

ISSA Proceedings 1998 - Falsification And Fieldwork In Recent American Anthropology: Argument Before And After The Mead/Freeman Controversy



Ethnographic fieldwork – going into the bush, into the unknown – to study some ‘tribe’ has arguably been the central feature of cultural or social anthropology in this century.^[i] “Ethnography has been, and is, the sine qua non of cultural anthropology. It accounts for our initial status and networks within our profession, legitimizes us as “real” anthropologists. . . and provides us with the means to survive the publishing dictates of the academy.” (Farrer 1996: 170). It has been taken as primarily the product of the individual researcher and as relatively unproblematic. It then provides the evidential foundation for anthropological theory, which is where controversy enters. Debates are about the implications of the ‘research findings’, not typically the findings themselves. In the last decade and a half, there has been increased attention paid to just how ethnographies are rhetorically constructed by an anthropologist.

This is a valuable emphasis, but I am adding another – looking at how fieldwork is criticized and accepted as reliable after publication. I explore this process as a social activity by the discipline in light of its various audiences. To do this I focus on what led up to and followed Derek Freeman’s attack on Margaret Mead’s *Coming of Age in Samoa*. My concern is not with argument by Mead or Freeman per se – that has been done (Weimer 1990, Marshall 1993).

A bit of quick history. In 1925 Margaret Mead went to American Samoa to test G. Stanley Hall’s then current account of adolescence as inevitably stressful.. Her subsequent book refuting Hall and giving a compelling portrait of South Sea life, *Coming of Age in Samoa*, became a bestseller, and its view of adolescent development, particularly in sexual relations, had a great influence on American culture. Mead became the best-known anthropologist in America, a veritable

cultural icon (Lutkehaus 1996).

In 1983, five years after Mead's death, the first notice of the Australian anthropologist Derek Freeman's critique of Mead was published on the front-page of the *New York Times*; a media event ensued, complete with television appearances. Freeman, who dedicated his book to Karl Popper, the philosopher who championed the importance of falsification in science, claimed to have definitely falsified Mead, as well as offered a more adequate account of the interaction of biology and culture. A multitude of reviews and rejoinders followed; Freeman replied vigorously to many of these.

The American Anthropological Association even took a vote deploring the recommendation of the book by the magazine *Science* 83. In 1989 a documentary film, *Margaret Mead and Samoa* (Heimans 1989), apparently supported Freeman with an interview with one of Mead's informants who stated that she and other Samoan girls had "pulled Mead's leg in response to probing questions about their personal lives, and that Mead, then 24 years old, believed their tall tales" (Monaghan 1989: A6).

Why was the Mead-Freeman controversy such an event? For some anthropologists, there has been a certain befuddlement - why won't it go away? One reason is the sheer number of issues involved - ranging from particular questions such as the degree of Mead's facility with the Samoan language, to the personalities involved, to larger issues such as the nature-nurture debate and social responsibility of scientists. It is a mistake to say, as some have, that "it was really about" one thing and not another. Nonetheless I focus in this paper primarily on the relation of an epistemic matter to a standard rhetorical one, on how anthropological fieldwork claims are taken to constitute reliable evidence or knowledge for the audiences of anthropology. Following Lyne, I distinguish anthropology's intra-field audience - other anthropologists, its inter-field audience - other scholars and scientists outside the discipline, and its extra-field audience - the general or educated public (Lyne 1983). My issue involves how, as Lyne puts it, epistemic expertise is projected to these various audiences.

1. A Criterion of Science

Although many discussions of whether or not social sciences are really sciences are at best unfruitful, let me begin with one criterion for being a science set out by a philosopher writing for anthropologists (see also Kuper 1989: 455). In "*Objectivity, Truth, and Method: A Philosopher's Perspective on the Social Sciences*" Little writes, that while there is no "cookbook" version that can be

given for scientific method:

The epistemic features of science include at least these criteria: an empirical testability criterion, a logical coherence criterion and an institutional commitment to intersubjective processes of belief evaluation and criticism. . . . And all [sciences] proceed through a community of inquirers in which the individual's scientific results are subjected to community-wide standards of adequacy. And these standards are designed to move the system of beliefs in the field to greater veridicality and explanatory power. (1995: 42)

It is the last criterion that is my focus – the requirement of an effective critical assessment community of inquirers. The connection of this criterion of scientific standing to the audiences of American anthropology is highlighted by two influential anthropologists, who see the controversy as a “scientific scandal” for “the reading public” who had come to look to Mead and others to deliver the discipline's “long-established promise: its capacity on the basis of reliable knowledge of cultural alternatives to critique and suggest reform in the way we live.” (Marcus & Fischer 1986: 3).

