

DEREK FREEMAN AND MARGARET MEAD: WHAT DID HE KNOW, AND WHEN DID HE KNOW IT?

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Derek Freeman's published autobiographical statement and his biographers' account of his life report that, before he went to Samoa in 1940, Freeman was a cultural determinist strongly influenced by Margaret Mead's work. While in the islands, Freeman stated that he discovered that Mead was wrong about Samoan culture and felt responsible for refuting her work, thus establishing a linear progression in his critique of Mead from his own first trip to the islands to the eventual publication, some four decades later, of *Margaret Mead and Samoa: The Making and Unmaking of an Anthropological Myth* (1983). Interviews with Freeman suggest, however, that this narrative is incomplete and that the path he took was more circuitous and indirect. In fact, although Freeman had opportunities to do so, for more than two decades he avoided published criticism of Mead's work. This more complex narrative raises questions about what Freeman knew about Mead's work, when he knew it, and what he did with that knowledge.

Introduction

Of the many issues in the Margaret Mead–Derek Freeman controversy, one of the most intriguing is the chronology of Freeman's critique of Mead's work on Samoa. At what point did Mead's work become a focus of Freeman's attention? How well did Freeman understand Mead's work on Samoa early in his career? And when did his critique of Mead move from the private sphere to the published academic sphere? Addressing these questions is of interest because Freeman's (1983) published autobiographical statement

and Appell and Madan's (1988) biographical account are incomplete in this regard. Freeman himself has provided additional information in interviews with Frank Heimans (2001) and Hiram Caton (2002). These materials now supplement the published record and provide a more complete, although still imperfect, understanding of the young Derek Freeman's relationship to Mead and her Samoan scholarship.

Freeman's (1983) brief autobiographical account about his early involvement with Mead's work can be found in the Preface of *Margaret Mead and Samoa: The Making and Unmaking of an Anthropological Myth*. There he discusses the circumstances that led to his critique of Mead, commenting in the preface that it was "by accident that I have come to write this book" (Freeman 1983, xiii). This "accident" began in the late 1930s when, as an undergraduate at Victoria University College in Wellington, Freeman took courses with Ernest Beaglehole, who, he reminds us, had studied with Edward Sapir at Yale. Freeman noted that

Beaglehole's anthropology was very similar to Mead's and it was this approach, stemming from the teaching of Boas, that I adopted when, with Beaglehole's encouragement, I decided to do ethnographic research in the Samoan islands. When I reached Western Samoa in April 1940, I was very much a cultural determinist. *Coming of Age in Samoa* had been unreservedly commended to me by Beaglehole, and my credence in Mead's findings was complete. (1983, xiii)

Freeman continues his account, stating that, while in Western Samoa, he mastered the language after two years of study and was adopted into a Samoan family in the village of Sa'anapu on the south coast of Upolu. After living for about fifteen months in Sa'anapu and having been conferred a princely title in the village, Freeman felt that he had come to know a good deal about "the realities of Samoan life" (1983, xiv). Only then, on the basis of considerable experience in the islands, did Freeman begin to question his confidence in Mead's findings, remarking that

[i]n my early work [in Samoa] I had, in my unquestioning acceptance of Mead's writings, tended to dismiss all evidence that ran counter to her findings. By the end of 1942, however, it had become apparent to me that much of what she had written about the inhabitants of Manu'a in eastern Samoa did not apply to the people of western Samoa. After I had been assured by Samoans who had lived in Manu'a that life there was essentially the same

as in the western islands, I realized that I would have to make one of the objectives of my research the systematic testing of Mead's depiction of Samoan culture. (1983, xiv)

At that moment, Freeman recognized his obligation to correct Mead's errors: "By the time I left Samoa in November 1943 I knew that I would one day face the responsibility of writing a refutation of Mead's Samoan findings" (1983, xiv). This responsibility, of course, culminated in the publication of *Margaret Mead and Samoa* in 1983.

Freeman's brief autobiographical account gives his critique of Mead a coherent narrative with a noble purpose. Freeman had sincerely embraced cultural determinism, handed down in lineal fashion from Boas through Sapir to Beaglehole to Freeman. Although he had initially accepted Mead's Samoan findings, based on his own research experience in the islands in the early 1940s, Freeman came to know the realities of Samoan life and felt it his duty to refute Mead, presumably in published form. This narrative gives Freeman's critique of Mead a sense of authenticity and, indeed, inevitability. Mead had not accurately portrayed the Samoa that Freeman had come to know. It was therefore appropriate that he would be responsible for correcting the ethnographic record. In contrast to an allegedly inexperienced, naive, and gullible Mead, Freeman overcame his own naïveté about Mead's work while gaining the ethnographic credentials to put forth his critique.

