

The Mead–Freeman Controversy Continues: A Reply to Ian Jarvie

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Abstract

In the Mead–Freeman controversy, Ian Jarvie has supported much of Derek Freeman’s critique of Margaret Mead’s *Coming of Age in Samoa*, arguing that Samoan society was sexually repressive rather than sexually permissive, that Mead was “hoaxed” about Samoan sexual conduct, that Mead was an “absolute” cultural determinist, that Samoa was a definitive case refuting Mead’s “absolute” cultural determinism, that Mead’s book changed the direction of cultural anthropology, and that Freeman’s personal conduct during the controversy was thoroughly professional. This article calls into question these empirical and theoretical arguments, often using Freeman’s own field research and publications.

Keywords

Mead, Freeman, culture, biology, anthropology

I. Introduction

I would like to thank Ian Jarvie (2012) for providing an opportunity to clarify some of the important issues in the Mead–Freeman controversy, now in its fourth decade. Jarvie’s article-length review of my book, *The Trashing of Margaret Mead: Anatomy of an Anthropological Controversy* (Shankman

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2009), raises issues that have been present throughout the controversy but deserve further explication. In this article, I will focus on these key issues, including Mead's evidence concerning Samoan sexual conduct, her theoretical perspective, her ethnographic competence, the politics of reputation that have been part of the controversy, and the extent to which the controversy was about larger issues such as the current state of anthropology.

The controversy began in 1983 with the publication of Derek Freeman's *Margaret Mead and Samoa: The Making and Unmaking of an Anthropological Myth*. Freeman made front-page headlines around the world with his critique of Mead's *Coming of Age in Samoa* (1928), arguing that she was wrong about Samoan adolescence, wrong about Samoan sexual conduct, and wrong about the role of culture in human life. In this book, Freeman focused primarily on what Mead got wrong about Samoa. In his second book, *The Fateful Hoaxing of Margaret Mead: A Historical Analysis* (Freeman 1999), he provided an explanation of why Mead got Samoa wrong. Freeman alleged that two Samoan women told Mead innocent lies about their private lives that she believed as the truth and published in *Coming of Age in Samoa*.

As the result of Freeman's critique, Mead's reputation has gone from respected public icon to cultural punching bag. A brief search on the Internet using the words Margaret Mead, hoax, fraud, liar, and scandal demonstrates how widespread and credible Freeman's critique has become.

Apart from the very public nature of the controversy, it has also been the longest and among the most academically charged controversies in the recent history of cultural anthropology. My book tells the broad story of the controversy—what it was about, how it became a media sensation, what Mead and Freeman brought to their research in Samoa, how *Coming of Age in Samoa* became a best-seller, what Mead's American audience of the 1920s read into her work, what the issues and evidence were—real and imagined, and what the controversy meant to Samoans, anthropology, and the public. That is, the book provides a context for understanding the controversy for a general audience as well as evaluating the specific arguments of Mead and Freeman.

Jarvie, a long-time observer of anthropology and admirer of Freeman's work,¹ regards much of my book as "quite unscientific," "ad hominem," full of "gossip," "speculation," and "padding," as well as "special pleading" on Mead's behalf, all in a vain attempt to tarnish Freeman's reputation (Jarvie 2012). He also finds the book to be an example of the alleged dire condition

¹For Jarvie's early publications on anthropology, see Jarvie (1964, 238-39). In *The Revolution in Anthropology*, Jarvie (1964, 171) noted that Freeman was "an anthropologist whose work I greatly admire . . ."

of American cultural anthropology and its implacable embrace of cultural determinism. Jarvie believes that most of the book is “irrelevant” to the “facts” of the controversy, stating, “What has always interested me in this controversy was the simple philosophical issue of a grand, beautiful theoretical scheme brought down by a recalcitrant ugly little fact . . .” (Jarvie 2012, 539). For Jarvie, that grand theoretical scheme is the belief in the “autonomy of culture” (Jarvie 2012, 540), that is, that “culture is destiny” (Jarvie 2012, 534), a view he attributes to Mead. And the “ugly little fact” is Freeman’s description of Samoa as a sexually repressive society that “strongly emphasized female chastity, which was tested by a male relative” (Jarvie 2001, 557) and in which adolescence was filled of “storm and stress.” Jarvie, like Freeman, is interested in the refutation of Mead’s work on Samoa and its implications for a broader critique of contemporary American cultural anthropology. How persuasive are their arguments? This article evaluates the arguments of Freeman and Jarvie, often using Freeman’s own field research and publications.

2. An “Ugly Little Fact”: Ceremonial Virginity

Coming of Age in Samoa (Mead 1928), written for the American public, characterized Samoa as a sexually permissive culture, where adolescent girls could have a number of lovers before marriage. Freeman (1983, 250) sharply criticized this characterization, countering that “the cult of female virginity is probably carried to a greater extreme than in any other culture known to anthropology . . . ,” because virginity was institutionalized in Samoa and a public defloration ceremony was performed at marriage to confirm the chastity of certain young women. How could Mead have so misunderstood this “ugly little fact”?

Some historical context may be helpful here. In pre-European Samoa, high-ranking chiefs had the exclusive right to appoint their daughters or other close female relatives as ceremonial virgins (*taupou*) whose arranged marriages to the other high-ranking chiefs or their sons cemented political alliances between groups. These ceremonial virgins were representatives of the village’s honor and a carefully guarded political asset. They would act as official hostesses for visiting parties from other villages as well as making kava on ceremonial occasions involving chiefs. *Taupou* wore special dress and had their own hairstyle. When they married, there was a public defloration ceremony to demonstrate their virginity. Both Freeman and Mead concur with this description of the *taupou* system in pre-European Samoa.

