

## Culture, Biology, and Evolution: The Mead-Freeman Controversy Revisited

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*Derek Freeman argues that the central issue in the Mead-Freeman controversy is evolution. He views Margaret Mead's Coming of Age in Samoa as not only misleading about Samoa but as a "sacred text" that promoted an antievolutionary paradigm among American cultural anthropologists. A review of Mead's writing on culture, biology, and evolution demonstrates that contrary to Freeman's claim, Mead favored an evolutionary approach throughout her career. Moreover, while Mead's book was a popular text and a bestseller, it was not a "sacred text" among anthropologists. Freeman's misrepresentation of the historical record concerning Mead's views and the place of Coming of Age in anthropology raises major questions about his scholarship.*

### INTRODUCTION

Freeman's current critique of Margaret Mead's *Coming of Age in Samoa* (hereafter CA) includes 3 interrelated arguments: (1) Mead was wrong about Samoan sexual conduct because (2) she was "hoaxed" by 2 young women telling innocent lies that Mead believed as the truth and that (3) the book resulting from this "fateful hoaxing" reinforced the antievolutionary paradigm of "absolute cultural determinism" which, in turn, caused anthropologists and the public at large to neglect the roles of biology and evolution in human behavior for over half a century (Freeman, 1997, p. 68).

Much of the response to Freeman's critique has dealt with his first 2 arguments, showing them to be deeply flawed.<sup>2</sup> But the third argument also deserves careful

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<sup>2</sup>See, for example, the critiques by Côté (1994, 1998), Orans (1996, 1999), and Shankman (1996, 1998a). Freeman's responses to these critiques sometimes take contradictory directions. For example,

review because it is the ultimate point of Freeman's critique. He believes that the controversy his work has generated is, at heart, about evolution. The allegedly antievolutionary "Mead paradigm" is for Freeman the most significant and far-reaching consequence of what he calls "Mead's mistake" in Samoa.

This article will discuss Freeman's argument concerning the relationship of culture, biology, and evolution in Mead's work. Freeman's own views about the relationship of culture, biology, and evolution will also be reviewed and compared with Mead's. In addition, the influence that CA and Mead had on anthropology will be discussed. To anticipate the conclusions of the article, I will demonstrate that Mead was not an "absolute cultural determinist" and that she regarded biology and evolution as important areas of study for anthropologists. Freeman's argument about the antievolutionary "Mead paradigm" and "absolute cultural determinism" misrepresents the documented historical record about Mead's thought as well as her influence on anthropology. Indeed, these are nonissues.

But if Freeman's argument so clearly misrepresents Mead's views, why has it attracted such attention? This is an important question. Much of the attention has come from nonanthropologists, journalists, and the public. Within anthropology, Freeman's critique has not been well received. In fact, most anthropological commentary has been critical. But outside of the profession, Freeman's visibility has risen. Part of his success in the public realm comes from his invocation of science, biology, and evolution. It also stems from Freeman's sense of high drama in which he portrays a world of "paradigms in collision" (Freeman, 1992, 1997), a world in which Mead, antiscience, and antievolutionary thought are pitted in against Freeman, science, and evolution. The language that Freeman uses is replete with rhetorical absolutes such as "purely," "totally," "wholly," and "completely," in reference to Mead's alleged errors. Thus, for example, Freeman claims that Mead was "grossly hoaxed" and that she "completely misinformed and misled virtually the entire anthropological establishment. . ." (Freeman, 1997, p. 68, italics used for emphasis). Freeman's sense of certainty is compelling. Presented in this manner, a matter of minor professional interest became a major media event.

In attempting to refute my critique of his view of sexual conduct in Samoa (1996), Freeman (1998) tries to discredit a source previously central to his argument against Mead. In his first book, Freeman cited and quoted the German observer Augustin Krämer, noting his "monumental" social history of Samoa (Freeman, 1983a, p. 115) and crediting Krämer with understanding the "particularities, intricacies, and subtleties" of Samoan culture (Freeman, 1983a, p. 285), including sexual conduct. But after I provided a closer reading of the same source that cast doubt on his critique of Mead, Freeman questioned the reliability of Krämer on sexual matters. Indeed, Freeman (1998) stated that Krämer relied on Europeans for his knowledge of the culture, implied that Krämer did not speak the language, believed that Krämer spent little time in villages, and concluded that Krämer was valuable primarily when researching genealogies and other oral traditions. In fact, none of these assertions is true. Krämer relied heavily on Samoans for his knowledge of the culture, spoke the language, spent time in villages as well as in the port town, and examined many aspects of the culture well beyond genealogies (Shankman, 1998b). Most recently, after dismissing the reliability of Krämer on sexual matters in his response to my article, Freeman (1999a, p. 178) once again cites Krämer as an important source on "the sexual mores of Samoans" in his continuing critique of Mead!

