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Aspects of the Cannibalism Controversy: Comments on Merrilee Salmon

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Professor Salmon argues that the controversies about Mead's work and about cannibalism encourage healthy discussion of anthropological standards of evidence and definition, and provide an opportunity to consider the scientific status of anthropology. Her paper is broad in scope, concerned as it is particularly with how Arens's criticisms make an impact across the discipline and apply to a number of general theoretical controversies. I would like to look in somewhat more detail at some of the issues on which her discussion of the cannibalism controversy touches.

I

Arens's position is that despite the considerable literature on cannibalism, it has actually occurred infrequently in human history, and has never been customary. His position is based on two premises: (1) There are few reliable eyewitness accounts, and none such by trained anthropologists. (2) The attribution of cannibalism is typically made of others in order to make one's own group look better or more civilized and to rationalize various kinds of action. Many of Arens's arguments involve finding ways to make the existing body of evidence compatible with his hypothesis. If the majority verdict of the anthropological community is to be trusted, Arens has overstated his position: he has not established that ritual cannibalism has never occurred. His critical attitudes, in the words of one reviewer "amount to little more than a refusal to believe any statement of the existence of cannibalism, combined with a variety of impeachments of the motives of those who report it" (Springer 1980, 148). But a number of anthropological commentators critical of Arens on this score agree that his work has made them much more careful about interpreting evidence for this phenomenon. (And some remain impressed by how much of the evidence for cannibalism is circumstantial.) The literature seems to bear out the truth of Professor Salmon's assertion that Arens's work has "shifted discussion

from how to interpret alleged episodes of cannibalism to questions about how to define the practice and how to support claims that a group engaged in cannibalism" (Salmon 1995, 5).

Still, there is reason to wonder whether this is a useful point of departure for philosophical discussion of anthropological standards of evidence. Professor Salmon seems to share concerns expressed by anthropologists involved in this controversy that the discipline's empirical credibility is under scrutiny. But one could ask why an arguably worst-case scenario should be cause for such concern. When we deal with a phenomenon so likely to encourage lying, or so ripe for use as propaganda, the problems of trusting informants will be particularly pressing. As Arens himself learned early in his research, people tend not to say that they themselves are cannibals: it is typically attributed to strangers, enemies, or "primitives." It is not surprising that people are uneasy about cannibalism or that it is the kind of thing that is particularly useful as a political smear. But does this incline us to think the problems of trusting and interpreting informants generalize to the study of other cultural phenomena? One might say it is a particularly, but also peculiarly troublesome phenomenon.

To answer this it will help to consider why this controversy has been so heated. Arens's book places the challenge to cannibalism in the context of a controversy at the center of general theoretical developments in anthropology: Doctrines such as the cultural determinism of Boas and Mead were part of a reaction to what were perceived as racist and colonialist tendencies in nineteenth- and early twentieth-century anthropology—tendencies implicit in assumptions such as that primitive societies were to be seen as evolving in the direction of advanced western ones. Arens, in fact, casts the easy acceptance in sixteenth- and seventeenth-century Europe of beliefs about New World cannibalism, based on highly questionable evidence, as intrinsic to an ideology supporting colonialism and the slave trade. The New World's native inhabitants were portrayed as so bereft of human qualities, that enslavement was an improvement on their condition. One message Arens's critique sends is that in believing so easily in cannibalism, anthropologists are still carrying about the colonialist assumptions of the likes of Livingstone (who, Arens remarks, "saw cannibals everywhere" [Rosenthal 1983, 10]). It can lead one to wonder in what other ways such attitudes are playing themselves out in the practice and application of anthropology. In addition to hitting at this particular sore point, the issue has potentially significant impact on the content of anthropological theory because the questions whether cannibalism occurs or occurred and whether it was more prevalent in our past are seen by anthropologists as casting significant light on human nature. Also, it is quite embarrassing to find that the

existence of something that has become such a commonplace of anthropological study can be so effectively questioned. This can be quite threatening to a discipline whose rationale is perceived, in some quarters still, as questionable. In this vein, when asked to explain why anthropologists should turn out to be so unscholarly in their acceptance of cannibalism, Arens suggests "that anthropologists have an implicit, possibly unconscious, vested interest in maintaining the notion of cannibalism among exotic peoples. Cannibalism points to the fact that there are people so radically different from us that their social systems are worth studying" (Rosenthal 1983, 14). He adds that many of the other things earlier thought to sharply differentiate societies from us, such as acceptance of incest, have fallen by the wayside under more recent ethnographic scrutiny.