For Freeman, the *taupou* represented the importance of virginity of all unmarried Samoan women. For Mead, the *taupou* mostly represented the

importance of virginity for daughters of high rank, a minority of the young women. For men and women of lower rank, there was a different system of marriage. While the daughters of high-ranking chiefs had elaborate arranged marriages, lower ranking young men and women privately eloped. Elopement (or *avaga*) was a second, publicly recognized type of marriage.

Mead stated that *taupou* were less important when she did fieldwork in American Samoa in the 1920s because the *taupou* system was in decline and the public defloration ceremony had been banned. Freeman responded that the *taupou* and *taupou* system were central Samoan institutions before, during, and after Mead's fieldwork (Freeman 1983, 350). Yet the *taupou* system in American Samoa, where Mead worked, and in Western Samoa, where Freeman worked, was in sharp decline in the early 20th century. In his unpublished work based on research in the 1940s, Freeman himself described the historical decline of the *taupou* system during the colonial era, just as Mead and other anthropologists had previously reported.² His unpublished postgraduate diploma thesis (Freeman 1948) reported that in the village he studied, there were no *taupou* appointed by chiefs, although there were five vacant *taupou* titles at the time. There were also no *taupou* marriages. Because they were so rare, Freeman (1948, 108) stated that "they do not here concern us." In fact, Freeman stated that the *taupou* system was, in his words, "virtually defunct" throughout Western Samoa (Freeman 1948, 245), and he provided an explanation of why this was so (Freeman 1948, 245). In other words, Freeman knew from his own fieldwork that Mead had accurately portrayed the decline of the *taupou* system at the time of her research.

What of Freeman's (1983, 250) assertion that "the cult of female virginity is probably carried to a greater extreme than in any other culture known to anthropology . . ."? Here Freeman provided valuable data about the importance of virginity in Samoan public morality and belief, a significant contribution to the ethnography of Samoa and an aspect of Samoan culture that Mead did not sufficiently discuss. However, Freeman himself acknowledged that the ideal of virginity applied primarily to girls of high rank, and his own data on virginity, like Mead's, show that the older the adolescent girl, the more likely she was to engage in sexual activity. Furthermore, young men were encouraged to engage in premarital sex and praised for their success in this endeavor. So, at the same time young men were expected to guard their own sisters' virginity, they were also expected to pursue other young men's

²The decline of the *taupou* system was well documented (Keesing 1937) and well known, appearing in an introductory text in social anthropology authored by Raymond Firth (1958, 153-54), one of Freeman's academic advisors at the London School of Economics and an authority on Polynesian culture.

sisters. Thus, Samoan behavior did not necessarily conform to the sincerely articulated public belief about virginity. Schlegel's (1991) comparative study of the value on virginity found that Samoa fell somewhere in the middle between the most restrictive and most permissive societies.

Freeman's assertion about the high value Samoans placed on virginity is also called into question by Samoan sexual conduct during Freeman's fieldwork in the 1940s. In Samoa during World War II, many unmarried young women became involved with American servicemen stationed there without serious conflicts between Samoans and Americans. Indeed, many Samoan parents permitted, encouraged, and condoned these unions, and a number of children resulted from them who were subsequently accepted in Samoan society. A more restrictive culture would have secluded daughters, prohibited interethnic unions, and severely sanctioned such unions and their offspring. Since Freeman resided in the islands during the war as a school teacher, an amateur anthropologist, and as a member of the local defense force, he must have known of these frequent relationships (Shankman 1996, 2009). Yet he said nothing about them in his published work.

Freeman knew a great deal about Samoan history and culture. However, his neglect of readily available evidence from his own fieldwork undermines his credibility as a reliable narrator in the controversy. When I published an article (1996) reporting the decline of the *taupou* system and Samoan sexual conduct during World War II Freeman (1998) responded in print that my argument was "all made of fantasy" and that I did not know what I was talking about. Yet the evidence from his own research and personal experience during the 1940s easily refuted his response (Shankman 2006).

Does this mean that Mead's depiction of Samoan sexual conduct was flawless? Of course not. While Samoan sexual conduct was less restrictive than Freeman allowed, it was also more restrictive than Mead depicted. Although Mead discussed limitations on young women's sexual conduct, her evocative descriptions of clandestine affairs left the impression among readers that Samoan adolescents embraced "free love." Mead did not use these words in *Coming of Age in Samoa* (Orans 1999, 74-75), but critics often cite them as if she had. Mead's book does contain errors of fact and interpretation, as critics have noted since its publication. However, this does not mean that Mead was "fundamentally in error" (Freeman 1983, xii). Nor do Mead's errors excuse those in Freeman's critique of her work.

3. Single Cases and Comparative Data

If the "facts" about Samoan culture are more problematic than Jarvie believed, what about the importance of the Samoan case for the idea of cultural determinism? Both Freeman and Jarvie view Samoa as a pivotal case, undermining

the entire theoretical edifice of contemporary cultural anthropology and opening the door to biological explanations. Arguing that Mead's description of Samoan adolescence was "erroneous" and no "negative instance" at all (Freeman 1983, xii), Freeman (1983, 68) concluded that the importance she gave to culture in explaining the correlates of adolescence must also be erroneous. Jarvie (2001, 558) concurs, stating,

Freeman viewed Mead's error as important since it functioned as a crucial test of the underlying Boasian view that the form of adolescence is a cultural particular rather than a universal (or biological) developmental phase.

He further argues,

A rational reception of Freeman 1983 book would have required of American anthropologists that they call into question not just the truth of the researches of Margaret Mead and the ideas of her teachers but the entire cultural and nurturist identity. (Jarvie 2001, 558-59).