A second reason that Freeman's work has received attention in the media is less obvious. He relies on partial quotation, rarely quoting whole sentences, let alone whole passages. Freeman also omits relevant passages in the works he does cite and certain relevant works are not cited at all. Many of Freeman's critics have noted this problem (see McDowell, 1984; Shankman, 1996; Orans, 1999). The result is that while Freeman's work appears academically authoritative on first reading, it is only by returning to his sources and evaluating his use of them that we can judge the academic soundness of his arguments. The media are poorly equipped to referee the scholarly quality of Freeman's work and typically have not reviewed the accuracy of his presentation of Mead's work.

In this article, Mead's published record will be quoted extensively in order to document her thinking on culture, biology, and evolution and to provide a context in which to understand Freeman's arguments. As we shall see, by systematically misrepresenting Mead, Freeman calls into question his own scholarship, which should be the central issue in this long-running controversy.

### NATURE AND NURTURE

Freeman (1999a, p. 217) concludes both his books about Mead with a call for an anthropology that addresses nature *and* nurture, not nature *or* nurture. Using almost identical wording to end each book, he states,

The time is thus conspicuously at hand for an anthropological paradigm that gives full recognition to the radical importance of both cultural and biological factors, and of their past and ongoing interaction.

In *The Fateful Hoaxing of Margaret Mead*, he adds one final thought: "*To enact this is the principal task of the anthropology of the twenty-first century.*" (italics in original, Freeman, 1999a, p. 217).

The rationale behind this declaration stems from Freeman's view of the nature/nurture controversy during the early twentieth century and its course over the remainder of the century. Freeman finds that at a crucial intellectual turning point in the 1920s, Mead was deeply influenced by her mentors Franz Boas and Ruth Benedict, who Freeman believes were "absolute cultural determinists." As a result, Mead became a "Boasian ideologue" committed to the "ideology of absolute cultural determinism" (Freeman, 1999a, p. 212) in which biological factors play no role in human behavior. According to Freeman, this prior belief led Mead to submit findings about Samoa that were profoundly wrong; nevertheless, this belief endured over the course of Mead's life and became the cornerstone of "the Mead paradigm." CA would become a foundational work in anthropology and immensely influential beyond the discipline.

Freeman (1997, p. 68) elaborates on the consequences of "Mead's mistake," stating that

We are here dealing with one of the most spectacular events of the intellectual history of the 20th century. Margaret Mead, we now know, was grossly hoaxed by her Samoan informants, and Mead, in her turn, by convincing others of the "genuine-ness" of her account of Samoa, completely misinformed and misled virtually the entire anthropological establishment, as well as the intelligentsia at large. . . .

As evidence of Mead's influence, Freeman (1999a, pp. 191–202) cites several well-known anthropologists and a number of well-known public figures who have made approving remarks about her work. He also believes that the reaction to his critique by Mead's "cult-like" following of American anthropologists is proof of her pernicious influence. Freeman (1997, pp. 69, 70) deplors the adherence to a "prescientific" ideology by these anthropologists and their devotion to a "totemic mother."

Freeman's argument about culture and biology in the work of Mead can be condensed into 2 assertions:

1. Mead believed that culture wholly determined human behavior and that biological factors played no role.
2. This belief endured throughout Mead's life as CA became a "sacred text" in anthropology, and the antievolutionary "Mead paradigm" became "the hal- lowed dogma" of American cultural anthropology (Freeman, 1997, p. 68).

Let us examine each of these assertions in more detail.