II

This controversy also brings attention to a number of epistemological and methodological problems. Many anthropologists regard as the central objective of their discipline the overcoming of their own preconceptions in a process of discovering the possibly radically different conceptions of other cultures. (This is often linked explicitly to the retreat from colonialism.) They argue about whether or not the cultural subjects' conceptions should define the context in which a phenomenon is identified and studied, as well as about how to do this. Arens himself takes a clear stand on the "insider-outsider" controversy in discussing the matter of defining cannibalism:

Social action or ideas can only be appreciated in the context of the cultural system of which they form a part. In isolation the interpretation of such facts is sometimes meaningless and at other times misleading. (Arens 1979, 43)

Members of other branches and schools of anthropology have a different attitude about the nature and importance of "context." For instance, anthropologists following the ecological-materialist tradition of Leslie White, who are typically seen as opposed to the so-called "language-and-culture" tradition of Boas, tend to downplay linguistic analysis, interpretation, and the adoption or articulation of the cultural insider's viewpoint. Arens himself is critical of ecologists and cultural materialists who ignore the cultural significance of cannibalism, as well as "classifiers" (in cultural and physical anthropology) who are content to categorize cannibalism according to culturally neutral features. But he, like Professor Salmon, is also critical of the structuralists and symbolists at the oppo-

site extreme, who strike many critics as being more concerned to ply intellectual constructs than attend to cultural facts.

Some of these philosophical tensions emerge in a relatively recent cannibalism controversy in which Arens has not, to my knowledge, taken part directly, but reflects his concerns. It is about the so-called Windigo or Witiko psychosis—a culturally specific condition purportedly occurring among Northern Algonkians, in which the sufferer has an inexplicable craving for human flesh (Marano 1982; cf. Marano 1983, Brightman 1983, 1988). Some critics follow Arens's lead in arguing that the phenomenon is largely an anthropological myth, resulting because anthropologists naively accept informant reports, not realizing that they should not be taken at face value. The reports might obscure other practices, for example when testimony of infant windigos serves to rationalize infanticide under extreme environmental conditions (Marano 1982, 389). Reports of the psychosis itself can be exaggerations designed to discourage contact with people who have committed acts of starvation cannibalism. That is, the reports indicate a fear that such people might be more likely to do it again, rather than represent a true assessment of their psychological states (Marano 1982, 388).

It is also argued that the term often designates psychological states that share observable similarities with genuine psychoses, but which are also dissimilar in major respects. This is illustrated in cases in which suggestive remarks by authority figures effect instant cures. The term is often applied to anger, anxiety, and grief, and "to cases in which no cannibalism or murder is suspected" (Marano 1982, 389). Moreover, when the term is applied to genuine psychosis, critics maintain, the condition is an already clinically recognized variety and not culturally specific.

This controversy illustrates in a particularly well-documented way the kinds of concerns that Arens has. It shows how isolated instances of survival cannibalism can be exaggerated into a culturally accepted practice. It also gives an idea of just how anthropologists' understanding of beliefs and other cognitive states associated with cannibalism, or their grasp of the cultural significance of cannibalism, can be distorted by mistaken beliefs about its existence or nonexistence. Thus, it lends some support to a contention of Professor Salmon which I shall examine shortly.

These criticisms are curtly summarized by one commentator who remarks that these distortions show how we tend "to overpower a very poorly known Witiko phenomenon with our own intellectual creations" (Marano 1982, 394–395). This particular remark expresses a cultural materialist's antagonism toward cognitive anthropology and, more generally, toward the language-and-culture tradition.

However, the remark strikes home even to language-and-culture theorists who emphasize careful translation and attention to the conscious beliefs and conceptions of their subjects. The remark reminds them of problems they perceive in the work of inquirers they deem to be excessively concerned with unconscious cognitive or symbolic structure. The worries apply equally to psychological anthropologists, generative grammarians, symbolic anthropologists, and Levi-Straussian structuralists. The criticisms are also closely related to, and often indistinguishable from, complaints made against anthropologists concerned more with conscious cognition—as in the case of Ward Goodenough's efforts to analyze Trukese kinship groups as kinds of "corporations" (Goodenough 1951). Critics raised objections to the effect that important dissimilarities in the conception of these groups by the Trukese were overlooked (see, e.g., Cochrane 1977). The general worry is that anthropologists have too easily attributed to their subjects conceptions familiar to the inquirers, as professionals or as members of western cultural groups, but not familiar to their subjects.

The issues here are complex. Sometimes theoretical allegiances produce demonstrable distortions of evidence. But at times the differences are not resolvable by the discovery of obvious omissions, but reflect instead a difference in theoretical interest. Different approaches select different kinds of data as relevant. The data omission this entails is not of an evidently problematic kind.

In a case I studied some years ago, an anthropologist compared earlier ethnographic studies of his of the ability of Guajiro Indians to adapt to urban life done from the standpoint of psychological theory, relying on evidence from Rohrschach tests and the like, with a more recent study he had done of that group which analyzed their adaptation in terms of the conscious, culturally specific conceptions they brought to bear on their efforts to cope with the urban environment (Watson 1981; cf. Feleppa 1988, 91f., 222ff.). He favored the latter study, partly because he believed it reinforced positive, appreciative attitudes toward his subjects, and partly because he believed he was getting closer to the truth of the matter. The first consideration reminds us of the typical juxtaposition in the literature of social-epistemological concerns with theory-evidence questions. This inquirer believed his earlier approach to be at fault because it engendered or reinforced attitudes of indifference and superiority. As for the second point, I could not find clear warrant for the claim of better approximation to the truth. And examination of the treatment of evidence could not support or defeat this assertion, since the accounts were largely incommensurable and equally informative.