Little cites several examples of anthropological ethnography, to “show that it is possible for interpretive anthropology to be supported by appropriate empirical methods; and that is all that we need in order to show that anthropology is a scientific discipline in which there are appropriate standards of empirical reasoning as a control on scientific assertion.” (1995: 43). However he does not examine how any of these were critiqued by the anthropological community. Are there in fact standards of ethnographic accuracy? And most importantly for this paper have anthropologists applied them? I will argue the record is mixed.

There has been a tendency to see the ethnographic process as unproblematic, and thus not especially needing critical assessment. In considering the Mead/Freeman controversy, Rappaport comments “Even poor ethnography usually gets the facts right.” (1986: 347). Heider asserts that “ethnographers rarely disagree with each other's interpretations of a culture” (1988: 73). It should be added in defense of anthropology that in the beginning years of this century a high priority was placed on studying societies before they disappeared or radically changed. It was rare that two researchers would work on the same society, or even two adjacent ones. Thus the likelihood of conflicts such as between Mead and Freeman was low, though they certainly occurred. Given the relatively small number of anthropologists it “seemed a waste of scarce resources to let two or more researchers go to the same place.” (Kloos 1997: 430).

A second tendency is to neglect the role of the community of scientists in

critiquing the evidence in the constitution of the evidence as such. "Real science" is what goes on before publication. Just one example. Headland slips into this tendency even though it does not even reflect his own practice. At the close of a survey of controversies in ecological anthropology – in effect showing how anthropology meets Little's third criterion, he writes: "Basically, we need to do good anthropology – which means longer periods of fieldwork, more archaeology, especially in the wet tropics, and interdisciplinary team research." (1997:609). Given what he is trying to show, that "a refreshing new approach in ecological anthropology called historical ecology" has been part of effective critique of a number of "doctrines long accepted", it is surprising he does not stress that more good anthropological criticism is needed.

2. Views of the Controversy At the Time and Later

In the initial round of reviews of Freeman's book, many anthropologists basically rejected Freeman's claim to have refuted Mead (Weiner 1983, Schneider 1983). A number attacked Freeman for the manner of his critique, waiting until after Mead was dead, using questionable rhetoric, and the like. For some within anthropology the controversy was really peripheral to anthropology itself. It was simply a result of the vagaries of publishing and media misunderstandings. Others found Freeman basically correct on many of the elements of his critique, even though they may have questioned his approach (Appell 1984, Brady 1991). Freeman saw himself as vindicating anthropology, that is, by using anthropological means to refute Mead's work on Samoa, and thus redeem his discipline (as well as presenting a more accurate picture of Samoans).

For many outside of anthropology, Freeman set the agenda. There was a clear and decidable issue: "Who was correct about Samoan sexuality and adolescence?" and Freeman was seen as right. For example, Martin Gardner in an article entitled "The Great Samoan Hoax" writes: [Freeman's] "explosive book roundly trounced Mead for flagrant errors in her most famous work, *Coming of Age in Samoa*. ... new and irrefutable evidence has come to light supporting the claim that young Mead was indeed the gullible victim of a playful hoax. Her book, until recently considered a classic, is now known to be of minimal value – an amusing skeleton in anthropology's closet." (1993: 135) As I discuss below this view is *not* commonplace within the field of anthropology, but this pro-Freeman view of the matter is prevalent in two camps, in the inter-field area called "evolutionary psychology", where Freeman has been described as a "hero" of the movement (*Economist* 1998: 84, Pinker 1997) and, extra-field, in politically conservative or

right-wing American writing (Jones 1988, Davidson 1988). For many in the extra-field audience the Mead-Freeman controversy is not simply a matter of historical curiosity, but also part of clearing away misconception, propaedeutic to new intellectual advances. Wrangham and Peterson in *Demonic Males: Apes and the Origins of Human Violence* use Mead as a prime example of what their book is to offset, the “misleading separation of nurture from nature”. They assert that Mead’s “findings from this expedition [to Samoa] would capture the imagination of the Western world and galvanize a movement toward cultural relativism. Yet she was later proven extraordinarily wrong in many of her claims about Samoan life.” (1996: 106, 97).**[ii]**

