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Scrambling on Defense: An Anatomy of Anthropological Responses to the Mead/Freeman Controversy

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ABSTRACT: In 1984 Derek Freeman launched a crusade against Margaret Mead’s *Coming of Age in Samoa*, claiming to have definitively falsified her central claims. Anthropologists responded in a proliferating variety of ways, while failing to project scientific coherence. Underlying anthropology’s inability was its changing, and increasingly fissuring, conception of itself as a discipline. Was it a humanistic field or a science, and if a science what kind of science? Most saliently revealed was the discipline’s failure to recognize its own tacit acceptance of Mead’s impact on the American reading public without clearly—and earlier—advancing critical views of Mead’s earliest work.

KEYWORDS: Margaret Mead, Derek Freeman, anthropology, scientific controversy.

1. INTRODUCTION

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. (*Denver Post* as cited in Rappaport, 1986, p. 316)

Margaret Mead has a curiously divided standing today among Americans. For some she is one of the inspirational leaders of the 20th century. Many walls and websites display as a proud motto her proclamation stating “Never doubt that a small group of thoughtful, committed, citizens can change the world. Indeed, it is the only thing that ever has” (Stover, 2005). For some in the new field of evolutionary psychology, however, Mead is close to a *bête noire*; her first book *Coming of Age in Samoa* (hereafter, *COAS*) is excoriated for misleading American social science for many years while being based on a dreadfully misleading hoaxing. Steven Pinker’s comments in *The Blank Slate: The Modern Denial of Human Nature*, are representative. “Mead’s descriptions of peace-loving New Guineans and sexually nonchalant Samoans were based on perfunctory research and turned out to be almost perversely wrong. As the anthropologist Derek Freeman later documented, Samoans may beat or kill their daughters

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1 Stover claims that it is not clear where Mead first made this statement.
if they are not virgins on their wedding night, . . .” (2002, p. 56).2 And as reflected in the opening quote, this discredits the field of cultural anthropology as well as Mead.

Yet among American cultural anthropologists, whatever their opinions of Mead’s work, their general view seems to be that they have quite rebutted Freeman’s attacks. I wish to explore, and partly explain the divergence between these differing views. In particular, why hasn’t anthropology been able to put Freeman to rest? At the beginning of his 2010 review of Paul Shankman’s The Trashing of Margaret Mead, Robert LeVine asks: “Can there ever be an end to the 27-year-old controversy over Margaret Mead's Samoan fieldwork of 1926?” Similar plaints can be found throughout the literature, such as this comment from Morton (1996):

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. (p. 166).

American cultural anthropology seems to have been continually playing catch up with Mead’s critics.3 Like a monster that one tries to bury, the repercussions of Freeman’s attacks still echo in certain venues.

I will focus on one major factor in the controversy: anthropology’s inability to frame the criticisms of Freeman in a fashion that made their various defenses of Mead — and repudiations of Freeman’s position—persuasive to its inter-field and extra-field audiences. All their responses to Freeman did not accumulate into a cogent overall response. Indeed, as I will argue, anthropologists could not make their attacks persuasive without simultaneously undercutting their field’s epistemic standing.

I structure my analysis using a helpful distinction taken from John Lyne (1983) between three audiences: the intra-field audience of other anthropologists, the inter-field audience, scientists and academics in other disciplines, and the extra-field audience of academia—the general public, or reading public. Given, as I will argue, anthropology’s history of not taking its extra-field audience seriously, it seems to have lost credibility here, seemingly having lost ground to its inter-field competition, sociobiology and, more recently, evolutionary psychology. Another issue that I emphasize is the responsibility for a discipline to publicly criticize itself, to exemplify what is supposedly one of the hallmarks of a science, being a self-correcting enterprise (Laudan, 1981).