#### CULTURE AND BIOLOGY IN COMING OF AGE IN SAMOA

Freeman believes that the absolute cultural determinism of the "Mead paradigm" is self-evident in CA. Using 2 sentences from Mead's book, he summarizes them as stating that "we cannot make any explanations" of adolescence in terms of the biological process itself (Freeman, 1983a, pp. 77, 78). Yet, as Rappaport (1987, pp. 6, 7) noted in an exchange with Freeman in *Scientific American*, Freeman quoted Mead out of context. The full passage from Mead (1928, p. 197) reads as follows:

A further question presents itself. If it is proved that adolescence is not necessarily a specially difficult period in a girl's life—provided it is if we can find any society in which that is so—then what accounts for the presence of storm and stress in American adolescents? First, we may say quite simply, that there must be something in the two civilisations to account for the difference. If the same process takes a different form in two different environments, we cannot make any explanations in terms of the process, for that is the same in both cases. But the social environment is very different and it is to that we must look for an explanation. What is there in Samoa which is absent in America, what is there in America which is absent in Samoa, which will account for the difference?

Rappaport (1987, p. 7) comments that

The "same process" that takes "different form" in "different environments" is identified on page 196 as "the process of growth by which the girl baby becomes a grown woman."

Mead assumes the "developing girl is a constant factor in America and in Samoa" and asks if "the sudden and conspicuous body changes which take place at puberty [everywhere]" are necessarily accompanied by the kinds of emotional and cognitive upheavals common in American girls. Are conflict and stress inevitable concomitants of "change in the girl's body"?

Mead, it seems clear, recognized the biological character of puberty, never claimed that biological factors have nothing to do with behavior and simply stated that differences in the emotional and cognitive correlates of "the same [biological] process" in "different environments" are to be accounted for by differences in environment.

This was the crux of CA. Mead argued that puberty was a universal biological process, but that there were differences in the way this process was shaped by different cultures. Cultural differences arose from a common biological basis, yet they could not be explained by biology alone. These differences suggested to Mead that there was no single outcome of adolescence. Americans could, therefore, make choices about shaping this stage in the human life cycle.

The principle of cultural determinism posits that biological constants cannot by themselves explain cultural variability. It does not mean that biological factors play no role whatsoever. For Mead, puberty was a biological process. The question of how adolescents experienced this process and how cultures interpreted and shaped it were interesting for Mead and others in the first half of the twentieth century because they were concerned with the description of cultural variability. At that time, ethnographic priority was given to the documentation of cultural variability because relatively little good descriptive data existed (Bennett, 1998).

Mead (1949, p. 113) further clarified her position on the roles of biology and culture in a chapter in *Male and Female* entitled "Basic Regularities in Human Sexual Development," noting that,

different as are the ways in which different cultures pattern the development of human beings, there are basic regularities that no known culture has yet been able to evade. After excursions into contrasting educational methods of seven different societies, we can sum up the regularities that must be reckoned with by every society. Every attempt to understand what is happening in our own society, or in other societies, every attempt to understand ourselves, or to build a different life for our children, must take these into account.

She restated her position again in the Preface to the 1950 edition of *Sex and Temperament in Three Primitive Societies*, this time commenting that biology not only limited human development, it also provided potentialities, arguing that the "biological bases of development as human beings, although providing limitations that must be honestly reckoned with, can be seen as potentialities by no means fully tapped by our human imagination" (Mead, 1950, p. 7).

Mead's views on the interaction of biology and culture were complex, not simplistic, and developed over her long career. In her early work, Mead did argue that human nature was extremely malleable. However, as Degler (1991) found in his comprehensive review of Darwinian thought in the twentieth century, Mead's views on human nature developed over 4 decades, during which the political and intellectual climate was changing. Thus, when arguing against racial and

exclusively biological explanations, Mead (1964, pp. 10–11) emphasized culture; when discussing sex roles, biology received more attention. She comments that

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.

Far from naively embracing a “*tabula rasa*” point of view as Freeman believes, Mead (1942, 1947) specifically pointed out its weaknesses in the 1940s, while noting the importance of the interaction of biology and culture in the human maturation process.

### BIOLOGY AND EVOLUTION IN MEAD'S LATER WORK

Freeman (1999a, pp. 200, 201) alleges that it was only late in Mead's career that she mentioned incorporating biological factors into her work, and even then only in a peripheral manner. He contends that

in her introduction of April 15, 1961, to the Pelican edition of *Male and Female*, she noted that if she had been writing this book in the early 1960s rather than in the late 1940s, she would have laid “more emphasis on Man's specific biological inheritance from earlier human forms.” This was largely due to the friendship she had formed in 1954 with the eminent ethologist Konrad Lorenz, whose photograph she had on the wall of her office in the American Museum of Natural History. However, this recognition by Mead in 1961 of “man's specific biological inheritance” did not lead to any reconsideration of her conclusion of 1928 that “we cannot make any explanations” of adolescent behavior in terms of the process of adolescence itself, this being the doctrine on which her anthropological reputation is based.