As convincing and plausible as this narrative seems, it is nevertheless incomplete and, in some ways, misleading. Freeman gives the impression that he knew Mead's work well, that he went to Samoa to do ethnographic research, that Mead's perspective guided his thinking for much of his time in the islands, that he had taken as his responsibility the refutation of her work when he realized her alleged error, and that this was a priority in his own work. However, the interviews of Freeman by Heimans (2001) and Caton (2002) complicate this account and, in some ways, subvert it. Freeman's path was less direct and more haphazard than the one presented in his published, retrospective account. For example, it now seems that Freeman could not remember whether he had read *Coming of Age in Samoa* before going to the islands. In the 2001 interview with Heimans, Freeman readily acknowledged that he knew almost nothing about the islands when he arrived there. And while Freeman stated that Beaglehole encouraged him to do ethnographic research, Freeman had limited training in anthropology and no undergraduate degree in any subject. He went to Samoa not as an ethnographic researcher but as a schoolteacher, and the research he conducted in his spare time, by his own account, was initially archaeological and curatorial rather than ethnographic.

It is not clear exactly when Freeman began to question Mead's work in a professional forum. Freeman privately critiqued Mead's research and expressed his personal dislike for her among his colleagues as a graduate student during the late 1940s and early 1950s. Yet he did not correspond with her until 1957, nor did he publish criticism of her work until 1972, neglecting a major opportunity to do so in his 1948 postgraduate diploma thesis on Samoan social structure at the London School of Economics and another opportunity to do so in 1964 in an article in the *American Anthropologist* on Samoan kinship and political organization. Indeed, Freeman seemed to be unaware of or unconcerned with Mead's (1930) professional monograph on Samoa, *Social Organization of Manu'a*, until much later in his career and to the detriment of his 1948 thesis. Thus, a closer look at Freeman's relationship to Mead's Samoan work through the 1960s raises the question, What did he know about Mead's Samoan research, and when did he know it?

Right from the Start?

Freeman's interview with Frank Heimans on February 12, 2001, just prior to his death, is one source that provides more detail about Freeman's intellectual and personal journey during the early years of his career. Heimans (1988) had produced the documentary film *Margaret Mead and Samoa* with Freeman's collaboration. The film was, in part, a visual version of Freeman's book and articles on Mead, and while the film was quite critical of Mead, it added little to our knowledge of what Freeman knew of Mead's work in Samoa during the late 1930s and early 1940s. Heimans's interview with Freeman is therefore helpful in this regard. Freeman was also interviewed between 1985 and 1987 by Hiram Caton, a professor of history and politics at Griffith University and a colleague of Freeman's. Caton had become close to Freeman and edited *The Samoa Reader* (1990) during the early years of the Mead-Freeman controversy before becoming more critical of Freeman later (Caton 2002: v-vi; 2005). What do these interviews add to our knowledge?

According to the 2001 interview with Heimans, Freeman, as a young college student of seventeen, described himself as having been "intensely interested in the human condition" and of having "kind of anthropological interests from the beginning" (2001, 7). Yet Victoria University College in Wellington did not offer courses in anthropology. Freeman enrolled in psychology, economics, and philosophy: "But in 1937, a man called Ernest Beaglehole, who was a psychologist who had taken up anthropology, joined the psychology department and I came under his influence" (Heimans

2001, 7). As Freeman (1983, xiii) recounted in *Margaret Mead and Samoa*, Beaglehole had studied with Edward Sapir, Boas's brilliant protégé, after Beaglehole received his PhD from the London School of Economics. It was through Beaglehole that Freeman says that he learned the doctrine of cultural determinism advocated by Boas and Mead, and Freeman implies that Sapir was influential in the transmission of Mead's work to Freeman. But what Beaglehole learned about Mead from Sapir is unclear.