Both Freeman and Jarvie believe that Mead's Samoa put the "grand theory" of cultural determinism on trial.

This kind of argument conflates the single case with a broader argument, employing what has been called the "anecdotal veto" (Wallace 1968, 42). That is, if Mead was wrong about Samoa, cultural determinism must also be wrong.³ However, whether Mead was right or wrong about Samoa, or for that

³Jarvie states that "Shankman concedes that Mead's *Coming of Age in Samoa* was factually in error but argues that her reputation in anthropology did not rest on it but rather on her extensive works on other societies" (Jarvie 2012, 531). He suggests that I was trying to excuse Mead's alleged error in *Coming of Age in Samoa* by deflecting attention away from her book. However, Jarvie actually agrees that Mead's reputation in anthropology did not rest primarily on her first book, commenting, "Her reputation in anthropology rested not on *Coming of Age in Samoa* but on a number of more scholarly and technical monographs including her Samoan ethnology *Social Organization of Manu'a* (1930)" (Jarvie 2012, 533). More accurately, Mead's reputation in anthropology was mixed and rested on her popular and professional work on Samoa and several other cultures as well as her many diverse contributions to anthropology that were the subject of an entire issue of the *American Anthropologist* (1980). Jarvie also states that I thought that *Coming of Age in Samoa* was ostensibly about Samoa but, in fact, was "rather a critique of American society and should be judged as such" (Jarvie 2012, 531). This statement is misleading. *Coming of Age in Samoa* used ethnographic data for social commentary and criticism about adolescence in America. It was about adolescence in Samoa and America rather than

matter whether Freeman was right or wrong, the broader theoretical issue of the extent to which patterned human behavior in adolescence is the result of culture or biology is not dependent on the Samoan case or any other single case. If Mead and Freeman had never studied Samoa, cultural and biological explanations would still vie for theoretical supremacy. The single case is one point of entry into a theoretical debate rather than a final destination.

In the 1920s, Mead herself used the “negative instance” argument to call into question psychologist G. Stanley Hall’s ideas about the universal nature of adolescence. At that time, there were little cross-cultural data available on adolescence, so Mead’s simple “compare and contrast” method using Samoa and America was understandable. Mead observed that American adolescents were experiencing problems that she attributed to the many new choices young people, especially girls, faced; for American girls, this was a time of “storm and stress.” Mead argued, in contrast, that Samoan adolescent girls experienced less “storm and stress” than their American counterparts because the choices they faced were mostly the same choices their mothers faced, and the problems that arose from these choices were dealt with in familiar ways. Mead documented conflict, stress, and jealousy among the girls she studied; indeed, there is a whole chapter on “The Girl in Conflict.” So there was adolescent problem behavior, and the lives of the Samoan girls were far from perfect models of cooperation and harmony (Freeman 1983; Orans 1999). Nevertheless, Mead (1928, 37) argued that their lives were less stressful than those of adolescent girls in precolonial Samoa, less stressful than those of Samoan boys, and less stressful compared with American adolescent girls in the 1920s. Therefore, she argued that adolescence was not necessarily a uniformly or inevitably difficult period for young people.

Freeman countered Mead’s largely positive portrayal of the choices Samoan girls faced in their life cycle with evidence, sometimes from more recent decades and often involving males, demonstrating that Samoan adolescence was characterized by high rates of delinquency, conflict, rape, and aggression in comparison with adolescents in the United States, England, and

being a standard ethnography of Samoa. Mead conveyed her intention for the book in its subtitle: “A Psychological Study of Primitive Youth for Western Civilization.” In writing the book, both Mead and her publisher, William Morrow, anticipated a popular audience including educators, teachers, and parents (Shankman 2009, 104). As Morrow told Mead, her ethnographic report on Samoan adolescent girls written for the National Research Council would not have commercial appeal, so she revised the report and added chapters for a broad American public (Shankman 2009, 104). It was the first anthropological book of its kind and became a best-seller precisely because it contained lessons for Americans.

Australia. Using these high rates, Freeman made the argument that there was considerable “storm and stress” among Samoan youth. Moreover, he contended that biology was a significant factor in adolescence because delinquency and crimes of violence increased dramatically during this period in the life course not only in Samoa but in other cultures as well.

Freeman’s data on rates of adolescent delinquency, rape, and crimes of violence, and so on, not only depict storm and stress in Samoa; they sometimes depict more storm and stress than in other cultures. Although there have been critiques of Freeman’s use of statistical data (see Côté 1994, 43–47; Kuper 1991, 43–47), assuming that his data are valid, they demonstrate the *variability* of the outcomes of adolescence across cultures rather than their uniformity—the very point that Mead was making. That is, the comparative data Freeman provides do not support a strictly biological explanation of this variation.

A specific example of this problem involves Freeman’s (1983, 249) argument about rape in Samoa. Freeman found that rates of rape in Samoa were “among the highest in the world.” The relevant biological variables involved, according to Freeman, were the age of the rapists and the hormonal states of the individual perpetrators. Yet he offered no biological data beyond the gender and age of the perpetrators to support this hypothesis.

These data, by themselves, do not explain variable rates of rape cross-culturally (Otterbein 1979). Moreover, Freeman offered more detailed information and a cultural explanation of rape in the islands based on Samoan beliefs and practices. He reported that young Samoan men *learned* to rape from their peers through “*culturally* transmitted male practices” that are “part of Samoan *culture*,” and have “long been intrinsic to the sexual *mores* of Samoan men and are major elements in their sexual behavior” (Freeman 1983, 249–50, emphasis added). So, although initially favoring biological factors in his explanation of rape in Samoa, Freeman’s preferred explanation was cultural.

