

could not understand the culture she was attempting to describe. She thus became an unwitting accomplice in her own hoaxing. For Freeman, Mead's failure to accurately describe Samoa was largely the result of these personal and professional inadequacies.

Martin Orans, James Côté, Serge Tcherkézoff, and I have written elsewhere about the lack of evidence for Freeman's hoaxing argument, and the availability of more plausible alternative explanations.¹ We have also written about Samoa itself, and about how Freeman created controversy by misrepresenting Mead and other authorities on the islands. While the specific arguments that Freeman made about hoaxing and Samoa are relatively easy to critique, his more general portrayal of Mead as a young, naive, and gullible fieldworker has been pernicious, tarnishing her reputation. Indeed, in a profession where fieldwork is fundamental to an ethnographer's credibility and identity, Freeman's portrayal of Mead was especially damning.

In this article, I will show that Freeman's argument about Mead's alleged incompetence based on his interpretation of COA is not supported by a broader examination of her Samoan research. Mead's ethnography of Samoa, *Social Organization of Manu'a* (hereafter, *SOM*), was first published in 1930 and reprinted in a second edition with a new introduction by the author in 1969. It provides an important and underused source for rethinking Mead's credibility as an ethnographer. This work demonstrates that Mead was a committed fieldworker and sophisticated thinker, conducting her fieldwork under challenging circumstances and building on a limited amount of scholarship about Samoan social organization by other specialists.

Two Books: One Ethnography

Freeman focused his attention on COA, treating it as if it was an ethnography and Mead's most important work on Samoa. However, COA was not an ethnography in the conventional sense; rather it was a popular work of social commentary written for a mass audience. As Mead herself noted, it was a book that used ethnographic data to discuss how Americans might learn about adolescence from the study of other cultures. Her anticipated audience consisted of teachers, educators, and parents. The book, written and edited with this audience in mind, was published as a trade book. According to Mead (1972a:179, see also Côté [this volume]), the original manuscript, entitled "The Adolescent Girl in Samoa," was submitted to her publisher, William Morrow, as a relatively straightforward descriptive study with a minimum of social commentary. At the request of Morrow, Mead added chapters and deliberately made the manuscript more interesting and provocative for a general audience, sometimes worrying that she had gone too far.

MARGARET MEAD'S OTHER SAMOA: REREADING SOCIAL ORGANIZATION OF MANU'A

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Derek Freeman focused his attention on Margaret Mead's *Coming of Age in Samoa* to such an extent that her ethnography of Samoa, *Social Organization of Manu'a*, has been neglected. This professional monograph demonstrates Mead's credibility as an ethnographer and as a modern student of social organization. While Freeman believed that Mead was young, naive, and gullible, a closer examination of *Social Organization of Manu'a* indicates that she was energetic, resourceful, perceptive, and theoretically sophisticated. Any evaluation of Mead's Samoan research should include this enduring contribution to our knowledge of the islands.

IN HIS CRITIQUE of Margaret Mead's *Coming of Age in Samoa* (1928; hereafter, COA), Derek Freeman argued that Mead's Samoa was a "myth." For Freeman, this truth was so self-evident that he shifted his focus from Samoa to Mead herself, asking why she got Samoa "wrong," and what led her to create this mythical paradise. His answers, spelled out in *The Fateful Hoaxing of Margaret Mead: A Historical Analysis of Her Samoan Research* (Freeman 1999), focused on Mead's alleged personal and professional inadequacies. Freeman stated that Mead was hoaxed by young Samoan women who told her innocent lies that she believed as the truth, publishing them in COA. She was, in Freeman's (1997:68) words, "grossly hoaxed" and, as a result, "completely misinformed and misled virtually the entire anthropological establishment."