Sapir had been involved in a passionate love affair with Mead during the mid-1920s before she went to American Samoa. He had urged Mead to leave her husband, Luther Cressman, and to marry him. While in Samoa during 1925–1926, Mead rejected Sapir, and he became openly antagonistic toward her. In an interview with Caton (2002, 32), Freeman reported that anthropologist Weston LaBarre sent Freeman a letter stating that Sapir had labeled Mead a “pathological liar” while he was teaching at Yale. And in a letter to Ruth Benedict, written in 1929, Sapir had called Mead a “loathsome bitch” and “a malodorous symbol of everything he hated in American culture” (quoted in Banner 2004, 24). Furthermore, in a review published in the *New Republic*, Sapir referred to *Coming of Age in Samoa* as “cheap and dull” (1929, 279). In another article in another magazine, Sapir alluded to Mead and Benedict, deploring feminists and lesbians, whom he accused of being both “frigid and ambitious,” and attacked “emancipated women” in general as “being little better than prostitutes” (quoted in Molloy 2004, 39). These articles shocked Mead and Benedict and ended their further collaboration with Sapir (Silverstein 2004, 152).

Given this antagonistic relationship, it is difficult to imagine that, when Beaglehole was at Yale, Sapir would have praised Mead and *Coming of Age in Samoa*. So it is doubtful that Beaglehole's high regard for Mead came directly from Sapir; more likely, it came from Beaglehole's friendship with her. After completing his doctorate in 1931, Beaglehole received a Commonwealth Fund Fellowship to travel to Yale to work with American psychological anthropologists, particularly Sapir. He later met both Benedict and Mead. As Freeman noted in his interview with Heimans, Beaglehole “became a personal friend of Margaret Mead's” (2001, 11); in fact, both Ernest and Pearl Beaglehole became lifelong friends of Mead.

Although Beaglehole may have conveyed the general idea of cultural determinism to Freeman, it was certainly not a unique perspective in anthropological circles.¹ Nor does it seem that Freeman fully embraced this doctrine, even before going to Samoa and despite his published statement to the contrary. In his interview with Heimans, Freeman remembered that he began to question cultural determinism as a result of an intellectual encounter with Jiddu Krishnamurti, the noted Telugu mystic, in 1939:

[Krishnamurti's] view was a kind of Buddhist view. He was totally against all social conditioning which he thought made a mess of the mind, and was a master of meditation and you had to rid yourself of all the false ideas in religion and so on and kind of reach a pristine state from where you could make good choices. I was the literary editor of the student newspaper called *Salient* and we were specialising in debunking people. . . . I was sent along to debunk Krishnamurti, but he debunked me. I mean, I was hugely impressed by what he was saying. He was deeply questioning cultural determinist theory, and he was no relativist. He said that you could, by severe intellectual effort, win through to an enlightened state. (Heimans 2001, 11)

Freeman was so impressed with Krishnamurti's views that he took two weeks off from teaching in order to have private meetings with him. Krishnamurti encouraged Freeman to doubt cultural determinism *before* he was aware that Mead was allegedly wrong about both Samoa and cultural determinism.²

Ethnographic Research in Samoa

Freeman had to work in order to support himself after his first year at the university, and he became a schoolteacher of young children in New Zealand for about two years in the late 1930s, earning a Trained Teacher's Certificate from Wellington Training College for Teachers in the process. On seeing a position in the Department of Education in Western Samoa advertised in the *Teacher's Gazette*, Freeman applied for and obtained it. As he noted, "[I]t was through teaching that I got to Samoa, you see" (Heimans 2001, 9). However, Freeman did not know much about Samoa by his own admission (Heimans 2001, 13). Furthermore, his desire to go to the islands was not animated by his specific knowledge of them so much as his alienation from his home country. As he explained to Heimans, "I wanted to escape from New Zealand society and from the whole suffocating atmosphere there" (Heimans 2001, 12).

Freeman was looking for new experiences in life, and the islands were for him "the most romantic and lush place"; they were "overpowering" (Heimans 2001, 13). Freeman stated in *Margaret Mead and Samoa* (1983, xii) that, with Beaglehole's encouragement, he decided to conduct "ethnographic research" in Samoa. Yet it is not clear how he learned to do so. Beaglehole did give Freeman a list of readings on the subject, but did this list include readings about Samoa? Again, Freeman recalled that he "didn't know much about it [Samoa] at all" (Heimans 2001, 13).

Had Freeman actually read Mead's work before arriving in Western Samoa in 1940? In his interview with Heimans, Freeman was asked, "When you arrived in Samoa, did you have Margaret Mead's book, *Coming of Age in Samoa*, under your . . . :

Derek Freeman: "I sent away for it and I got the volume. It's called *From the South Seas* and it has *Coming of Age in Samoa*, *Growing Up in New Guinea* and *Sex and Temperament in Three Primitive Societies*. I had Boas' *General Anthropology* textbook. But I wrote to America and got them. I was already collecting books."