For some the most salient issue has been whether Mead was duped by some of her informants. The fear that, in turn, the American public was duped has colored anthropological responses. For one, “Perhaps the most painful part of this controversy has been the erosion of the ‘public trust’ in the social sciences to which many educated Americans have traditionally looked for guidance with respect to how to raise their families.” (Scheper-Hughes 1984: 90).

An editorial the *Denver Post* asserted: This is more than just another academic teapot tempest; anthropology is a science often accused of being a haven for social theorists manipulating facts to prove their preconceived points . . . Mead . . . made major contributions to U. S. social attitudes. Her reputation is secure. The real loser may be anthropology’s reputation as a science. If its methods haven’t made quantum leaps forward since Mead’s day, the whole discipline might find a better home in creative literature (in Rappaport 1986: 316).

3. Whose Responsibility?

Are such public perceptions American anthropology’s fault? Some anthropologists have tried to distance their discipline. Rappaport argues that “Anthropology is no more capable of establishing the mythic status of narratives than is chemistry. All anthropology can do is to offer to a public accounts from which that public can select some (as it can from other sources) to establish as myth, leaving the rest to anthropologists’ arcane in-house conversations.” True enough, but as Rappaport mentions on the very next pages, “The book enjoyed substantial classroom adoptions for decades.” (1986: 322, 324, also Kuper 1989: 453). Such distancing attempts, such as Marcus’s comment, apparently intended to downplay Freeman’s critique, that “outside of introductory courses, [Mead’s] work has not generally been read in recent years.” are revealing (quoted Fields 1983: 232-233). But it is precisely in such courses that anthropology has its greatest opportunity to

educate its extra-field audience about itself. As the philosopher Philip Kitcher has suggested in his analysis of the conflicts between evolutionists and scientific creationists, the use of slogans, raw dichotomies ('proven fact' vs. 'only a theory'), and simplistic philosophies of science by biologists provide readily exploitable starting points for creationists (Kitcher 1983). The extra disciplinary audience for anthropologists, like evolutionary biologists, is in part a reflection of how scientists have educated it, including their critics. At least one would expect them to cite their efforts to rectify the misperception, even if the efforts are unsuccessful.

There is another tactic. If, as the Denver Post suggested above, anthropology was more like literature, then it would not be responsible for attempting to resolve the controversy. As one literature professor suggested: "[T]here is *a priori* no reason why we should attribute a greater degree of truth to her account of Samoan life than we might to a travel journal or a realist novel on the same subject.

And the same is true of Derek Freeman's . ." (Porter 1984: 31). But then anthropology's standing as science and source of cultural critique would have to be reassessed, something many in the field would resist.

4. Critique in Anthropology Prior to 1983

In responding to Freeman's critique some anthropologists rather dismissively said that the problems with *Coming of Age in Samoa* were well-known. In a review, Ivan Brady says by 1983 though Mead's Samoan research was still respected for "its pioneering impact . . . It was also recognized as inadequate on several counts . . . And had been relegated largely to discussions of disciplinary history" (1991: 497). And there certainly were several critiques. Indeed Freeman published a list of errata in Mead's *Social Organization of Manu'a* after they were not included in its republication (1972). Examples of published critiques are an article by Worsley in *Science and Society*, a socialist oriented British publication (1957) and an analysis of education in an African tribe, *Chaga Childhood*, by a South African anthropologist (Raum 1940).**[iii]** These do not seem to be obvious places to look for responses to Mead's work on Samoa. Someone from outside the discipline would easily miss these.