Mead was not only misled by Samoans but also "fatefully hoaxed" by her own inexperience and preconceptions. According to Freeman, she was a novice fieldworker and an ideologically committed cultural determinist who spent too little time in Samoa, did not master the language, and therefore

Coming of Age in Samoa became a bestseller, and because it was so visible, it also became a target for criticism in America and, later, in Samoa. When Mead briefly revisited American Samoa in 1971, young Samoans criticized her depiction of them in *COA*. Mead responded that she had written the book in another era, not anticipating that Samoans themselves would later read it; it was a book that she had written about Samoans but not for them (Mead 1972:34). Although today anthropologists accept the responsibility of writing with indigenous audiences in mind, this did not become a major concern of the profession until the 1960s.

While writing for a broad American audience, Mead did not neglect her peers in cultural anthropology. Her ethnographic monograph, *SOM*, was written for anthropologists, contained no social commentary, and was published in the Bernice P. Bishop Museum Bulletin series. It has languished in the shadow of its best-selling counterpart to such an extent that Freeman could treat it almost as a footnote to *COA* instead of acknowledging the importance of *SOM* in evaluating Mead's ethnographic research.

Mead's two books on Samoa stand in stark contrast to each other. *Coming of Age in Samoa* was a popular trade book about adolescence and sex; *Social Organization of Manu'a* was a narrowly professional monograph on social organization. *COA*, boldly comparative, was ultimately about America and how adolescence might be envisioned through the lens of Samoan culture. *SOM* was a sober, more cautious, scholarly ethnography on Samoa—her most important book on Samoa from a professional perspective. *SOM* can be criticized, but it cannot be neglected. Had this been the only book that she published on Samoa, she would have been remembered as a competent and pioneering ethnographer. Had this been the first book that she published on Samoa, *COA* might have been received somewhat differently.

Of course, most scholars who study Samoa take *SOM* for granted. It is a standard reference, and a number of Samoan specialists regard it as her best work on the islands. Even a casual reading of *SOM* reveals that it is a solid contribution, as Nancy McDowell (1980) noted in her assessment of Mead's Oceanic ethnographies. And it is *SOM* that was used by Marshall Sahlins (1957) and Irving Goldman (1970) in their synthetic and comparative works on ancient Polynesian social and political organization. It is therefore unusual that Freeman minimized its significance.

Thoroughly Modern Mead

Mead's analytical framework in *SOM* is remarkably modern. In 1928, when she completed the monograph, Mead was already making the distinction between social structure and social organization that would become familiar

decades later (Firth 1964), the former being the idealized structure while the latter is its concrete manifestation in social groups. Mead was also very interested in the dynamic and changing nature of Samoan social organization, differences between the ideal and the real, and how rules were bent to circumstances. Indeed, she sounds almost Malinowskian in her analysis. For models of how to write this ethnography, she did, in fact, study Malinowski's *Argonauts of the Western Pacific*, as well as Rivers' *The Todas*, Roscoe's *The Baganda*, and Grinnell's *The Cheyenne Indians* (Mead 1976:3).

Mead had written a Boasian doctoral dissertation in 1925 using Polynesian culture trait distributions but in *SOM*, she minimizes the analysis of culture traits and the emphasis on culture history that were so common among Franz Boas' students during the early decades of the twentieth century. In writing *SOM*, Mead was influenced by her mentor, Ruth Benedict, and Benedict's focus on configurations or "patterns" (Banner 2003:274). Even so, *SOM* itself does not seem particularly American in its conceptual framework. It reads more like well-written British social anthropology from a somewhat later period, more holistic and organismic in perspective than most American works of the period.

Samoan social organization was not easy to understand. Although non-unilineal systems of descent were documented by ethnographers such as Mead, full recognition of their nature and regional distribution did not occur until the 1950s and 1960s. Samoan social organization has been much discussed and debated, with different authors and their ideas about kindreds, cognatic descent groups, and extended kin groups vying for terminological ascendancy. Mead understood non-unilineal descent, as well as the hierarchical nature of Samoan social organization with its complex arrangement of titles at the village, district, and pan-Samoan levels. She described the parallel and interlocking hierarchies of chiefs and talking chiefs (*ali'i* and *tulafale*), the relationship between male and female sides of the family, and the important brother-sister relationship.