Today we may have lost sight of Margaret Mead’s standing in America. She was the best known American anthropologist, one of the most prominent social scientists of the time, a major cultural figure. And Freeman attacked her fieldwork, the defining mark of anthropology as a distinctive discipline: “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, p. 170). At the same time, the controversy ranged over such a range of issues, from very specific empirical issues of fact, to issues of method—techniques for gathering evidence, to methodology, theories of how research should proceed,

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3 For simplicity I will here use anthropology to stand for what more precisely is “American cultural anthropology.”
to more abstract philosophic or epistemological issues of the grounds of anthropological knowledge and justification (Harding, 1987, p. 2). Given this range, let me emphasize that this is a very limited look at just certain aspects of the Mead–Freeman controversy.

The notion of controversy itself deserves some attention. First, this is not a “manufactured scientific controversy” in Leah Ceccarelli’s sense of one centered around an issue about “there is actually an overwhelming scientific consensus” (2011, p. 196) such as whether HIV causes AIDS or about the reality of global warming. I take Martin Orans to be basically correct in his general conclusion in Not Even Wrong that given what we have of Mead’s records it is basically impossible to say about the truth of many of Freeman’s claims. Further Orans and Shankman are probably correct on the specific issue of whether Mead was hoaxed about female adolescent sexuality—no reason to think she was. If she was wrong here, it was on the basis of no single such incident. Ceccarelli points out that much work in the rhetoric of science criticizes “the world-defining hegemony of scientific discourse” (2011, p. 199). She is looking at cases where there is no more uncertainty than in any other area of science. I am expanding that focus by investigating a messy instance outside of cases of manufactured uncertainty and ones where uncertainty needs to be generated.

It should also be noted that American anthropology has had a goodly number of controversies in recent years. The prominent anthropologist George Marcus (2010) discusses these as one way of tracking “Developments in US Anthropology Since the 1980s.” He lists the Mead/Freeman controversy, “Gananath Obeyesekere’s critique of Marshall Sahlins’ account of the murder of Captain Cook in the Hawaiian Islands, and Sahlins’ refutation (early to mid-1990s)” David Stoll’s deconstruction of Nobel Prize winner Rigoberta Menchu’s testimonial about atrocity in Guatemala, and journalist Patrick Tierney’s 2002 attack on the work of Napoleon Chagnon with the Yanomami. As he says, “these controversies were about the modes and the effects of anthropological representations of traditional subjects and the real social stakes involved in representation itself” (p, 206). To this one could add the Tasaday “hoax” in which a small band of people in the Philippines was wrongly claimed to be a paleolithic people (Headland, 1992).

2. COUNTERATTACKS

Certain initial responses to Freeman amounted to counterattacks on him such as those that focused on “debate etiquette.” While certainly valid, these responses did not address the issue of the reliability of Mead’s work. For example, Freeman never satisfactorily explained why he waited as long as he did to publish his findings, finally publishing them five years after Mead’s death. Mead was a certainly a prolific writer, and likely would have mounted a spirited defense. He was also charged with resorting to ad hominem attacks and with seriously selective use of some of his sources (Holmes, 1983). Freeman’s personal style clearly was a

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5 In Robin (2004), three of the seven major cases he discusses are from anthropology.

6 One can also note in sociology, the criticism by W. A. Marianne Boelen of Whyte’s classic Cornerville study. See Whyte (1993).
factor; he was characterized as abrasive, on a vendetta, and unprincipled (Caton, 1990, p. 268). An undercurrent in some attacks was a concern that Freeman's work would support sociobiology, or “feed social conservatism and racial backlash in the U. S” (Fields, 1990, p. 231).7

Further, as several anthropologists working on Samoa have noted, Freeman never did publish what would have been a valuable contribution, an ethnographic monograph of Samoa (Levy, 1983, pp. 831–832). Freeman had done excellent fieldwork elsewhere; having his considered findings would have been valuable. Rather Margaret Mead and Samoa was an extremely a wide-ranging book, not just discussing Mead, but also providing a reassessment of the history of Boasian anthropology, and sketching a new synthesis of the interrelation of biology and culture in anthropology.8 These responses did not rehabilitate Mead, they only served to detract from Freeman’s credibility. And thus they do not particularly vindicate anthropology as a discipline.