Here Freeman compounds his initial error in *Margaret Mead and Samoa* by further misconstruing Mead's intellectual biography. Why would Mead need to reconsider a conclusion that she never reached in the first place?

Freeman's interpretation of Mead's allegedly belated and limited interest in biological factors is effectively countered by Mead's autobiographical discussion of her renewed interest in biology and evolution at a much earlier date. In *Continuities in Cultural Evolution*, Mead (1964, p. vii) prefaces the book with the following statement:

My interest in evolution was reawakened in 1948, when I was asked to review *Touchstone for Ethics* and, while I was doing so, also took time to reread *The Origin of Species*. This reading in turn reawakened memories of discussions, in the mid-1930s, with C. H. Waddington and Gregory Bateson. Renewed interest in the study of animal behavior, an interest which I owe originally to Kingsley Noble and Ray Carpenter, was stimulated by contacts with Konrad Lorenz in the World Health Organization Study Group on the Psychobiological Development of the Child, and later by work with both American and European students of comparative animal behavior in the Macy Conferences on Group Processes.

Two pieces of writing—*Male and Female*, written in 1948, and “Cultural Determinants of Sexual Behavior,” first written in 1950 for the compendium, *Sex and Internal Secretions*—which I was able to discuss extensively with Evelyn Hutchinson, focused my attention on the need to integrate more specifically our knowledge of man's species-characteristic behavior, the peculiarities introduced by domestication, and our knowledge of cultural evolution. An invitation to participate in the second of two Symposia on Behavior and Evolution, organized by Ann Roe and George Simpson in 1955, created the necessary focus.

Throughout the remainder of her Preface to *Continuities in Cultural Evolution*, Mead refers to additional colleagues and conferences that stimulated her thinking about evolution, as well as the changing perspectives on biology and culture that occurred from the 1930s through the 1950s.

One of Mead's clearest statements about the significance of biological factors and evolution can be found in her presidential address to the American Anthropological Association in 1960, later published in the discipline's premier journal, the *American Anthropologist*. Mead (1961, p. 480) stated that genetics is “enormously relevant to problems absolutely central to our discipline” and was concerned that research on genetics had been largely confined to physical anthropology. She also urged her colleagues to take advantage of “the opportunity provided by the new upsurge of interest in the whole field of evolution, in which human evolution is one part and cultural evolution a smaller one,” and she reminded anthropologists that Theodosius Dobzhansky, George Gaylord Simpson, and other natural scientists were interested in communicating with them about evolution (Mead, 1961, p. 481). Mead also mentioned the importance of evolution in her presidential address to the American Association for the Advancement of Science, subsequently published in *Science* (1976).

Given such public statements on evolution in important forums, how can we take seriously Freeman's assertions that Mead believed that “all human behavior is the result of social conditioning” and that she was “totally unevolutionary” (Freeman, 1999b, p. 60)? Freeman simply neglects those parts of Mead's work that do not support his views while misrepresenting her published record.

Had Mead been an “absolute” cultural determinist, she would have opposed sociobiology, which emerged in the 1970s near the end of her life. However, Wilson (1994, p. 331), the founder of sociobiology, recalls in his autobiography that

at the 1976 American Anthropological Association meetings, a motion was made to formally censure sociobiology and to cancel two symposia on the subject that had previously been scheduled. During the debate on the matter Margaret Mead rose indignantly, great walking stick in hand, to challenge the very idea of adjudicating a theory. She condemned the motion as a “book-burning proposal.” Soon afterward the motion was defeated—but not by an impressive margin.

The following year, at a conference on human behavior, Mead invited Wilson to dinner to discuss sociobiology. Wilson (1994, p. 348) remembers,

I was nervous then, expecting America's mother figure to scold me about the nature of genetic determination. I had nothing to fear. She wanted to stress that she, too, had published ideas on the biological basis of social behavior.