And other anthropologists praised Mead's work. McDowell wrote that "Most significant is [Mead's] concern for the precision and accuracy of the data she gathered In presenting her material accurately and precisely, Mead is a careful and exceptionally honest ethnographer." (1980: 127). At least until very recently it has been quite rare for anthropologists to do restudy of a group

previously studied by another anthropologist. But Ta'u, where Mead worked, has been restudied, in 1954, by Lowell Holmes, perhaps the first time a "methodological restudy was ever conducted with the specific purpose of evaluating the validity and reliability of an earlier observer's work." (Holmes 1987: 14) Holmes writes that his advisor Melville Herskovits suggested he restudy Mead's work, in part because "for some time scholars (including himself) had been skeptical about Mead's findings in American Samoa"(1987: 18). This gives some credence to the claim that Mead's work was thought to be suspect. However Holmes sums up his results as indicating that, though in some cases Mead "over-generalized and was given to exaggeration", overall Mead "was essentially correct in her characterization and conclusions about coming of age in Samoa. And I still am impressed with the quality of her investigation." (1987: 172-73). Unfortunately for anyone looking for a clear-cut resolution of the controversy, Freeman claims that Holmes's assessment is suspect, and that Holmes changed his evaluation of Mead's work over time, and under pressure. Nardi cites Holmes and an article by Naroll, which in turn cites Holmes, as examples of preexisting critiques (1984: 323) . However, the criticisms of Holmes are hardly comparable to Freeman's. Further Naroll also included an article by Mead in the collection in which the criticisms Nardi cites are included (Naroll 1970, Mead 1970). Of course, whatever one's view of Mead on Samoa, she was an indisputable pioneer in other areas, for example, in visual anthropology.

An examination of surveys published before Freeman's book in 1983 does not show any signs of this supposed widespread knowledge of Mead's weaknesses. For example, Agar lists a number of disputes over fieldwork, but does not mention Mead's work as one of these (1980). Edgerton and Langness discuss a number of cases where ethnography has been questioned - Ruth Benedict's Pueblo work, the Redfield-Lewis divergence - in a chapter where they also mention Mead, but make no indication of any reservations about her work (1974). Indeed the strength of the defenses of Mead after Freeman suggests that he was far from simply rehearsing or amplifying commonly held suspicions, albeit in an objectionably antagonistic fashion.

Either the supposedly well-known problems with Mead's work were not in fact known or recognized to be serious problems by very many, or not made public, even within the wider field. In any case the discipline never confronted them. Indeed there are mentions of a general custom of not being a public critic of a colleague's work. Jackson quotes an anthropologist informant as "commenting on one of the discipline's unwritten rules 'We've built up a sort of gentlemanly code

dealing with one another's ethnography. You criticize it, but there are limits, social conventions . . . You never overstep them or you become the heavy.'"(1990: 22).**[iv]** So when Freeman did bring them up, one speculates that there were some guilty consciences. Whether from simple oversight or Mead's iconic status, her Samoan work went without adequate critical assessment. In terms of the criterion of a critical assessment community prior to 1983 in this respect there is little evidence of it existing.

5. Critique in Anthropology After 1983

What has happened since 1983? One major change is the importance anthropologists now place on listening to those they study, to their subjects, as Freeman emphasized. Taking into account their views has become more common, indeed expected. As responses of Samoans to the controversy indicate there is at least much to learn from that audience.**[v]**

Another common response to the whole debate is to 'perspectivize' it, that is, to attribute the dispute to the effects of different perspectives or approaches of those involved, and not due to any inaccuracy per se. Thus falsification is impossible. For example, a review of a new book on the controversy begins: "I was amazed to find that yet another contribution to the so-called 'Mead-Freeman controversy' had been published, . . . It is even more unfortunate that authors cannot resist making judgements on this issue and trying to resolve the issues involved, insisting that there is and was a definitive , 'real' Samoa to be discovered. . ." (Morton 1996: 166). Scheper-Hughes, whose own ethnography in Ireland seemed to conflict with previous work of Arensberg, argues that

.. when we are talking about Samoan culture or Irish culture we are talking about an interpretation that is the result of a complex series of interactions between the anthropologist and his or her informants. . . . Ethnography is a very special kind of intellectual autobiography, a deeply personal record through which a whole view of the human condition, an entire personality, is elaborated. .. And the knowledge that it yields must always be interpreted by us, by the particular kind of complex social, cultural and psychological self that we bring into the field.Hence there can be no "falsification" of a 1925 ethnography by a 1940 or a 1965 "restudy" because the particular ethnographic moment in the stream of time that Mead captured is long since gone. (1984: 90)