3. DEFLECTIONS

Another range of responses I see as trying to deflect or defuse Freeman’s criticisms. A first variety focused on the legitimacy of the comparison between Freeman and Mead. Can one compare the 1940s Western Samoan village, where Freeman taught, within a bus ride of the capital of Apia, to the much more isolated island of Ta’u in the Manu’a islands of American Samoa in the 1920s? Could the differences in their accounts be understood in terms of Mead working as 23-year-old woman with adolescent women and Freeman working with older Samoan men, and becoming a chief? I will take up below the claim that he was inappropriately applying 1983 standards to 1920s work.9

A second deflecting defense—one I find especially curious—was to claim that though Mead might have been wrong about Samoa in a number of ways, her general conclusions were nonetheless well-taken. Levy (1983) argued that even if Samoans were aggressive, the Tahitians were as the Samoa were supposed to have been.10 Thus Mead was right, but for inadequate reasons. Obviously such a defense also does not serve to shore up anthropology’s status.11

Another deflection asserted that ethnography, by its very nature, was inherently impressionistic, and thus the differences between accounts were very understandable, and in sum did not reflect back on any ethnographer’s reliability, or indeed the discipline. A good

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7 See for example the summary of "The Barnard College Symposium of April 1983," by Fields (1990), p. 231. This was certainly a major concern in the early responses; interestingly it seems to have been quite quickly superseded by other concerns, as attention shifted from theoretical issues such as nature/culture, or nature/nurture, to more methodological, disciplinary ones.

8 Note that Freeman was a critic of sociobiology per se, offering what I would call a moderate account trying to reconcile culturalist and biological claims in anthropology.

9 Paul Radin’s 1933 critique would seem to indicate that at least this charge against Freeman was invalid.

10 See also Marcus (1983).

11 Shankman cites (2009, p. 222) Alice Schlegel and Herbert Barry (1991) who in their review of over 170 anthropological studies touching on adolescence found that the “widespread belief that adolescence in tribal or peasant communities flows smoothly, without completion for resources (which can include a desirable spouse and powerful in-laws) and without areas in which choice must be exercised . . . is belied by the data from this study” (pp. 41–42).
example of this is from Nancy Scheper-Hughes, whose own ethnography in Ireland seemed to conflict with the previous work of Arensberg and Kimball. She argues:

[W]hen 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, p. 90)

Attribution of ethnographic differences to personal perspectives or interpretive points of views was not limited to this controversy. It was part of a concurrent movement toward seeing ethnography as a much more complicated 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. (1988/1990, p. 44)

However, while anthropology’s internal, or intra-field, audience was not especially interested, its inter- and extra-field audiences may have been drawing different conclusions. 13

A serious problem that could be drawn from this “defense” is that it renders otiose one major strand of anthropological writing. Atkinson (1982) writes of “what cultural anthropologists do best, namely, it heads full tilt at culture bound assumptions in our own thinking” (p. 257). And this, of course, is something that Mead had made a major part of her own work, make implications for American life. Just as Freeman presented his work as a falsification of Mead’s account of adolescence, Mead thought of her work in Samoa as a falsification or refutation of G. Stanley Hall’s account of adolescence. This “personal perspectival” defense of Mead, and of ethnography, defuses Freeman’s attack, but at the cost of framing ethnography not as a scientific grounding for critique, but a simple personal opinion. Thus this defense did not really salvage Mead, however true it might be of ethnography as a practice.14

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12 See also Morton (1996).

13 Though really a matter for another day, I do not believe that postmodernism 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.

14 One complicating factor in assessing Mead's work was that an American anthropologist, Lowell Holmes, had in 1954 done a "methodological restudy" of Mead's work in Ta'u. He had summarized his work as finding that despite some more or less important deficiencies, "the reliability of Mead's account is remarkably high." This had led to letters between Freeman and Holmes, starting in 1966, which Freeman reads as indicating that Holmes's dissertation advisor, a Boasian, made him tone down his original criticisms of Mead's Samoan research; Holmes denies this. See Bargatzky (1988/1990).
4. DISTANCING

Another category of responses to Freeman was to distance contemporary American anthropology from Mead, and certainly from Mead’s 1920s work on Samoa. This tacitly admits the legitimacy of Freeman’s criticisms, but frames them as old hat, and thus inconsequential for current science. In the review mentioned above, LeVine writes:

> Although Mead became a celebrity through her books and talks popularizing cultural anthropology, her work was often sharply criticized by her anthropological colleagues. Within the discipline, she had, particularly after the 1950s, little theoretical influence. By 1978 (when she died at age 77), however, she was appreciated as a founding figure of American anthropology whose flaws represented an early era of the field. (LeVine, 2010, p. 1108)

However, LeVine’s claim that Mead’s work was frequently and extensively criticized is not substantiated. *Coming of Age in Samoa* was criticized by some but it was generally well received after its publication (Freeman, 1999, ch. 15; Shankman, 1999, pp. 113–115). Nor is it the case that Freeman was anachronistically applying later-day standards in his critique of Mead’s Samoan research. Paul Radin, a well-known student of Boas from a generation prior to Mead, in his 1933 *Method and Theory in Ethnology*, was quite critical of Mead, describing her as a “journalist, though of the best sort” (1965, p. 255). He questioned whether the period of time Mead was in Samoa was at all sufficient to substantiate her claims. But since he does not think a period of even five years would be sufficient, he may have been seen as overly critical. And as Arthur Vidich says in his introduction to the reissue of Radin’s work in 1966, the book does not seem to have made much impact (pp. viii, cxiii).\(^{15}\)

After the first round of reviews there are few published critiques. Among them are a critique by O. F. Raum in his book on Africa (1940) and an article by Peter Worsley in an English journal *Science and Society* (1957).\(^{16}\) But both of these are by non-American authors: Worsley is English and Raum South African. Now there may be a “fear factor” behind this paucity of critique. In commenting on the controversy Worsley writes that after publication of his 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, p. xi).

Another distancing tactic is to place Mead as an exponent of a very particular approach within American anthropology. For example, Conrad Kottak implies that part of the problem is Mead's participation in the "impressionistic" and methodologically less scientific "culture and personality" (1987, p. 282) school of anthropology.

Another point made is that *Coming of Age in Samoa* was written in a time when there was great awareness of the disappearance of many tribes. This was particularly so for Boasians who were very aware of the dying off of the last generations of displaced and decimated North American tribes who had lived in times prior to the massive influence of white American

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\(^{15}\) For a substantive methodological critique of Mead’s work, see Marvin Harris’s *The Rise of Anthropological Theory* (1968). Harris was himself a controversial figure, and a proponent of “cultural materialism,” a quite divergent approach to that of Mead.

\(^{16}\) Raum is mentioned by Bargatzky (1988/1990), p. 255.
society. There was a norm of “one ethnographer/one tribe” (Stocking, 1992, p. 282). In consequence, there was little emphasis on critique of ethnography. On the question of whether there was a culture of criticism in American anthropology, George Stocking, the premier historian of anthropology, notes that in the generation after *Coming of Age*, there was criticism of certain ethnographies, for example, the famous restudy by Oscar Lewis of a Mexican community earlier studied by Robert Redfield and a critique of interpretations of Pueblo culture by John Bennett (1946). Stocking writes:

> The problem of the reliability of ethnographic data—which might perhaps have been suggested by the laboratory metaphor and by the frequent self-identification of the new academic professionals as “scientists”—was largely forestalled by the archetypal distinction between the ethnographic amateur and the academic profession. (1992, p 283.)

When one looks at the decades before Freeman published in 1983, criticism of Mead on Samoa are not at all prominent. Instead she is cited as a paragon of American anthropology. 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, p. 127). 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 (1980) lists a number of disputes over fieldwork, but does not mention Mead’s work as one of these. Edgerton and Langness (1974) discuss a number of cases where ethnography had 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. 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.

5. ANTHROPOLOGY AND ITS AUDIENCES

But if Mead’s work was either so shoddy or passé, why did it for so long continue to be used as a textbook in college and other courses? Reflection on this, I believe, raises deeper issues concerning the relation of American cultural anthropology to its audiences.