This suggests an attitude of pluralistic tolerance we might usefully apply to the questions of existence and significance to which Professor Salmon calls attention. She is critical of symbolic anthropology's emphasis on the meaning not the fact of cannibalism. Her criticisms have merit, both in light of the problems of kindred psychological anthropologists in studying the Windigo psychosis, and in light of the fact that structuralists and symbolic anthropologists may be among the worst offenders for imposing intellectual structure at the expense of relevant ethnographic facts. But I am not convinced they will necessarily go awry in giving low priority to cannibalism's existence.

It is useful to link the work of these symbolic anthropologists, as Salmon does, to Evans-Pritchard's studies of witchcraft. She aptly characterizes Evans-Pritchard as believing that "the real object of anthropological study is not to determine whether cannibals or witches exist, but to understand the role that beliefs about them play in the lives of people." Salmon sees affinities to the symbolists' concern to articulate meaning here. But, probing further into Evans-Pritchard's views, she argues that knowledge of the nonexistence of witches, "significantly shaped his understanding of Zande accusations of witchcraft and their explanations involving the activity of witches." This reverses the initial implication a comparison of cannibals and witches suggests, leading instead to her main conclusion here that "[i]f anthropologists who study cannibalism talk do not know whether or not it ever occurs their understanding of the meaning of the practices is limited" (Salmon 1995, 13). However, Professor Salmon gives no indication in this criticism, which I take to be aimed at all the symbolic accounts discussed, of the respects in which they are thus limited.

Evans-Pritchard's "intellectualist" commitments may not make him the best person to appeal to here. In the context of the formative controversies in anthropology I sketched earlier, the intellectualist attitude toward witchcraft, religion, and magic has been seen as implicitly derogatory toward other cultures. Evans-Pritchard was particularly interested in contrasting the "magical" and false with the scientific and true. This is an attitude symbolic anthropologists often adamantly reject.

From what I can glean from these essays, these symbolic anthropologists may be well within their rights to ignore the existence of cannibalism. When looking at Poole's symbolist account of Bimin-Kuskusmin cannibalism, it is very easy to see it in a positive light as an ethnography that shows remarkable sensitivity to context, and shows how culturally comprehensive context can be when cannibalism is, as it appears in this culture, to be centrally and intimately related to other beliefs, conceptions, and institutions. He sees in the great pandanus rite fundamental categories of gender, kinship,

ritual status, and bodily substance linked together in a rite wherein a single figure is sacrificed and consumed. It is in the stories of the rite that one finds

... different constellations of the cultural construction of cannibalism ... woven from different ideas about fertility, reproduction, growth, bodily substances, foods, strength, power, ritual efficacy, and pollution, all of which are bound up with distinctions and transformations of gender. (Poole 1983, 30)

It is not clear to me why, given these interests, he should care about whether this group actually practices cannibalism.

It also seems to me that the concerns of MacCormack (1983) are legitimate and also quite independent of the question of the existence of cannibalism. She is concerned, as Professor Salmon points out, with the function of attributions of cannibalism as tools serving various political purposes. The claim that "being a cannibal" has become a way of saying someone is "unfit to rule" gains some support from facts such as that cannibalism accusations increase in frequency with the onset of political upheavals. The smear tactic works regardless of truth-value: so why should an understanding of why and how it works require knowledge of the truth-value?

As for Sahlins (1983), his structuralist rendering of Fijian cannibalism's foundational conceptual role in Fijian religion, myth, and witchcraft does seem largely an exercise in comparison of beliefs, concepts, and mythic structures. He studies a fundamental etiological myth about the "first man" and the formation of marriage rights, in which cannibalism plays a central conceptual role. Again, I do not see why the existence of Fijian cannibalism is a relevant concern for him.

In summary, I agree that the cannibalism controversy calls attention to a number of interesting epistemological problems. Some have a more social political flavor, and some point to at times perplexing aspects of the theory-evidence relationship. I have acknowledged that the existence of cannibalism can be relevant to our grasp of its significance, but I suggest it is wrong to insist on this. On the other hand, I am impressed by the Windigo case of the pitfalls. My case for legitimate interest-relative data selectivity is a general one and it is not always easy to tell when this selectivity will lead you astray and when it will not.

Another complicating factor in these controversies about evidence standards is that it is often unclear whether what is at issue is whether our explanations encompass all the data needed to meet our theoretical interests or whether the issue is the kinds of theoretical interests we should have. Perhaps the issue of limitation raised by Evans-Pritchard is better joined if we question the value of a structural analysis far re-

moved from conscious cognition, or offering limited hope of subsumption into a causal account of structure and behavior. That is, it is not that they need to care about the existence of cannibalism in order to meet their theoretical interests, but rather that their theoretical interests should be of the sort that take the existence of cannibalism more seriously.

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