At a broader comparative level, cross-cultural research that demonstrates considerable variability in the correlates of adolescence (Cohen 1964; Lancy 2008). In their sample of over 170 societies, Schlegel and Barry (1991) found that in a large majority of cases, adolescence was a period of increased responsibility, problematic choices, and, for boys, a period of training for war. In a minority of cases, adolescence was a period of relative ease. There were societies that harshly punished adolescent problem behavior and societies that were more lenient. There were societies that were sexually restrictive and societies that were quite permissive. And there were societies without any substantial period of adolescence due to child marriage. Schlegel and Barry III argue that, although biology set very broad potentials and limitations for all

adolescents, it did not explain these variable outcomes.⁴ This is true of other phases in the human life cycle such as childhood and old age. Cross-cultural evidence helps place single cases in a broader context.⁵

4. Biology and Culture in Mead's Work

For Freeman (1983, xvi, 295), Mead was not simply a cultural determinist; she was an "absolute" cultural determinist who ignored biology altogether. Yet Mead addressed biology directly in *Coming of Age in Samoa*. As Freeman himself acknowledged, Mead "fully recognized" that puberty was a universal biological process (Freeman 1983, 254); it was her starting point for a discussion of the transitional period between childhood and adulthood. Mead appreciated the importance of biology in the process of coming of age, but noted that differences in adolescent behavior among societies could not be attributed to biological similarities common to adolescents in all societies.

Mead did not take a "black or white," "either/or" position on the nature/nurture issue. She agreed with her mentors, Franz Boas and Ruth Benedict, that biology was not destiny, but this did not mean that they thought that biology was irrelevant and that *only* culture mattered. Mead was thus not an "absolute" cultural determinist; she was a cultural determinist who was interested in the relative weights of biology and culture in explaining adolescent

⁴Adolescent problem behavior within a culture can also change markedly over a period of years, and this has been the case in the islands. Thus, in American Samoa, anthropologist Lowell Holmes (1974, 102) found that decreasing social control in the Pago Pago urban area in the 1960s and 1970s led to increased problem behavior among Samoan youth. In Western Samoa in the 1970s and 1980s, adolescent suicide rates increased dramatically to among the highest rates in the world (Macpherson and Macpherson 1987). For the United States, Modell's (1989) comprehensive study of adolescence from 1920 to 1975 examines changes in adolescent behavior in response to the Great Depression, World War II, and postwar prosperity. Biology alone cannot explain these changes.

⁵If the single case is no longer as persuasive as it once was, the way in which Freeman used "biology" as an explanation tells us very little. To say that there is a biological explanation for the correlates of adolescence does not specify which set of variables might, for example, explain *why* adolescence may be more stressful in some societies and less so in other societies. Biology is a master concept that is useful in suggesting where to look for possible explanations, but in and of itself is not explanatory. The same logic also applies to cultural explanations (Jarvie 2000, 542). Culture is also a master concept, like class, society, and community. Without specifying causal variables, so-called "biological" and "cultural" explanations remain far too general and vague to be very useful.

behavior. In *Coming of Age in Samoa*, she found culture to be determinative in this case.

Like Freeman, Jarvie contends that Mead was committed to an extreme version of cultural determinism and faults her for advocating “the autonomy of culture” (Jarvie 2012, 540). Yet Freeman used these very words in his own discussion of the concept of culture. In addressing the malleability of culture, Freeman noted that people attribute different meanings to the same genetically based behavior. Thus, in Samoa, the genetically based eyebrow flash means “yes” while in Greece it means “no.” According to Freeman (1980, 215, emphasis added), “It is the existence of such conventional behaviors, in great profusion, in all human populations, that establishes, indubitably, *the autonomy of culture*.”

Freeman (1980, 209) further stated that “human history reveals boundless diversity and often extreme variability of action” that “cannot possibly be explained by changes in gene frequencies.” In addition,

Because cultural phenomena are particular alternatives, created by human agency in the course of history, it is always possible for these alternatives to be rapidly, even radically changed . . . [T]he choice of new alternatives is, in many instances, not connected in any significant way with the process of genetic evolution, or, for that matter, with human physiology. (Freeman 1980, 215)

Does this make Freeman an “absolute” cultural determinist? No more or less than Mead. Freeman’s view of the relationship of culture and biology was essentially the same as Mead’s.

Mead and Freeman were both interested in the *interaction* of biology and culture. Although Freeman and Jarvie have argued that American anthropologists neglected biology due to Mead’s influence, for a number of decades, American anthropologists have been researching the interaction of biology and culture, including research in Samoa on adolescent stress (McDade 2002), and on the biological consequences of Samoan migration abroad (Baker, Hanna, and Baker 1989; Janes 1990). Indeed, biocultural research is a growing subfield of anthropology (Zuckerman and Martin 2016). However, as Freeman noted, biocultural interaction does not mean that the mechanisms of biological adaptation are identical to those of cultural adaptation. As he stated, cultural adaptations rely on social learning and choice rather than genetic control. Thus, the migration of Samoans overseas in recent decades has not been dependent on natural selection and differential reproductive success, although it has had biological consequences such as elevated levels of hypertension, diabetes, and obesity. These differences between cultural and biological adaptations are why Freeman (1980) was an early critic of socio-biology before becoming interested in evolutionary psychology.

Mead's view of the relationship between biology and culture, like Freeman's, developed over decades and changed in relation to political and cultural circumstances. Publishing in the 1920s, 1930s, and early 1940s, Mead wrote about culture against the backdrop of racial and eugenic theories that were popular during that era. For many thinkers of that period, biology and "race" were kindred concepts. Thus, Mead, like Boas and Benedict, used culture as an alternative to racial and hereditarian explanations of human behavior.