In addition, Mead gathered information on displays of rank at ceremonial occasions and as celebrated in Samoan mythology. There is all this and more for the careful student of Samoa, with Mead building on the work of nineteenth-century observers such as Augustin Krämer and Wilhelm von Bulow. In fact, Mead's bibliography covers the early German and French sources on the islands to a greater extent than most contemporary ethnographies.

How did Mead obtain her ethnographic data on social organization? With whom did she speak and in what contexts? We know that she spent roughly five months of her sojourn on Manu'a during a total of slightly more than eight months that she spent in American Samoa in 1925–26. Some of that

time was also devoted to the study of Samoan adolescence. Mead used conventional interviews as well as both formal and informal interactions. In her introduction to *SOM*, Mead states that:

In connection with my psychological research I became acquainted with every household group in these three villages [Luma, Siufaga, and Faleasao]. My material comes not from half a dozen informants but from scores of individuals. With the exception of two informants, all work was done in the native language. I found it particularly useful to utilize the Samoan love for pedantic controversy and to propound a question to the group and listen to the ensuing argument. As my fellowship did not provide field funds I had to rely upon the friendliness and the good will of Samoans for my material. Very little of it was gathered in formal interviews but was rather deviously extracted from the directed conversations of social groups, or at the formal receptions which the chiefs of a village accorded me on account of my rank in the native social organization (Mead [1930] 1969:5).

Mead not only interacted with Samoans informally, on three occasions she was appointed an honorary village "ceremonial maiden" (or *taupou*) by local chiefs. She thus became a genuine participant-observer in formal ceremonial events.

True Lies

Although Mead provided a brief description of her fieldwork in which she acquits herself nicely ([1930] 1969:5), Freeman (1999:161) contended that she spent too little time in the field, did not live with a Samoan family, and had too little competence in the language to understand the complexities and subtleties of the culture that she encountered. She was thus all too vulnerable to the potentially misleading stories that Samoans often tell. This problem, though, was hardly unique to Mead. It is a problem that all fieldworkers in Samoa face, even those with great expertise. As Bradd Shore (1982:128) notes, citing Mead ([1930] 1969) and Milner (1966):

Writers on Samoa have frequently commented on the Samoan passion for diversity and the graceful manipulation of social forms (Mead 1930). Milner, in the introduction to his *Samoan Dictionary*, notes the difficulty that the fieldworker in Samoa encounters in eliciting consistent explanations of even noncontroversial

matters. He refers to the 'dialectical nature' of Samoan culture where 'it is rare for information to be given, even from a reputedly sound and authentic source, without soon being contradicted from another reputable and equally reliable source' [Milner 1966: xii-xiii].

Samoan historian Malama Meleiseā (1987:vii-viii) makes the same point in a somewhat different manner, stating that even common historical knowledge can be controversial and that different versions of the truth are told to enhance the dignity of the teller's ancestors, family, or village.

Given the common difficulty of obtaining the "truth" and the ever-present danger of "multiple truths," was Margaret Mead aware of the possibility that she might be taken in? Of course she was. In her fieldnotes, Mead mentioned that Samoans lie to each other. And in *SOM*, she remarked that the problematic discourse of Samoan life was something that she sought to understand, stating that:

[Samoan] [i]nconsistencies and fabrications were not promoted by any desire for remuneration but by the forces which make for variation in the native life: family pride; love for constructing fanciful ceremonial edifices; and a desire to rearrange the social structure for personal preferment (Mead [1930] 1969:5).

Because Mead recognized the social nature of Samoan narratives, the idea that she could be somehow duped by the innocent lies of Samoans seems implausible (see also Orans 1996:90-100).

Does this mean that *SOM* is a flawless piece of scholarship? No. Samoan social organization is extremely complex, and many errors could have been made. Freeman himself, in his initial description of Samoan descent, believed that it was "purely" or "primarily" patrilineal (1948:72-73), only to realize much later that it was "optative with an emphasis on agnation" (1983:121, see also Appell and Madan 1988:9). If Freeman could make this kind of misjudgment, it is not surprising that there are some errors of fact, some omissions, and some questionable interpretations in *SOM*. For example:

- Freeman noted that Mead stated that there were no gods of war in Samoa. There were.
- Freeman also questioned Mead's account of the counterfeiting of the virginity of the *taupou*, although there is no definitive evidence on this point on either side of the argument (see Shankman 1996).