Frank Heimans: "So you read Margaret Mead's book in Samoa itself?"

Derek Freeman: "Yes."

Frank Heimans: "Was that the first time you'd read it?"

Derek Freeman: "I can't be clear about that. I probably did read it in New Zealand because Beaglehole was always talking about it. One, at any rate, knew what the message was." (Heimans 2001: 15–16)

The book that Freeman ordered, *From the South Seas*, was published in 1939, after Freeman had taken course work from Beaglehole and just before he went to Samoa.

Although he did not complete his undergraduate degree and had limited training in anthropology from Beaglehole, Freeman was interested in other cultures and in doing research. His teaching position in Samoa facilitated this desire. Working in the port town of Apia with young "half-caste" (part Samoan/part European) and European children, Freeman taught in the mornings and so had his afternoons free to practice the Samoan language and to learn about Samoan culture (Heimans 2001, 14). The research that he did initially was archaeological in nature, beginning at Seua Cave. As Freeman recalled,

I went and did archaeological work and found my first stone adze, which was a huge thrill. Then I would write down the story of it from the local Samoans when I'd got my Samoan well enough under control. I did a number of other studies like that. (Heimans 2001, 14)

Some of these archaeological accounts were later published in the *Journal of the Polynesian Society*. Freeman also collected artifacts for H. D. Skinner of the Otago Museum in New Zealand.

After visiting the village of Sa'anapu on the island of Upolu and being adopted by the Samoan family of a chief with whom he lived, Freeman began studying the village (Heimans 2001, 16); the chief gave him "secret information," including genealogies. Given Freeman's interest in social organization, it is interesting that he does not mention reading Mead's (1930) *Social Organization of Manu'a* during the interview discussion of his own fieldwork. The monograph would have been quite useful on this subject.

During his stay in Sa'anapu, Freeman acquired a title or, to be more precise, "the title of the heir apparent to the high chief, Anapu" (Heimans 2001, 17). This meant that Freeman could sit in the council of chiefs and listen to cases involving serious crimes, including rape. He also learned about the protection of young women and punishment of those who were engaged in sexual activity. Freeman regarded this village experience as "an extraordinary stroke of good luck, because now I was right in a Samoan family. See, Margaret Mead lived in a United States naval dispensary with expatriate Americans" (Heimans 2001, 15). Thus, Freeman believed that his authentic village experience stood in sharp contrast to Mead's allegedly inauthentic one.

Yet Freeman's most significant insights about Samoan sexual conduct came not from his intermittent visits to the village of Sa'anapu but rather from his time in the port town of Apia. On the boat to Samoa in 1940, Freeman met Dr. Hans Neumann, an Austrian doctor and refugee from Hitler. They became friends, and sometime later in the islands,

[a]fter I had sort of established myself in Samoa and become a kind of local authority on the folklore and archaeological sites and I was speaking Samoan fluently and so on, he [Neumann] invited me to come up and live with him. He had a house, a very lavish house, opposite the hospital and I went up and lived with him there and quickly established ties with the Samoan nurses and became very friendly with one of them in particular. She quickly divulged what the Samoa system was, that she would go to bed with you but you must make her a promise that you would not attempt to deflower her, you see. It's not a sexual thing. I mean, they don't mind sex, but they want to be virgins when they marry, you see, for prestige reasons. Then I checked this out with other nurses and they all confirmed it. I then looked at the law cases and they all confirmed it and it was quite clear that Margaret Mead was totally wrong. (Heimans 2001, 18)

From his published account, Freeman gives the impression that his data on Samoan sexual conduct had come largely from his fieldwork experience in the village of Sa'anapu, and some of it did. But from the unpublished interview, it appears that critical information was obtained from Samoan nurses who were familiar with both Samoan and Western traditions while Freeman was living as a European in a European house opposite the main hospital in the port town of Apia. As sources of information, these port town nurses are perfectly acceptable, but Freeman's narrative of authentic knowledge gained from village experience now seems less compelling.

Freeman's Knowledge of Samoan Sexual Conduct

Freeman's published autobiographical statement and his unpublished interview with Heimans suggest how systematic he was in obtaining data and reaching his conclusions about Samoan sexual conduct. Yet Freeman's data depicting Samoan sexual restrictiveness in the early 1940s are incomplete and misleading. The nurses with whom Freeman spoke sincerely believed that Samoan sexual conduct was restrictive and required virginity—particularly for women—a view that was and continues to be the public ideology of sexual conduct in Samoa. Yet between 1942 and 1945, a period that overlapped substantially with Freeman's first stay in the islands, World War II brought tens of thousands of American servicemen to the islands, where they engaged in sexual relationships with Samoan women and produced a sizable number of offspring.

