, the other heard it as a call to take up arms against the central government.

And what of the difference between those New Guinea studies done in the vernacular and those done in pidgin English? It is hard to determine the linguistic basis of most ethnographies (or the linguistic competence of most ethnographers), but considering the short time so often spent in the field, I wonder if the ethnographer working in pidgin does not have a real advantage over one who spends time trying to learn the vernacular from scratch.

i. What of different degrees of rapport? This is like the previous factor, but even more difficult to deal with. We know from informal discussions (and gossip about) our colleagues that there are tremendous differences in the ways different ethnographers relate to the people they are studying. This should surely make a difference in the ethnography, but how?

These last three (g, h, i) have a status somewhat different from the other factors on the list, since they refer to relative deficiencies in ethnographic competence. The phrase "optimal fieldwork conditions" has been used (e.g., Witkowski 1978) to describe ethnographies based on a stay in the field of more than one year and a working knowledge of the field language. I think that we can agree that more fieldwork is better than less, more language better than less, and more rapport better than less, other things being equal (although a long chummy stay is not a guarantee of deep insight).

j. What of different previous fieldwork? Most of us first go to the field at young and impressionable ages and our notions of culture (as well as our theories) are often strongly shaped by the first cultures we study. Thus, our subsequent ethnographies may well show traces of the expectations of our first, a process that Devereux has called "carryover" (1967:221).

Two ethnographies that have caused something of a stir because of their authors' extraordinarily negative views of the cultures are Colin Turnbull's book on the Ik (1972) and C. R. Hallpike's on the Tauade of Papua New Guinea (1977). In each case, the author had previously written a warm and empathetic ethnography about a culture he obviously liked. There is undoubtedly an order effect at work here, in which certain features of the second culture are judged more unfavorably against the comparable features of the first. My guess is that the Tauade are not much different from other New Guinea mountain people, but certainly Hallpike is the first New Guinea ethnographer to have such a negative reaction in print.

It is surely time to think about these matters systematically. With few exceptions anthropologists have lagged behind other scholars, most notably psychologists. This article is intended to explore some problems that deserve careful consideration by ethnographers. It boils down to the question: What do we need to know to resolve contradictions between ethnographers? And this, in turn, leads to the broader question of what we need to know to understand an ethnography—which in turn suggests what we need to include in our own ethnographies.

Notes

[1] The phrase "Rashomon Effect" turns out to be immediately intelligible to ethnographers, so much so that I claim no credit for inventing it, even though I cannot recall getting it from anyone else. After my first use of it (in the Summer 1980 ASAO Newsletter) I began to hear of other uses. A. Thomas Kirsch pointed out to me that Ruth-Inge Heinze used the phrase, although she did not elaborate on it, saying merely that "We have to be aware of the 'Rashomon Effect' " (1979:65). John W. Adams directed me to Marvin Harris's reference to the Rashomon film to make a similar point (1979:321); Barbara Frankel referred me to M. G. Trend's 1978 paper in which he recognizes "the Rashomon effect" when a situation is analyzed both quantitatively and qualitatively (1978:35). And Frankel herself has used the phrase to refer to a somewhat different phenomenon that occurs when the ethnographer receives different accounts from different informants (Frankel 1981). H. Russell Bernard referred me to Miles and Huberman (1984), who touch on some of these same issues and who have in their Index the entry "Rashomon Effect, 140," but I cannot find any use of the term on page 140 or elsewhere in their book. Lin Poyer used it in the title of a paper at the 1984 American Anthropological Association meetings. Clearly, it is a phrase whose anthropological time has come.

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