After World War II, and especially in *Male and Female* (1949), Mead paid more attention to biology, emphasizing universal biological differences between the sexes that she stated could not be ignored. Over the decades, she explained,

These two approaches to man—one of which sees man as a creature with species-characteristic instinctual patterns that play a continuing part in the forms that civilizations take, and the other which views man as lacking species-characteristic behavior patterns and as capable of being conditioned to almost any kind of system that takes into account survival needs—cross and recross each other. The optimism of the Watsonian position in the 1920s has been tempered by the experience of the following three decades, during which "techniques" of conditioning were used in the service of absolute or irresponsible power. (Mead 1964, 10-11)

During the first three decades of her career, Mead was one of the important scholars who helped move Americans away from thinking about similarities and differences across societies in terms of race and toward thinking about them in terms of culture; this was anthropology's major intellectual contribution to 20th-century thought (Degler 1991). In the following decades, culture became less significant for Mead. For some problems that she explored, such as adolescence, culture was more important; for others, such as gender differences, it was less so. Ironically, Mead's interest in both nature and nurture led to her being labeled an "absolute" cultural determinist on the one hand and a biological determinist on the other (Friedan 1963, 117-41).

5. The Alleged "Hoaxing" of Margaret Mead

Beyond the factual and theoretical issues in the controversy, there is the question of Mead's competence as a fieldworker, a point raised earlier in the discussion of ceremonial virginity. Freeman asserted that Mead's permissive view of Samoan sexual conduct was the result of a "prank" or "hoax" by two Samoan women who joked with Mead about what they did at night, not realizing that she sincerely believed what they said as the truth and published as fact in *Coming of Age in Samoa*.

According to Freeman, one of the two women, 86 year-old Fa'apua'a Fa'amu, confirmed the hoaxing in a filmed interview in 1987 (Heimans 1988), over 60 years after Mead's fieldwork. For Freeman, this interview was indisputable evidence of how Mead came to her allegedly false conclusion about Samoan sexual permissiveness. Fa'apua'a's credentials seemed impeccable. She was a Samoan of high rank, a woman, allegedly Mead's "principal informant," and "closest" Samoan friend, as well as the only living witness to her Samoan research, testifying under oath that not only was Mead mistaken about Samoan adolescent sexual conduct but that she and her friend Fofoa were the source of Mead's error.⁶

Fa'apua'a's testimony was the kind of evidence that Freeman felt could be presented "in a court of law" and was the "only" way that Mead's alleged error about the sexual permissiveness of Samoan adolescent girls could be explained. According to Freeman (1999), Mead was not simply the victim of a Samoan prank, but also the victim of her own limited experience in Samoa, her lack of knowledge about Samoan custom, and her prior beliefs about the role of culture in human behavior. He also found that throughout her life, Mead was "cognitively deluded," never realizing that she was hoaxed. Thus, Freeman (1999, 14) contended that Mead was "fatefully hoaxed," indeed "grossly" and "completely" hoaxed—an outcome that was, in Freeman's words, "virtually inevitable." Stated with great authority and seemingly incontrovertible evidence, it is not surprising that a number of very intelligent people have supported Freeman's hoaxing argument including Steven Pinker, David Buss, Matt Ridley, and Robert Sapolsky.

There are a number of problems with Freeman's hoaxing argument (Shankman 2009, 193-205; 2013), the most important being that, according to Fa'apua'a's *complete* testimony, Mead did not ask her about her own sexual conduct or the conduct of Samoan adolescent girls. Nor did she independently provide Mead with information about her own conduct or the conduct of adolescent girls. Three separate interviews with Fa'apua'a were conducted in 1987, 1988, and 1993, resulting in 140 handwritten pages of testimony. From this material, Freeman selectively quoted one passage from the first interview that *seemed* to verify the hoaxing. However, the remainder of that interview and the other interviews, which Freeman does

⁶The claim that Fa'apua'a was Mead's "primary informant" and "closest" personal friend was made by Freeman and Fa'apua'a rather than by Mead. There is no supporting evidence for this claim in Mead's published work or in her field notes (Shankman 2009, 199).

not quote or paraphrase, make it clear that Mead did not ask Fa'apua'a and Fofoa about their own sexual activities nor did they provide information to her about adolescent sexual conduct. Had Freeman simply published the entire first interview in translation, the hoaxing argument would have never gained credibility.

The first interview was conducted by Fofoa's son, a high-ranking chief and official in the government of American Samoa. He had read Freeman's first book on Mead, which Freeman provided, and thought that Mead had portrayed Samoan women, including his own mother, Fofoa, as "sluts." He conveyed this impression to Fa'apua'a before the interview. Although this impression was incorrect, it raised the question of whether Mead's Samoan informants may have somehow been responsible for this insulting and demeaning characterization.

It was in this context that the first interview began with a leading question: "Was there a day, a night, or an evening when Mead asked what Fa'apua'a and Fofoa did at night, and did they ever *joke* about it?" (Freeman 1999, 3, emphasis added). To this question, Fa'apua'a replied,

Yes, we did; we said that we were out at night with boys; she failed to realize that we were just joking and must have been taken in by our pretenses. Yes, she asked: "Where do you go?" And we replied, "We go out at nights!" "With whom?" she asked. Then your mother, Fofoa, and I would pinch each other and say, "We spend the nights with boys, yes, with boys!" She must have taken it seriously but I was only joking. As you know, we Samoan girls are terrific liars when it comes to joking . . . (Freeman 1999, 3)

Freeman, who was present at this interview, interpreted this passage to mean that Mead had asked the two women explicit questions about their own sexual conduct and that the phrase, "We spend the night with boys," referred to sex. Hence, Fa'apua'a's answer was *prima facie* evidence of how Mead's was hoaxed. However, in the very next set of questions, the interviewer probed more deeply to clarify what the phrase, "We spend the nights with boys," actually meant.⁷ Was Fa'apua'a referring to sex with boys?