As I have noted in other publications, the war years were a period of major change in the islands, which included a dramatic increase during this period in interethnic unions between American servicemen and Samoan women (Shankman 1996, 2001, 2004). W. E. H. Stanner, an anthropologist and postwar observer, found that

[a] great deal of sexual promiscuity occurred between Samoan or part-Samoan women and American troops. Responsible Samoans said that actual prostitution was restricted to a very small group of women. Romantic, at least friendly, relationships were very common. One mission society reported that in Upolu alone there were 1,200 known instances of illegitimate children by American soldiers from Samoan girls. The official statistics were not revealed, but put the number of known illegitimate children much lower. Only a few incidents were caused by the jealousy of Samoan men, and not much was made of them by either side. Some villages were said to have set up a special curfew for their girls, and at

Falefa (near Apia) no troops except officers on business were allowed to enter *fale* [houses]. With troops so widely dispersed in an area so densely settled it is impossible to prevent familiar association. Many soldiers regularly visited girlfriends within the villages, by no means only with single intention, but the entrance-gates to the airport, it was said, became known among Samoans as “the gates of sin.” At least one *matai* [chief] was summarily expelled from his church congregation and from the society of the village on suspicion of procuring girls for prostitution. (Stanner 1953, 327)

The well-known author James Michener (1992) reported in a discreet but detailed manner his own participation in one such relationship. As a lieutenant, Michener was responsible for base security. Early in his Western Samoan tour, he found a base where, during the day, sixty to seventy-two American men were on duty, yet at night there were only six. Concerned about security, Michener learned that military vehicles took the men to villages at dusk, where they were dropped off to meet with their Samoan girlfriends for the evening. Michener saw firsthand that Samoans openly welcomed these evening arrangements. In the morning, the servicemen were picked up and returned to their base. Michener himself was invited by a high-ranking Samoan chief to enter into such a relationship with his daughter and father her child (1992: 38–40). As a result of his own involvement, Michener felt so compromised that he never reported these relationships to his superior officers.

These accounts from wartime Samoa indicate that relationships between American servicemen and Samoan women developed quickly and often, although many villages more distant from bases and roads had little contact with American troops. Where relationships took place, young women were allowed and even encouraged by their families to enter into them, with contact to a large degree under the control of parents and the village. There were relatively few overt conflicts between families and American troops. Although Samoans were perfectly capable of secluding their daughters and punishing them for affairs with Americans and for having children with them, for the most part they did not do so. This pattern of permissive sexual conduct during World War II is very difficult to reconcile with Freeman’s portrait of a “severe Christian morality” and a culture in which he stated that “virginity was probably carried to a greater extreme than in any society known to anthropology” (1983, 250). It is also at odds with Freeman’s assertion that major changes in Samoan sexual conduct did not begin to occur until the 1950s (1983, 350).

Because the wartime occupation of Western Samoa by Americans began in 1942, perhaps the best opportunity to view these changes would have been during the time period shortly before then and immediately thereafter. Freeman arrived in Western Samoa in April 1940 and departed in November 1943. He was, therefore, in a position to have observed or at least known of these relationships. Freeman was a teacher of part Samoan/part European children in Apia, who were the offspring of earlier Samoan-European unions. As a New Zealander whose country was the governing power in Western Samoa at that time, Freeman served in the Local Defense Force and later served in the Royal New Zealand Volunteer Naval Reserve for the rest of the war. It was at this time that premarital sexual activity in Samoa was perhaps most apparent. Yet, although Freeman had gained valuable insights into the restrictive public ideology of sexual conduct, he neglected the widespread phenomenon of permitted interethnic relationships. At this time, what Freeman emphasized was that he would “one day have to face the responsibility of writing a refutation of Mead’s Samoan findings” (1983, xiv).