This pattern of attributing differences to perspectives is not limited to this controversy. There is a growing movement in anthropology toward seeing

ethnography as a much more complicated and multifarious endeavor than previously held. A greater sense of the personal nature of ethnography, and of the rhetorical construction of ethnography developed in the years after 1983. As Brady points out, these developments “which we lump under the heading of ‘post-modernism’, [influenced] . . . a common perception (but very little said in print) that even if Mead was wrong, Freeman didn’t have . . . the answer to what was right . . . The ‘meta-issues,’ in other words, seem to have carried the day against Freeman, against closure on multiple interpretations of Samoan ethnography.” (1988: 44). However, while anthropology’s internal, or intra-field, audience was not especially interested, its inter- and extra-field audiences were drawing their own conclusions, as discussed above. Though really a matter for another day, I do not believe that post-modernism in any stricter sense than Brady’s is really involved. The issues pre-date its rise; it serves more to provide a strawman to criticize (Pool 1991).

This ‘perspectivist’ response would seem to make a thorough going criticism otiose. Other anthropologists, of course, do not see it this way. It is striking that other ethnographic work by Mead has come under significant criticism. Gewertz and Errington have re-evaluated Mead’s analysis of one tribe the Chambri (or Tchambuli) in *Sex and Temperament in Three Primitive Societies* arguing that Mead’s interpretation was led astray by reliance on a Western conception of self (Gewertz 1984, Errington & Gewert 1987). Others have made substantial criticism of Mead and Bateson’s work on Bali (Jensen & Suryani 1992). If Mead and her work were ever sacrosanct that does not appear to be the case recently (Foerstel & Gilliam 1992, Roscoe 1995).

With respect to Samoa, and in particular the controversy itself, there has recently been a number of critical work. There are two book length assessments. Cote, a sociologist, in *Adolescent Storm and Stress: An Evaluation of the Mead-Freeman Controversy*, comes to the conclusion “that Mead’s coming-of-age thesis is quite plausible . . . There are some problems with some of what she wrote in *Coming of Age*. But there is little reason to believe that she was wrong in most of what she reported – contrary to what Freeman claimed and despite the mythology surrounding her book.” (1994: xiv). Orans in *Not Even Wrong: Margaret Mead, Derek Freeman, and the Samoans* (1996) concludes that Mead’s fieldwork and the claims she makes on its basis are seriously inadequate, that on a number of points Freeman is correct, but that Freeman is wrong to think that he could refute Mead in that her claims are really insufficiently formulated to be either verified or falsified. Hence the book’s title *Not Even Wrong*. Given the prominence given to

the 1989 filmed interview with an informant which led to the perception that Mead was duped, after examining Mead's fieldnotes and letters, Orans holds that there is no indication that the 'tall tales' had any particular impact on Mead's thinking.

Even more striking are attempts to not just adjudicate the controversy, but to learn from it. Taking up suggestions first raised by Shore, Mageo develops an account of that integrates what she calls "the incongruent impressions that surround Samoan character." She argues that Mead and Holmes "documented the communal personality, which is the ideal product of Samoan socialization. Freeman observes the psychological costs of this ideal." (1991: 405). She does not simply say that there are different approaches, the Rashomon 'perspectivist' tactic, but tries to account for this divergence, and thus advance beyond the controversy. There are other articles of a critical nature (Shankman 1996, Grant 1995). Perhaps book reviews of the three books on the controversy (Caton, Cote and Orans) will be revelatory. Textbooks now at least have perfunctory mention that Mead's work is contested.

What is striking is the contrast between the simplistic "Freeman falsified Mead" views prevalent inter- and extra-field and the recent critical work on the controversy within it. If prior to 1983, the American public listened to an incompletely scrutinized account from anthropology, allowing Mead's erroneous findings to go unchallenged, today they do not seem to be listening to anthropology at all. And if they are not listening, then the discipline cannot fulfill what Marcus and Fischer call its "long established promise: its capacity on the basis of reliable knowledge of cultural alternatives to critique and suggest reform in the way we live." (1986: 3).