- Shore (1982:187) found that, contrary to Mead's emphasis on Samoan conflict management and suppression of public expressions of emotion, there was open expression of frustration and anger.
- Freeman found fault with Mead's use of an ahistorical ethnographic present when describing Manu'a in the 1920s, noting that Mead did not discuss the tense political situation in the islands involving the U.S. Navy at the time of her fieldwork.
- Mead occasionally used a rhetorical overstatement to emphasize her points. Thus, in describing Samoan organization, she refers to its "extreme mobility" ([1930] 1969:7), and she comments that "the traditions of Samoa are almost unprecedentedly fluid and variable" ([1930] 1969:7).

This list could be lengthened, but taken as a whole, *SOM* remains a solid ethnography and durable contribution to Samoan scholarship. Lowell Holmes and Melvin Ember, both of whom worked in the Manu'a group, independently verified most of Mead's findings on social organization. And, while *SOM* would be written differently today, at the time it was published it was well received (Linton 1932).

Could Margaret Mead Speak Samoan?

Mead was only twenty-three when she began her work in Samoa, and this was her first fieldwork. She was determined to make the most of her time in the islands. But could she have done so, having never studied the language until she arrived there and having spent only a few weeks learning Samoan on Tutuila before beginning her work in Manu'a? Freeman (1983:286) raised this issue as a way of discrediting Mead, claiming that she had "a far from perfect command" of vernacular Samoan, not to mention chiefly Samoan, thus putting her at a disadvantage in her studies.

But what exactly does "far from perfect command" of Samoan mean? Perhaps Freeman is implicitly comparing his own experience learning Samoan to Mead's. Freeman went to Samoa as a young school teacher in 1940. He spent over two years learning the language and passed an exam certifying his proficiency in 1943. But he was not an anthropologist at this time and did not return to Samoa for extended fieldwork as a professional anthropologist until 1965. Most ethnographers, including Mead, have not had this kind of experience in learning a field language.

Mead had limited funds and therefore limited time when she arrived to do her research in the islands. Should she have postponed her work in Manu'a

until she had a "perfect command" of the language? Moreover, Mead's frame of reference concerning length of fieldwork and degree of language competency in the 1920s was her peer group of American anthropologists, who often visited Native American reservations very briefly and conducted salvage ethnography with short vocabulary lists (although this was not always the case [see Lowie 1940]). At that time, Malinowskian immersion in the field and indigenous language competence were not yet integral parts of the American approach. Mead pressed ahead with her research, learning Samoan as she did fieldwork.

Her linguistic competence as displayed in *SOM* might initially lead us to believe that Mead had only a minimum understanding of Samoan. The apparent misspelling of many Samoan words is striking. In the first edition title of *SOM*, Manu'a is spelled Manua! Faleasao, a village where Mead worked, also appears to be misspelled. When a new edition of *SOM* appeared in 1969, Freeman published an *errata* in the *Journal of the Polynesian Society* (1972) listing 214 spelling errors. Yet in her glossary to *SOM*, Mead explained her spelling choices very clearly, stating that:

In my spelling of Samoan words I have adhered to the usage of former students of the language. I did not make any formal study of the language but simply used it as a means of communication. The fact that many Samoans are letter-perfect in writing their language as the missionaries first translated it, made any departure on my part a matter for violent controversy. . . . In the body of the text Samoan geographic terms and proper names are spelled in accordance with Bishop Museum practice (Mead [1930] 1969:213).

Thus many apparent "errors" are explained. Moreover, in his analysis of Mead's language competency, Martin Orans (1996:20–21) notes that many of Mead's "errors" involve the use of the macron or are "vowel-errors typically difficult for English speakers." Other errors remain. Tcherkézoff (2001:69) has argued that Mead's misunderstanding of the gendered nature of Samoan discourse about sex may have misled her on the nature of sexual conduct in COA. Yet these problems do not seem to have seriously compromised Mead's overall description of Samoan social organization in *SOM*.