Question: Did Margaret Mead ask you both, my apologies . . . , whether you had sex with boys at night?

Answer: Absolutely not. (Shankman 2013, 57)

⁷For a complete analysis of the three interviews, including all of the questions and answers in the first interview, see Shankman (2013).

What about information that the two women may have provided Mead on adolescent sexual activity? Freeman carefully worded such a question, again misrepresenting Mead's research, for the second interview with Fa'apua'a.

Question: In her writings about Samoa, Makerita (i.e. Margaret Mead) stated that a Samoan girl (i.e. in Ta'u in 1926), before she married, would make love with (i.e. copulate with) many different young men, staying out all night with each of them in turn before returning to her home at dawn. Did [you] say anything like this to Makerita?

Answer: No. (Shankman 2013, 59)

Thus, Fa'apua'a denied providing Mead with information about adolescent sex. Freeman in his several publications on the alleged hoaxing simply omitted these much more specific questions and Fa'apua'a's unequivocal answers.

Given these omissions of crucial testimony, how was Freeman able to infer that Mead was hoaxed in response to questions that Fa'apua'a said Mead never asked them and by answers about adolescent sexual activity that, according to Fa'apua'a, she never provided to Mead? Fa'apua'a's complete testimony offers no support for the hoaxing argument. Furthermore, an examination of *Coming of Age in Samoa* and Mead's field notes found no information about sex attributable to Fa'apua'a or Fofoa. Nor were there any references to these two women and sex in Mead's letters to colleagues and friends.

The information on adolescent sex that Mead gathered did not come from these two women but mostly from 25 adolescent girls with whom she worked. Why would Mead rely on two women in their mid-twenties when she had detailed information from adolescent girls themselves? Moreover, Mead's publications on Samoa and her field notes show that she understood joking as part of everyday Samoan conversation. Freeman's portrayal of Mead as a gullible fieldworker again neglects available evidence to the contrary.

6. Jarvie's Version of the Hoaxing Argument

Jarvie believes that my critique of Freeman's hoaxing argument is a "skeptical" exercise—a just-so story—rather than a careful refutation using Freeman's own terms of argument and the interview evidence that he provided (Jarvie 2012, 538). Jarvie writes that skeptical arguments are "valid," but use of them "smacks of desperation" (Jarvie 2012, 538). Yet Jarvie offered his own hoaxing argument in the documentary film, "Margaret Mead and Samoa" produced by Frank Heimans (1988) in collaboration with Freeman.

In the film, Jarvie noted the circumstances of Mead's fieldwork in American Samoa and then asked viewers to imagine the awkward position of Samoan girls being questioned about sex by Margaret Mead. Jarvie stated that Mead's interviews with the girls took place in an American colony in which Christian missionaries, after the abolition of headhunting, imposed a strict puritanical morality on Samoans. Samoan girls would have seen Mead "hobnobbing" with missionaries and government officials at the same time she was asking them very personal questions about their private lives. In this context, Jarvie believed that Mead was setting herself up to be fooled, because the girls would tell her what they thought she wanted to hear.

There are some factual errors in Jarvie's argument. Samoans were never headhunters. Nor did Mead hobnob with missionaries in Manu'a, the islands where she did most of her fieldwork. These islands had been missionized decades earlier; Samoan pastors were now in place, and Mead did not associate with them. These errors aside, in its general argument, Jarvie's version of the hoaxing of Mead is similar to Freeman's, hypothesizing that Mead was hoaxed by her failure to understand what her informants were really thinking. Yet if Mead was so naïve as to not comprehend the circumstances of her fieldwork, why is it that Jarvie finds that Mead was a "formidable fieldworker" (Jarvie 2012, 540)? And why is it that he argues that her alleged "error" on this matter requires "no explanation"? (Jarvie 2012, 540). If her "error" requires "no explanation," why did he offer one?⁸ And, if no explanation was required, why did Freeman devote an entire book to the alleged hoaxing as well as several of his other publications? The answer to these questions may lie in the larger significance that Freeman and Jarvie assign to the hoaxing for the so-called "paradigm" of "absolute" cultural determinism.

7. The Significance of the Hoaxing Argument and the State of Anthropology Today

Freeman (1997, 68) viewed the importance of the hoaxing of Mead in the following manner:

We are here dealing with one of the most spectacular events of the 20th century. Margaret Mead, as we know, was grossly hoaxed by her Samoan informants, and Mead in her turn, by convincing others of the "genuineness" of her account

⁸Jarvie criticizes Freeman's efforts to explain Mead's errors as "inductivist," arguing that her errors require no explanation (Jarvie 2012, 539-40). By the same token, Jarvie's version of the hoaxing argument is also inductivist.

of Samoa, completely misinformed and misled virtually the entire anthropological establishment, as well as the intelligentsia at large . . . That a Polynesian prank should have produced such a result is deeply comic. But behind the comedy there is a chastening reality. It is now apparent that for decade after decade in countless textbooks, and in university and college lecture rooms throughout the Western world, students were misinformed about an issue of fundamental importance by professors placing credence in Mead's conclusion of 1928 who had themselves become cognitively deluded. Never can giggly fibs have had such far-reaching consequences in the groves of academe.