A telling example of the disconnect between the discipline and its audiences is in one of the best early overviews of the dispute by Roy Rappaport in his "Desecrating the Holy Woman: Derek Freeman's Attack on Margaret Mead" (1986). He sets up a category of “Myth” as a third area of discourse separate from the realms of the "necessary truth of logic" and "the empirical truth of science"—that of fact and law. The realm of myth has the "truth of sanctity" (1986, p. 321). Rappaport seems to take over the positivist philosophic tenet that there is some sort of logical gulf between the three. He asserts,

> 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. (1986, p. 322 )

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17 Bennett states his critique is of interpretations, not data, though how he distinguishes these is questionable.
In dealing with mythic truth, "Where there is disparity between fact—observable states of affairs—and the truth of myth, it is fact that is wrong" (1986, p. 320). (Though he suggests the eugenics 'myth' was somehow "in violation of discovered fact or law" (1986, p. 347).) This last realm is where Rappaport places the true significance of COAS, and he insightfully discusses how it both supported and coincided with the American mythos. As science, COAS is "not so much incorrect as thin and in need of enrichment, it did make a modest contribution to Samoan ethnography." As myth he suggests it served Americans well insofar as it is "humane and liberating" (1986, p. 347).

But this separation is questionable in a number of ways. First it is not obvious that fact and myth are so entirely distinct. Facts can be taken not simply as "observable states of affairs," but as "substantiated or proven claims." What we take to be factual is influenced by our myths, but also the reverse, especially in the American mythos with its emphasis on facts and science. Even conservative religion is defended via scientific creationism. Thus Rappaport's attempt to move the debate away from issues about the facts ("even poor ethnography usually gets its facts straight" (1986, p. 344)) is questionable.

Second, he implies this myth works at the level of American culture, independent of the discipline of anthropology. This is unpersuasive on his own account. As Rappaport mentions, COAS "enjoyed substantial classroom adoption for decades" (1986, p. 324). Of course, this was predominantly because anthropologists were ordering it for their classes, a ritual academics are quite used to. As he also states, "participation in a ritual is a formal act of acceptance of whatever is represented in that ritual" (1986, p. 321). Insofar as anthropologists used COAS and others of Mead's books in their introduction to anthropology courses, because as Goodenough claims, "they turned students on" (1983, p. 906), it would seem they are implicated in maintenance of Mead’s place in the American myth, which Freeman attacked, generating controversy. Both Rappaport and Goodenough want to claim that the "crisis is in the public's view of a public idol" (Goodenough, 1983, pp. 906–907), not in anthropology itself.

This ex post facto exculpation implies a lack of responsibility for one's extra-field audience, and amounts to another version of the distancing strategy, as neatly pointed out in a letter commenting on Rappaport's piece by S. D. Cornell. Cornell writes that as he read,

I kept waiting for a resume of critical publications by those American Anthropologists who were said to have early recognized the true character of COAS. Did they try to warn a deluded public that it was largely myth, not science? Surely the least they owed to those swallowing the story and joyfully accepting it as gospel (good news!) was such a warning. Failure in that seems irresponsible of professionals in a field as closely related to human behavior as cultural anthropology. (1987/1990, p. 253)

Rappaport's rather disingenuous reply was

First, I don't think that anthropologists have been aware that a few of their texts have played a mythic role in public discourse. The mythic significance of Coming of Age in Samoa and Mead's publicly sanctified status were matters that escaped us until Professor Freeman forced us to reflect upon them. (1987/1990, p. 254)

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18 The work of the philosopher Willard Van Orman Quine on 'the web of belief', and the impossibility of sharply demarcating necessary truths and empirical truths is relevant here. See Quine (1953) and Quine and Ullian (1968).

This raises questions about how a profession polices itself, how it questions itself, and pursues these questions—and portrays itself to its inter- and extra-field audiences. For example, Marcus claims that disputes such as between Mead and Freeman, involving "radically different interpretations from fieldwork in the same culture" are "commonplace" (Marcus, 1983, p. 22) in anthropology; Heider asserts that "ethnographers rarely disagree with each other's interpretations of a culture" (1988, p. 73). A discipline that disagrees about how often it disagrees is not a pretty sight.