Nevertheless, the question must be asked: Was Mead competent in Samoan? The best evidence comes from Freeman himself. Working in the Mead Archive in the Library of Congress, Freeman reconstructed Mead's fieldwork in Manu'a for his second book on the controversy. He describes how the U.S. Navy Lieutenant Commander William Edell, a chaplain, used Mead to do most of his interpreting of Samoan, even though Mead

acknowledged that there were three people on Ta'u who spoke much better English than she spoke Samoan (Freeman 1999:115). Freeman continued:

This included such tasks as translating into Samoan official letters to the high chief of the off-lying island of Olosega. When this happened, Mead would have Fatuola, the official Samoan interpreter at the naval dispensary, go over her translation. When Fatuola found 'only three mistakes' in one of her translations, she felt 'quite puffed up.' Mead was also called on to act as an interpreter when Lieutenant Commander Edell 'held court over a land case.' She was she remarked 'properly nervous.' But that she was able to act as an interpreter in a case involving chiefs is evidence that by the end of January 1926, she had become reasonably competent in Samoan (quoted in Freeman 1999:115-116).

In other words, within five months of arriving in Samoa, and well before her alleged hoaxing in March of 1926, Mead was "reasonably competent" in Samoan, to use Freeman's (1999:116) own words.

There was also a fair amount of English spoken in Manu'a. Orans has argued that had Samoans spoken as little English as Mead stated, she might well have had trouble understanding the culture. But Orans found that Mead's repeated claim that she spoke only Samoan with all but a few informants was improbable. Having read Mead's field notes and letters, Orans believes that a number of Samoans in Manu'a spoke English with varying degrees of proficiency and that Mead spoke English with them, thus mitigating her self-admitted limitations in Samoan during the early months of her fieldwork (Orans 1996:20-23). By February of 1926, she felt herself proficient enough to go to the village of Fitiuta, where almost no English was spoken (Freeman 1999:123). Thus, given the combination of Mead's developing competence in Samoan and the English spoken by Samoans on Manu'a, there is reason to believe that Mead understood Samoan well enough to do credible fieldwork, although she did not speak it perfectly.

Freeman's Use of *Social Organization of Manu'a* in His Critique of Mead

Freeman, in recounting his own intellectual history, stated that it was in Samoa, where he first worked as a school teacher, that he gradually realized that Margaret Mead was wrong. He commented that, "By the time I left Samoa in November 1943 I knew that I would one day face the responsibility

ity of writing a refutation of Mead's Samoan findings" (Freeman 1983:xiv). When did that day come? And to which findings is he referring? The findings to which Freeman has attended are largely those in *Coming of Age in Samoa* rather than those in *Social Organization of Manu'a*, and his critique of COA did not appear in manuscript form until the mid-1960s. This was not due to lack of opportunity, since Freeman had been writing about Samoa since the late 1940s. Yet he did not criticize Mead in print until much later, and even then he did not criticize her on the subject of social organization.

In 1948, Freeman completed his Postgraduate Diploma in Anthropology at the London School of Economics under the direction of Raymond Firth. Freeman's thesis topic was the social organization of a Samoan village, and his thesis is the most substantial ethnographic work that Freeman produced on Samoa during his entire career. Given their shared interest in social organization in the same culture and given Freeman's stated interest in refuting Mead at that time, SOM would seem an obvious place to commence a discussion of Mead's findings. SOM was the most important ethnographic work on Samoan social organization in English at that time. At the very least, Freeman should have cited it.

In a thesis that is over 300 typewritten pages in length, none of Mead's published work on the islands is cited, let alone discussed. There is no reference to SOM, nor to its empirical findings and conceptual approach. In SOM, Mead accurately described non-unilineal descent in Samoa; in his thesis, Freeman emphasized patrilineal descent. In SOM, Mead discussed the concept of social structure—whether it is an ideal construct or a concrete phenomenon; yet in the conclusion to his thesis two decades later, Freeman wrestled with the same problem with no mention of Mead. Had Mead's work on Samoan social organization been flawed, Freeman could have addressed and refuted it in his thesis. The omission of SOM in Freeman's thesis is all the more puzzling because his advisor, Raymond Firth, had cited SOM favorably in his own ethnography, *We, The Tikopia* (1936).