Freeman’s Knowledge of Mead’s Samoan Work

Freeman remembered being critical of Mead’s work immediately after the war, stating, “When I got back to New Zealand I reported this to Beaglehole, that I thought Margaret Mead had made an astronomical error and he sort of just laughed at me” (Heimans 2001, 19; see also Freeman 1996, 190). When he went to England to do graduate work after briefly revisiting Samoa in 1946, Freeman continued to criticize Mead’s research and was remembered for his personal antipathy to her. Robin Fox, a colleague of Freeman, recalled that Freeman “seemed to have a special place in hell reserved for Margaret Mead, for reasons not at all clear at the time” (2004, 339). Fox also noted that “the rest of the British school seemed to see her [Mead’s] fault as a case of whoring after cheap fame instead of doing a professional job of fieldwork” (2004, 339). However, at least one British social anthropologist, Raymond Firth, the foremost Polynesian expert of his generation, was not hostile to Mead and had favorably cited her work in his own. Firth became Freeman’s adviser for his postgraduate diploma degree program in anthropology at the London School of Economics.

Since he did not have an undergraduate degree, Freeman was required to enter the postgraduate diploma program, which was part of a larger advanced graduate program. Freeman now had an opportunity to refute Mead’s Samoan findings in his postgraduate diploma thesis on Samoa, which has been recently published (Freeman 2006). Titled “The Social

Structure of a Samoan Village Community" (1948), it remains Freeman's most extensive ethnographic report on Samoa, containing a very detailed description of the village of Sa'anapu. Had Freeman been interested in criticizing Mead, this would have been an appropriate place to do so, especially since Mead's *Social Organization of Manu'a* was the standard ethnographic work on Samoan social organization in English at the time. Both Freeman and Mead had written on the same topic, and *Social Organization of Manu'a* was Mead's professional monograph, in contrast to *Coming of Age in Samoa*, a popular trade book.

So where is *Social Organization of Manu'a* in Freeman's thesis, which runs to over 300 typewritten pages? It is nowhere to be found. It is neither cited nor discussed, nor are any of Mead's other works on Samoa, although some relevant works by other scholars are. Was Freeman unaware of *Social Organization of Manu'a*? Freeman biographer and historian Peter Hempenstall has noted the absence of *Social Organization of Manu'a* in Freeman's two seminar papers on Samoa, including one titled "On Samoan Social Organization." He concludes that "there is no indication that he [Freeman] was familiar with it, although he was in possession of Mead's *Coming of Age in Samoa* (Hempenstall 2004, 242). Moreover, Hempenstall (2004, 242) argues that Freeman's retrospective autobiographical account of his critique of Mead implies that he "recognized her flaws early on in his own Samoan days. There is no sign in this [seminar] paper that this was the case."

If Hempenstall is correct, then Freeman's early knowledge of Mead's work may have been limited solely to *Coming of Age in Samoa*. Yet Freeman, in his interviews with Caton, implied that he had read *Social Organization of Manu'a*. Freeman recalled that when he arrived at the London School of Economics, he already disagreed with Firth about Mead and Samoa (Caton 2002, 1). Freeman argued with Firth, stating that Mead's work had muddled and confused scholars studying Samoa and that it needed to be refuted because of its negative influence on the field (Caton 2002: 1–3). Firth did not agree. Moreover, Firth had cited *Social Organization of Manu'a* in his own ethnography *We, the Tikopia* (1936), and Freeman remembered that "there was no criticism of it" there (Caton 2002, 3), implying that Freeman had his own critique of *Social Organization of Manu'a* at the time.

If Freeman did know about and was critical of *Social Organization of Manu'a*, then the absence of any mention of it in his thesis and seminar papers would have been the result of a choice to deliberately neglect it. But why would Freeman do so? Could it have been the nature of the professor–student relationship he had with Firth? Freeman remembered

Firth's criticism of his work, but did he fear that Firth would reject his thesis if he criticized Mead? In an interview with Freeman, Caton raised this possibility:

Caton: "Did you construe Firth's views on this matter as being more than friendly persuasion, did it contain to you any sort of professional threat?"

Freeman: "Oh, not really I think he . . . he'd been, I'd been in a pupil-teacher relationship with him."

Caton: "Yes."

Freeman: "And he let me know what his views were, but by this time I was a professor at ANU [the Australian National University] and he had no constraints over me anymore. It's very much a personal matter." (Caton 2002, 2)

It is not clear from this interview whether Freeman felt pressure from Firth to refrain from criticizing Mead in his graduate work, possibly including his thesis. Nor is it clear why Firth and other faculty members would allow Freeman to omit any reference to Mead's work, especially *Social Organization of Manu'a*. Although Freeman would strongly criticize *Social Organization of Manu'a* for linguistic errors in his later publication (1972), his first mention of the monograph in print, to my knowledge, came in 1964, when he *favorably* cited it in his critique of Marshall Sahlins's and Melvin Ember's work on Samoan social organization, published in the *American Anthropologist* (Freeman 1964). This was a full sixteen years after his thesis was completed.