6. UPSHOT

Given this range of responses to Freeman, no clear anthropological view emerged as to why Freeman was clearly wrong, and Mead was right, or even any more nuanced account of how one might split the difference. I speculate that the failure to come to some disciplinary consensus left open the door, so to speak, for Freeman in his second 1999 book *The Fateful Hoaxing of Margaret Mead* to advance the very suggestive claim that Mead’s closest friends had engaged in the Samoan equivalent of pulling her leg by spinning tales of their sexual exploits with village teenage boys. Even though Orans (1999, pp.1649–50) and Shankman (2009, ch. 13) give quite plausible reasons why this is unlikely, in some inter-field circles this “myth” has quite taken hold. (Of course, as Stover (2005) indicates, this has had little impact extra-field.)

Interestingly the American Anthropological Association, in the Tasaday case and the Tierney-Chagnon dispute, has convened taskforces to investigate and pronounce on a controversy. But closure has not come easily in the second case. After accepting a committee report on the Chagnon case, the association rescinded its acceptance in 2005. Its board stated that this was because of “the members’ belief that the El Dorado investigation and the resulting report violated the association’s ban on adjudicating claims of unethical behavior and that the El Dorado investigation did not follow basic principles of fairness and due process for the accused” (American Anthropological Association, 2004). Nothing with any approaching such an official imprimatur emerged concerning Mead–Freeman (Shankman’s book may be able to do so.)

This disconnect from its extra-field audience is poignantly displayed in the paperback edition of James Clifford and George Marcus *Writing Culture: The Poetics and Politics of Ethnography* (1986). On the inside of the front cover of the book, one of the major works in recent American anthropology, the blurb reads:

> Why have ethnographic accounts recently lost so much of their authority? Why were they ever believable? Who has the right to challenge an "objective" cultural description? Was Margaret Mead simply wrong about Samoa as has recently been claimed? Or was her image of an exotic land a partial truth reflecting the concerns of her time and a complex encounter with Samoans? Are not all ethnographies rhetorical performances determined by the need to tell an effective story? Why have ethnographic accounts recently lost so much of their authority? Why were they ever believable? Who has the right to challenge an "objective" cultural description? Was Margaret Mead simply wrong about Samoa as has recently been claimed? Or was her image of an exotic land a partial truth reflecting the concerns of her time and a complex encounter with Samoans? Are not all ethnographies rhetorical performances determined by the need to tell an effective story?

The book itself contains stimulating and thoughtful essays. But it does not address these questions, much less answer them. Perhaps they did not intend to. Freeman's critique gets a brief, less than two-page mention, and Mead not much more. Very likely the blurb was written

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20 For a comprehensive archive of many documents on this see the site of Douglas Hume. Also see Headland (1992).
by some editor at the University of California Press, partly to entice purchasers, but also as a way of indicating the issues raised by the controversy. It is as if a member of the extra-field audience is speaking up from the auditorium, but not getting an answer.

Nor are the deeper theoretical and methodological questions answered much more fully. In his introduction to the book, Clifford poses questions I have tried to highlight here. "These contingencies—of language, rhetoric, power, and history—How are the truths of cultural accounts evaluated?" (1986, p. 25). But he resorts to the relatively weak rhetorical ploy of rhetorical questions, in place of answers or assertions.

Anthropology in the 1980s was in a period of flux, older paradigms being questioned, without successors clearly emerging to succeed them. Feminist anthropologists were focusing critical attention on questions of gender. Much attention was being paid to the nature of fieldwork or ethnography and to how ethnographic experience was channelled, created, and consumed by anthropologists, a concern which drew heavily upon literary criticism (Sass, 1987; Marcus & Fischer, 1986; Clifford & Marcus, 1986). And as Kuper claims, the Mead/Freeman "debate has had a special resonance within anthropology, for it has fed the contemporary unease about ethnographic fieldwork and writing" (1989, p. 454).