Freeman had additional opportunities to criticize Mead on the subject of Samoan social organization. He was not reluctant to criticize other scholars' work on Samoan social organization. In 1964, Freeman published an article on Samoan social and political organization in the *American Anthropologist*, criticizing the work of Melvin Ember and Marshall Sahlins for their alleged misunderstanding of Samoan kin groups. Yet, in this article, Mead's SOM is cited by Freeman (1964) as an authoritative source in support of his critique of Ember and Sahlins.

After 1983, Freeman published two books on Mead and numerous articles on the Mead-Freeman controversy, but these are primarily about COA; SOM is rarely mentioned. The third section of *Margaret Mead and Samoa*,

entitled "A Refutation of Mead's Conclusions," covers eleven different topics in 165 pages (Freeman 1983). Yet there is little discussion of social organization and little recognition of *SOM*. Although Freeman did publish a linguistic *errata* to *SOM* in 1972, he has very little to say about the ethnographic substance of *SOM* in the context of the Mead-Freeman controversy, possibly because *SOM* undermines Freeman's argument about Mead's competence as an ethnographer.

Conclusion

Freeman employed a caricature of Mead to explain why she got Samoa "wrong" in *COA*. This negative portrayal has been especially damaging to her reputation because it cast doubt on her professional credibility as an ethnographer. *Social Organization of Manu'a* demonstrates that Mead was a competent ethnographer who spoke Samoan reasonably well and produced an ethnographic monograph of enduring value. Any review of Mead's Samoan research should acknowledge *SOM*, a point that would be even more obvious had Mead published only this work. Of course, *SOM* is not a surrogate for *Coming of Age in Samoa*, and is not flawless, but it is a monograph that deserves recognition in its own right.

For these reasons, my reading of Mead's Samoan research is very different than Freeman's. Mead was young and without previous field experience, but she was energetic, resourceful, perceptive, and a very quick study. She accomplished an enormous amount in a very short period of time. Freeman (1999:157) himself acknowledged Mead's "phenomenal energy" in the field (see also Silverman, this volume).

Samoa was Mead's first field site, but not her last. Within two years of her return, she completed two books on her Samoan research and was off to new field sites. If Mead had not been a committed ethnographer, it is difficult to imagine why she would assume these new and often more difficult challenges. Mead learned how to become a better ethnographer as the result of her Samoan fieldwork (Bateson 1984). As if to underscore her commitment to ethnography, she did what no other American anthropologist has done before or since. Between 1925 and 1939, Mead studied the Samoans, Manus, Omaha, Arapesh, Tchambuli, Mundugumor, Iatmul, and Balinese, and wrote professional and popular works on almost all these cultures. No American ethnographer has done as much work in as many different cultures in so short a period of time and produced as much as professional and popular work as Mead did.

Mead's ethnographic work is not as well remembered as her popular work, perhaps because she wrote for different audiences. Mead was both a popularizer and an ethnographer. Her popular work was subject to close scrutiny

precisely because it was popular. But she was also a dedicated ethnographer. Although her ethnographies were eclipsed by the work of later scholars, Mead is recognized as a pioneer in conducting fieldwork on childhood, adolescence, and gender on which other anthropologists built. Mead was also an innovator in ethnographic method, including the use of team research, psychological testing, still photography, and motion pictures. While Mead should be remembered as the great popularizer of anthropology in the twentieth century, she should also be remembered for her commitment to ethnography, including her first ethnography—*Social Organization of Manu'a*.

NOTES

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1. For responses to Freeman's hoaxing argument see Côté (2000, this volume), Orans (1996, 1999), Shankman (1996, 2000, 2001), and Tcherkézoff (2001). Although all agree on the weaknesses in Freeman's hoaxing argument, Orans and Tcherkézoff are very critical of *COA* as well.

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