Freeman (1999, 27) further emphasized that "no sequence of events has had a greater effect on anthropology in the twentieth century." Jarvie concurs with Freeman that *Coming of Age in Samoa* had a profound effect on American anthropology, finding that "it had decisively pointed American anthropology down the path of culture and away from the path of biology" (Jarvie 2001, 558). Both Jarvie and Freeman believe that Mead's book led American anthropologists to embrace "absolute" cultural determinism, dating from the 1920s to its present-day "entire cultural and nurturist identity" (Jarvie 2001, 558). If true, this is a remarkable story. But exactly how did a young woman, who had just returned from her first field trip, write a best-selling book for a broad public audience and, at the same time, persuade "virtually the entire anthropological establishment," as well as the intelligentsia, to follow this mistaken paradigm for several decades and manage to accomplish all of this without holding a full-time university position?

Like the hoaxing argument, this was a good story. It was a story that many people wanted to believe. But it was a story too good to be true, as Jarvie himself seems to recognize. Mead did not mislead the anthropological establishment, nor was her work the foundation of its current theoretical orientation (Murray and Darnell 2000). Jarvie notes, "Despite her high public profile, she [Mead] was a middle rank anthropologist, a normal scientist rather than an influential innovator" (Jarvie 2012, 533). And, summarizing my account, he recognizes that "her weight within anthropology was modest" (Jarvie 2012, 536; 2017). In fact, *Coming of Age in Samoa* received mixed reviews from anthropologists after its publication, and most anthropologists, as Jarvie acknowledges, did not and do not view it as an "anthropological classic" (Jarvie 2012, 535). "Anthropologists distanced themselves from it, seldom using it in courses. Of course, the public ate it up, and so it gained broad significance beyond academic anthropology" (Jarvie 2012, 533).⁹ The

⁹*Coming of Age in Samoa* was assigned in a number of anthropology courses before the controversy began, sometimes as a way of encouraging student interest in anthropology and occasionally as a way of encouraging skepticism about Mead's work.

book's very popularity with a large American readership was itself cause for skepticism among anthropologists.

Current histories of cultural anthropology may briefly mention *Coming of Age in Samoa* as a popular and pioneering work but not one that had a transformative influence on the discipline, let alone an influence that defined an entire disciplinary agenda. As historians of anthropology Stephen O. Murray and Regna Darnell (2000, 570) candidly observed,

Rather than being admired as single-handedly vanquishing biology and establishing cultural anthropology, Mead was widely regarded with contempt as an overheated romancier and popularizer by American anthropologists during the 1950s . . . she was not taken seriously as a theorist.

If Mead was not an influential theorist, faulting her for what Freeman and Jarvie see as the current state of cultural anthropology misrepresents the history of anthropology and Mead's place in it.

Nevertheless, Freeman and Jarvie register their strong disapproval of contemporary American cultural anthropology in its "postmodernist and culturalist" incarnations. Indeed, Jarvie views my book as embodying many of these same "faults," stating,

American cultural anthropology valorizes culture as the governor of most human life while tipping its hat to evolution. It mixes disciplines in an undisciplined way: anthropology, sociology, psychology, psychopathology, political literary criticism, you name it, are larded together as though they were supplementary rather than mutually contradictory, as they more often are. It displays a patina of rationality and scholarship while pursuing other agendas that have more to do with internecine disputes than with anthropology. It is self-righteous and judgmental. It is methodologically chaotic and oblivious to the devastating critique to which the whole enterprise of cultural anthropology has been subjected. (Jarvie 2000; 2012, 538; Kuper 1999)

In addition to this list of faults, Jarvie (2000, 543) criticizes American cultural anthropologists for employing an allegedly antiscientific approach to culture that marginalizes behavior, excludes other possible variables like biology, and reifies "the autonomy of culture" (Jarvie 2012, 540). Jarvie is correct that in recent decades, many cultural anthropologists have employed nonscientific and antiscientific approaches such as interpretation, deconstruction, postmodernism, poststructuralism, and the ontological turn. However, the move away from science in cultural anthropology that Freeman and Jarvie refer to was not initiated or encouraged by Mead in the 1920s or thereafter. The incorporation of nonscientific and antiscientific perspectives developed in the 1960s, influenced by the work of Clifford Geertz as well as

other more humanities-oriented anthropologists (Jarvie 2000, 542-43; Harris 1980; Kuper 1999, 542-43; Shankman 1984, 2017).¹⁰ Mead herself supported science. In 1976, 2 years before her death, she was elected president of the American Association for the Advancement of Science. As Jarvie notes, Mead's scientific reputation "is solid" (Jarvie 2012, 536).

8. The Trashing of Margaret Mead and the Politics of Reputation

The Mead/Freeman controversy has inextricably involved the reputations of Mead and Freeman themselves. Defending Freeman, Jarvie contends that he never personally attacked Mead or tried to tarnish her reputation in his publications (although he notes that Freeman did "in private" [Jarvie 2012, 538]). He views my argument to the contrary as based on *ad hominem* gossip that has no place in scholarly discourse. In Jarvie's view, the controversy was and should remain purely academic. To be sure, there is a purely academic side to the controversy, and my academic work on the controversy has been published in refereed academic journals. However, as I stated in my book,

While the factual issues in the controversy can be examined without reference to the anthropologists involved, understanding the development of the controversy and its consequences for Mead's reputation (and Freeman's as well) cannot be separated from his personality and motives. (Shankman 2009, 19)

In a similar fashion, Freeman (1999, 212) argued that any appraisal of Mead's fieldwork should include an appraisal of Mead herself, stating, "There can be no adequate understanding of one without the other." People interested in the controversy's academic issues can read about them in academic journals. If they want to know more about the controversy and the people involved, they can turn to works like Freeman's second book on Mead, the many biographies of Mead, my book, or the recent biography of Freeman (Hempenstall 2017).