For those in anthropology’s intra-field audience the various responses catalogued above had genuine persuasiveness; they were more problematic for others in the inter-field audience. Many likely expected responses more along the lines of a natural science. But instead much depended on understanding the less familiar lines of an interpretive human science, such as undergirds the “perspectivalist” responses, such as that of Scheper-Hughes. Although this tactic seems a plausible resolution of some of the 'factual' issues in dispute, even some anthropologists have balked at accepting it as resolving the controversy. In his defense of "Anthropology as an Empirical Science," O'Meara points out that it seems quite implausible to take such a reconciling tactic on questions such as "the probability that 12 menstruating and thus presumably fertile girls could have engaged in sex for periods ranging from two months to four years without a single pregnancy occurring, as Mead claimed" (1989, p. 359). This I suspect is involved in the reactions of the evolutionary psychologists.

7. CONCLUSION

Describing a scientific controversy as a debate or a conversation is a tempting metaphor, but how does one evaluate it in retrospect? When a field does not address an issue, why does it not? When can one say it does not address the questions someone in an audience wants answered? Obviously, the intra-field audience of other anthropologists was most salient, and they can write replies. Inter-field relations do not seem as prominent. Certainly, the perceived standing of anthropology with regard to other social sciences and to biology was an issue. And it seems that anthropology’s standing as a source of reliable findings has suffered for many practitioners in these fields.

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21 See, for example, Hoebel, Currier, and Kaiser (1982).
22 There is also a significant "materialist" approach, broadly speaking, influenced by Marx. Participants in it do not seem to have involved themselves in the controversy, as far as I can tell.
23 Given O'Meara's meta-theoretical focus in the article, it is interesting that he begins by referring to his recent return from the field, apparently to validate his standing to speak on theory by referring to his fieldwork experience (1989 p. 354).
How does a discipline relate to its extra-field audience? American cultural anthropology today has no spokesperson or popularizer anywhere near the stature of Mead in her heyday. And it is unlikely that it could have one with the general public. And to what extent is this its responsibility? The scholarly isolationism supported by Sir Edmund Leach, a prominent British anthropologist, does not seem viable now, if it ever was. Commenting on the controversy and Mead's "impressionistic anthropology," he asserts, "Many of Mead's professional associates both in her own country and elsewhere have taken the view that they have other scholarly duties besides playing to the gallery" (1985/1990, p. 246). Such a stance seems as likely to have engendered problems as to have avoided them.

Indeed, George Marcus's comment, similar to those of Goodenough and Rappaport cited above, apparently intended to downplay Freeman's critique, that "outside of introductory courses, [Mead's] work has not generally been read in recent years" (1990, pp. 232–233) is revealing. It is in precisely such courses that anthropology has its greatest opportunity to educate its audience about itself. As the philosopher Philip Kitcher has suggested in his analysis of the conflicts between evolutionists and scientific creationists, the use by biologists of slogans, simplistic dichotomies ('proven fact' vs. 'only a theory'), and naïve philosophies of science provide readily exploitable starting points for creationists (1983, ch. 2). The extra-field audience for anthropologists, like that for evolutionary biologists, is in part a reflection of how scientists have educated it, including their critics.

It is among feminist anthropologists that the relation of scientists and their audiences is at all addressed. One explicitly feminist response to the issues raised by the Mead/Freeman controversy differs from Writing Culture precisely in its orientation to its audience. Frederick Errington and Deborah Gewertz frame their book, Cultural Alternatives and a Feminist Anthropology, by discussion of the controversy, explicitly mentioning the effect that COAS had on Gewertz's mother and many others (1987, p. 6). They conclude their re-examination of Mead's work on New Guinea with the comment, that despite its flaws,

Mead was, thus, quite correct in her perception that Chambri women conducted their lives with more assurance than do American women. . . . Through comparing the Chambri with ourselves we can, thus, become assured that male dominance is not inevitable and become more clear-sighted. (1987, pp. 140–141)

Though a piece of scholarship, their book does not place itself outside of politics. It is cultural critique of a conservative strand in American thought, but not simply that. They consciously reflect on the importance of COAS in promoting a liberal American movement, and direct their attention to how their work might support and sustain that tradition, or “myth” according to Rappaport (1986). Rather than distancing anthropology from its effects on American culture via its findings, Errington and Gewertz acknowledge this audience, an audience Mead, more than any other anthropologist, helped create.

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