Because *Social Organization of Manu'a* was so relevant to Freeman's own fieldwork in Samoa and to his area of expertise in graduate school, his omission of Mead's work had theoretical and ethnographic consequences. For example, Freeman could have benefited from reading *Social Organization of Manu'a* on a theoretical level. In 1930, Mead discussed the distinction between social organization and social structure, a distinction that Freeman would also address in his 1948 thesis without reference to her work.

Mead's monograph could have also assisted Freeman in his own ethnographic descriptions of Samoan social organization. Thus, in *Social Organization of Manu'a*, Mead correctly identified Samoan descent as "bilateral" (1930, 18), whereas Freeman's thesis identified Samoan descent as purely or primarily "patrilineal" (1948: 72-73), an error he would later acknowledge (Appell and Madan 1988, 9). Interestingly, Freeman would

become well known for his analysis of cognatic descent groups among the Iban of Borneo and would win the Curl Bequest Prize in 1961 for his essay on the kindred. Yet in his thesis, not only did he not recognize Mead's early work on descent in Samoa, he may have missed an opportunity to have better understood bilateral descent and the kindred much earlier in his career.

Another instance where Freeman's thesis could have benefited from a closer reading of Mead is his analysis of the decline of the *taupou* system, which he discussed in some detail. Freeman's thesis cited Felix Keesing's essay in Oceania, "The Taupo System: A Study of Institutional Decline" (1937), the only publication of that era to deal exclusively with the decline of the Samoan system of institutionalized virginity. Keesing in turn cited Mead's account in *Coming of Age in Samoa*, among others, as demonstrating that the *taupou* system had undergone major changes in the nineteenth- and early twentieth-centuries and that it no longer existed in practice in most of Samoa by the 1930s. In his 1948 thesis, Freeman added his own ethnographic description of the decline of the *taupou* system in the village of Sa'anapu, including a three-page discussion that confirms the observations of Mead and Keesing but goes beyond them in ethnographic detail.

Freeman began by stating that "[t]he *taupou* system has now become virtually defunct in Western Samoa" (1948, 245). He then reported on the factors responsible for its decline:

Principal among the reasons for this change has been the rigorous suppression of customs associated with it by the Christian missions. Economic factors have also operated. Like a *matai* [chief], a *taupou* is obliged to have her title ratified by the other lineages of her village community. This is established at a feast (*saofa'iga*) provided by the *taupou's* lineage. Such a feast is a serious drain on a lineage's resources. Again, following the introduction of money into the Samoan economy, marked discrepancies have developed in the value of the property (*oloa* and *toga*) exchanged at marriage ceremonies. This has resulted in a situation in which a *taupou's* lineage and village gain nothing from her marriage or formal election. (Freeman 1948, 245)

As a result, Freeman found that of the five *taupou* titles in the village of Sa'anapu, none were occupied in 1943. That is, none of the chiefs who could have appointed a *taupou* chose to do so. As for *taupou* marriages, they had become so infrequent that, as Freeman noted, "this type of marriage, now relatively rare, does not here concern us" (1948, 108).

When I published an article about the decline of the taupou system, citing Keesing and Mead (Shankman 1996), Freeman dismissed my argument as “all made of fantasy” (1998). At that time, I was unaware that Freeman’s unpublished thesis provided excellent ethnographic support for earlier arguments made by Mead and Keesing, and, later, by me (see Shankman 2006). Keesing’s article was also used by Raymond Firth, Freeman’s thesis adviser, who devoted two pages of his textbook *Human Types* (Firth 1958) to the decline of the *taupou* system. And Keesing had, of course, discussed Mead’s work. These intersections of knowledge about the decline of the *taupou* system make it all the more difficult to understand why Freeman’s thesis did not include Mead’s account in support of his own argument.³ And they make it more likely that Hemptenstall’s assessment of Freeman’s lack of knowledge of *Social Organization of Manu’a* is correct.