The controversy had its origins in a broader context and has had consequences far beyond the confines of academia. It also involved real people with strengths and flaws. In the introduction to my book, I noted:

¹⁰Although Jarvie (2000) had high praise for Adam Kuper's book (1999) because of its critique of culturalism, Kuper (1991) also wrote a robust critique of Freeman's first book on Mead as well as the Heimans documentary.

Some of the book is based on personal experience, and some of it presents Freeman and Mead in an all-too-human light. It is difficult if not impossible, to understand the genesis and development of the controversy without understanding the people involved, especially Derek Freeman. As Robin Fox, a long-time colleague of Freeman, commented, the controversy “would have never happened without Derek’s particular personality meshing with the cultural clashes of the times. [Fox 2005:7].” (Shankman 2009, 17)

Jarvie finds it disturbing that I would write about Freeman as a person as well as a scholar, asserting that Freeman behaved professionally throughout the controversy and was merely trying to make an “anthropological point” (Jarvie 2012, 540). He does allow that “Freeman was no doubt a difficult man, given to portentous and bombastic language and a good deal of self-dramatization.” (Jarvie 2012, 539). Yet, “Crazy or not, Freeman did not ‘trash’ either Margaret Mead or her reputation” (Jarvie 2012, 540). Thus, he finds my depiction of Freeman “invalid,” “reductive and irrelevant” (Jarvie 2012, 537).

My account of Freeman’s behavior was based on careful research, including published and unpublished sources, his personal correspondence, and discussions with his colleagues and former students, as well as a number of hours spent with Freeman reviewing issues in the controversy at its height in 1984. This research demonstrated that Freeman publicly criticized Mead in a deliberate and personal manner, despite his assurances and Jarvie’s to the contrary. Prior to the publication of his first book on Mead, Freeman stated that her reputation would descend at the rate of a “falling body” (32 feet per second squared) and that he “may have written a book that will create the greatest denouement in the history of anthropology, not excepting the Piltdown hoax” (Howard 1984, 67). After the publication of *Margaret Mead and Samoa*, he stated, “There isn’t another example of such wholesale self-deception in the history of the behavioral sciences” (McDowell 1990, 213), and after the initial publication of the hoaxing argument, he determined that Mead was not simply wrong but in a “chronic state of cognitive delusion” about Samoa throughout her life (Freeman 1991, 117). Freeman also alleged in print that Mead had affairs with Samoan men (Freeman 1991, 115-16), although there is no credible evidence of this (Shankman 2009, 200-202). These are Freeman’s own words, and they do not seem to be making “an anthropological point.” Rather, they illustrate some of the ways in which he trashed Mead’s reputation.

Freeman’s antipathy toward Mead was no secret. Robin Fox reported that, long before the controversy commenced, Freeman thought there was “a special place in hell reserved for Margaret Mead, for reasons not at all clear at

that time” (Fox 2004, 339). Freeman’s biographer, Peter Hemenstall, has also linked Freeman’s private view of Mead to his public critique of her, noting that while Freeman was an accomplished scholar, he had an “obsession with Mead” and that he “ruined his later life with this obsession” (2010, 284). Hemenstall (2010, 283) also writes that “Freeman was an intellectual bully in many ways” and that he was “crusading, morally puritanical, and [Karl] Popper-obsessed.” Although knowledge about Freeman as an individual does not resolve the academic issues in the controversy, it may illuminate some of the motivation behind his involvement in it.

9. Conclusion

Throughout the controversy, Freeman promised his audience “startling new evidence,” “smoking gun” evidence, a “final” verdict on Mead, and a new “paradigm” that would bring anthropology into the 21st century. According to Freeman, the discipline had been under the “anti-scientific” and “anti-evolutionary” spell of Mead for decades, and now there would be a moment of reckoning. These tantalizing promises were delivered with great conviction and high drama. Unfortunately, Freeman did not deliver on them. Mead and *Coming of Age in Samoa* did not hold sway over anthropology as Freeman imagined. The issues in the controversy were often mundane and readily examined with existing evidence, some of it based on his own research. Freeman claimed that his work was a necessary course correction to the widespread influence of Mead’s *Coming of Age in Samoa* and that he was providing a new path forward in the name of science, anthropology, and the honor of Samoans. However, none of these causes were well served by Freeman’s arguments and use of evidence.

In retrospect, the controversy has generated much heat but little light. It has encouraged some scholars to look more closely at the history and nature of Samoan sexual beliefs and practices, Samoan adolescence, the lives of Mead and Freeman, the history of anthropology, and the broader influence of anthropology on public life in America. Recent research on the biological and psychological correlates of adolescence could be helpful in moving discussion about the nature/nurture issue forward. However, on this issue, Freeman offered nothing new. On the alleged “hoaxing” of Mead, there is no compelling evidence that it ever took place. And the idea that the “grand theory” of cultural determinism has collapsed due to the existence of an “ugly little fact” also receives no support.

Nevertheless, Mead’s reputation has suffered considerably as a result of Freeman’s deeply flawed critique.

Although Freeman and Jarvie view the critique of Mead and *Coming of Age in Samoa* as vital to the larger struggle for the soul of anthropology, for most cultural anthropologists, the controversy has been of less significance. After more than 30 years of controversy, Freeman's agenda is still in search of a substantial audience within cultural anthropology. And, as Jarvie (2001, 562) has acknowledged, there is slim hope of finding such an audience in the future.

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