6. Conclusion

Is anthropology "the gang who couldn't shoot straight"? That is certainly not my contention. As Kloos points out in an examination of disagreements in anthropology, there also are many examples of sites studied by anthropologists from a number of countries, including the one studied, where no radical disagreements have emerged. And he rightly stresses that these outnumber the thirty some cases on the list of serious discrepancies that he has compiled (Kloos 1996). Nor do discrepant results necessarily indicate the absence of a critical assessment community. Tracing the history of research on the !Kung people, Kuper argues for the existence in that area of anthropology of a disputatious, but

at the same time cooperatively interacting, group of researchers from different countries and theoretical backgrounds, working, as he says, "in many ways like conventional scientists." - or at least like the standard conception of scientists (1993: 68). The practice of the journal *Current Anthropology* of publishing articles followed by comments from other scholars, often quite critical, is also signal. The American Anthropological Association has a precedent here. It published a collection of articles on another, somewhat similar dispute within the discipline: *The Tasaday Controversy: Assessing the Evidence*. (Headland 1992).

My conclusion is that, if one examines the discipline of American anthropology with respect to Mead's *Coming of Age in Samoa* prior to 1983 in light of the criterion of functioning as a critical assessment community the judgment has to be anthropology's achievement is at best mixed. A book that many in retrospect claim was inadequate was allowed to be seen as adequate, or even better. Since 1983 the evidence is considerably stronger, but not univocal. Here the *practice* seems better than the theory. That is, there is a considerable amount of criticism. What is problematic the strand of what I call 'perspectivism'. Here I have suggested the problem is not so much the practice of critical assessment, but confusion over the nature of, or even need for what Little terms "community-wide standards of assessment". This history in turn is, I have suggested, is partially reflected in the relation of American social anthropology to its various audiences. Meeting Little's criterion is of course at most a necessary condition. I have not tried to *explain* what occurred. Perhaps it is a matter of disciplinary structure and practice, of how a scientific discipline functions. Or perhaps it is the nature of social reality - the stuff ethnographies are about - as just too complicated or transitory to be studied in the ways anthropologists study it. One could argue that the culture and personality school, of Benedict and Mead, was particular prone to problems (see Stocking 1989). Establishing claims about temperament of a culture or dominant personality traits in a group may simply not be an endeavor for which anthropological methods are appropriate. The particular factors of Mead's iconic status, and Freeman's approach, must be considered. I am inclined to favor the first explanation, or perhaps some combination of factors.

Nonetheless there is only so much a discipline can do to educate its audiences. I was taken aback to read in a recent book by a psychologist - from Harvard University Press, the publisher of Freeman's book no less - that *Coming of Age in Samoa* is "considered by some to be one of the great anthropological studies of all time." (Plotkin 1998: 241). After all the controversy, I cannot believe that even Mead's strongest supporters would evaluate it that highly.

NOTES

i. I use 'anthropology' as short for American cultural or social anthropology. I draw on Strikwerda 1991. I want to thank Penny Weiss and Clarke Rountree for their comments, the Indiana University Kokomo Division of Arts and Sciences and Interlibrary loan staff and the Indiana University Institute for Advanced Study for their support.

ii. I have not done a comprehensive search, but the prevalence of these interpretations of the upshot of the controversy is striking. I did find more favorable treatments of Mead in books and tapes for children (for example Ziesk 1990).

iii. Note that these are not American authors. Gupta and Ferguson (1997: 45, n. 38) cite Radin's 1933 critique of Mead. Their omission of any mention of Freeman strikes me as rather disingenuous.

iv. Worsley writes that after publication of his 1957 article Mead wrote him attacking the piece. "Taken aback by the virulence of this language, I soon discovered that it evidently was not unusual, for I received several communications from anthropologists in the United States who told me that they had been treated to similar withering counterattacks when they had dared, especially in public situations, to say anything critical of her work". (1992: xi).

v. In her preface to the 1973 edition of *Coming of Age in Samoa*, Mead acknowledged Samoan concerns but stated that "It must remain, as all anthropological works must remain, exactly as it was written, true to what I saw in Samoa and what I was able to convey of what I saw; true to the state of our knowledge...." (1973: xii). Why she did not discuss these concerns in some depth elsewhere is not clear.

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