Conclusion

Recent research by Judith Heimann (1999), James Côté (2005), and Hiram Caton (2002) has explored the immediate context, commencing about 1960, in which Freeman became more critical of Mead. Freeman, too, believed that this period was a turning point in his work on Mead (Heimans 2001, 4). Up to this point in his career, Freeman’s record of publication suggests a pattern of avoiding Mead’s work rather than addressing it and of avoiding her personally despite his privately expressed criticism of her research. Freeman did not begin his correspondence with Mead until 1957, and this was not initially about Samoa. Moreover, it was not until 1968 that Freeman wrote up and circulated his critique of Mead in an unpublished manuscript titled “On Believing Six Impossible Things before Breakfast.” This manuscript became the basis of *Margaret Mead and Samoa*. However, Freeman did not send the essay to her despite his public confrontation with her over Samoa at a seminar in Canberra in November 1964. Again, Freeman chose to avoid Mead rather than providing her with a manuscript that he was circulating to other colleagues.

Freeman’s first published criticism of Mead’s work appeared in 1972 in an obscure note to the *Journal of the Polynesian Society* about apparent linguistic errors in *Social Organization of Manu’a*. The implication of this errata was that if Mead could not use the Samoan language properly, then how reliable could the monograph itself be? But Freeman did not directly criticize the monograph or *Coming of Age in Samoa*. It was not until 1983 that his full-blown critique of her Samoan work appeared in book

form, almost forty years after the date that he says he first realized his responsibility for such a critique.

There are a number of missing pieces in this story. The narrative that Freeman presented in *Margaret Mead and Samoa* about his early work on Mead now appears too neat and partial. It is a heroic narrative in which in-depth ethnographic research overcomes blind faith and in which determination and intellectual honesty triumph over shoddy scholarship. Yet, in attempting to undo the “myth” that Mead had allegedly created about Samoa, Freeman seems to have created misconceptions about his own biography and destiny.

While Freeman may have learned about cultural determinism from Beaglehole in college, as a result of his conversations with Krishnamurti in 1939, he became skeptical of it before going to Samoa. Freeman was also uncertain about whether he read Mead before going to the islands, although he did read *Coming of Age in Samoa* once he was there. He said that he had been encouraged to do ethnographic research in Samoa but did not mention that he was relatively untrained in anthropology and without an undergraduate degree as well as being unfamiliar with the islands. Nor did Freeman note that his first research was archaeological rather than ethnographic in nature.

Freeman’s conclusions about Samoan sexual conduct were only partially based on research in the village of Sa’anapu in the 1940s, and Freeman neglected the interethnic unions taking place in the islands during World War II while he was there. Moreover, when Freeman had the opportunity to critique Mead’s work as a graduate student during the late 1940s, he did not cite or discuss her work, to the detriment of his postgraduate diploma thesis. His professional critique of Mead would not emerge until decades later. So Freeman’s path to his critique of Mead was more circuitous and indirect than his published narrative suggests.

The narrative Freeman created for himself contrasts with the one he created for Mead, whom he depicted as a young, naive, and gullible field-worker. While Mead was young, Freeman was more of a novice and much less of an anthropologist than Mead when he first went to Samoa (see Tiffany 2009). Further, on the subject of Samoan sexual conduct, he seems to have been vulnerable to the very weaknesses that he attributed to Mead. Freeman would become a careful observer of Samoa and an excellent scholar with a superior understanding of Samoan culture on many matters, but this mostly seems to have occurred separately from rather than in response to Mead’s work.

If Freeman felt a professional responsibility in 1943 to refute Mead’s work, why did he not do so until decades later? Indeed, why did he actively

avoid doing so for a considerable period of time? From this review of Freeman's early encounters with Mead's work, two possibilities emerge. Either Freeman knew of Mead's research and publications and, for reasons that remain unclear, chose not to recognize or critique them in written form early in his career, or he did not know very much about Mead's work, especially *Social Organization of Manu'a*, and therefore could not use it either to support his own work or to critique hers. Neither of these scenarios is in accord with Freeman's published version of the professional decisions he made early in his career regarding Mead's work. Future biographers of Freeman with greater access to his early writings may be in a better position to determine which of these two scenarios is more plausible or, for that matter, whether either is. It is sufficient for now to view Freeman's narrative as less a definitive account of his early relationship to Mead's work and more as a stimulus to further research.

NOTES

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1. This is a relatively minor point, but it does indicate that there were personal and professional schisms in the so-called Boasian school and that there was less ideological conformity among Boas's students than Freeman had imagined (see Murray and Darnell 1998).
2. The philosopher had a powerful influence on Freeman; later in life, after more fully studying Asian religions, Freeman became, in his own words, "an evolutionary Buddhist" (see Freeman 2001).
3. It is also difficult to understand why, after including Keesing in his thesis, Freeman omitted this key source in his book *Margaret Mead and Samoa*, published in 1983.

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