### FREEMAN'S VIEWS ON BIOLOGY, CULTURE, AND EVOLUTION

What of Freeman's own views on biology, culture, and evolution?<sup>3</sup> They are, in fact, very similar to Mead's. Freeman often emphasizes the importance of culture. Like Mead, he believes that since humans can learn nongenetically and transmit information symbolically, culture often gives meaning to behavior. He notes that people may attribute different cultural meanings to the same genetically prescribed behaviors. As an example, he cites the genetically prescribed behavior known as the eyebrow flash, which means "yes" in Samoa but means "no" in Greece. According to Freeman, "It is the existence of such conventional behaviors, in great profusion, in all human populations, that establishes, indubitably, the *autonomy of culture*" (Freeman, 1980, p. 215, italics is used for emphasis). Moreover, he notes that human history reveals "boundless diversity and often extreme variability of action" that "cannot possibly be explained by changes in gene frequencies" (Freeman, 1980, p. 209).

On the issue of choice, Freeman is as much a cultural determinist as Mead. Freeman (1980, p. 215) states that

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, and 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.

He concludes that humans, "with their biologically given and culturally nurtured capacity for alternative action, cannot be said to have any kind of 'ultimate nature'" (Freeman, 1981, pp. 99, 100).

Freeman stresses our primate heritage, our evolutionary history, and the emergence of culture as a biologically based means for allowing choices to be made and transmitted through nongenetic mechanisms. This view of culture, based on a common biological heritage, is the one he shares with Mead. She (1964, p. 31) stated that

<sup>3</sup>Freeman's own interest in biology and evolution apparently began in the late 1950s, according to Appell and Madan (1988, p. 12), with his realization of the limits of social anthropology. But it was in Borneo in 1961 that Freeman experienced a "cognitive abreaction" that "suddenly" caused him to see human behavior in a "totally new light" (Appell and Madan, 1988, p. 12), after which he began reading in ethology, the neurosciences, evolutionary biology, primatology, and genetics. What actually happened in Borneo is unclear in Freeman's intellectual biography, but is alluded to in Williamson's play *The Heretic* (Williamson, 1996) about Freeman and written with his help. In the play, the character of Freeman remembers an intense encounter in Sarawak, which led him to destroy what he believed was a fake indigenous pornographic carving (Williamson, 1996, pp. 38-42). The Freeman character describes what happened as a "cognitive abreaction" which provided him with "an important insight that changed my life" (Williamson, 1996, p. 38). Other characters in the play refer to this incident as a mental breakdown; a psychiatrist was called in and a colleague flew in to Sarawak in order to escort Freeman back to Australia. A factual account of these events, reconstructed from witnesses, is now available (Heimann, 1997, pp. 332-335).

[c]ultural systems will be treated as extensions of the power to learn, store, and transmit information, and the evolution of culture as dependent upon biological developments of these abilities and the cultural developments to actualize them.

The emphasis on cultural variables at one point in time and biological variables at another may give the appearance of the existence of 2 very different paradigms. Yet for Mead, the focus on culture or biology was a matter of emphasis, not one of irreconcilable differences. In *Continuities in Cultural Evolution* (Mead, 1964, p. 25), she stated that

At some points in the history of anthropology it has been important to stress the discontinuity between man as a culture-building animal and all other living creatures. It has also been important to stress that man is a mammal with certain types of behavior appropriate to mammals and to identify these behaviors which can be recognized as related between monkeys, apes, and man.

Mead, along with most anthropologists, would no doubt have agreed with Freeman (1997, p. 70) when he asserts that

humans, like our chimpanzee cousins, far from being empty tablets at birth, are born with phylogenetically given primate nature, components of which remain with us throughout our lives beneath all of the conventional behaviors that we acquire by learning from other members of the society to which we belong.

Freeman advocates an interactionist paradigm in which the genetic and exogenetic (i.e., cultural) are distinct but interacting parts of a single system (Freeman, 1983a, p. 299) and in which genetic factors combine with environmental factors to influence behavioral differences among individuals (Freeman, 1997, p. 71). Mead anticipated this position in 1935 when discussing the role of innate temperament and its interaction with culture. In *Behavior and Evolution*, edited by Anne Roe and George Gaylord Simpson, she commented that "We can get some picture of how change occurs only when each individual is fully specified in his genetic and experiential peculiarity . . ." (Mead, 1958, p. 496).

At this level of generality, Mead's cultural determinist position and Freeman's interactionist position are virtually identical; both acknowledge a relationship between culture and biology. However, at the level of ethnographic specificity, the interactionist position, as Freeman states it, cannot explain cultural variability. Take, for example, the phenomenon of bereavement. Freeman has noted that bereavement is a pan-human phenomenon, and the fact that the colors associated with the symbolic expression of grief vary in different cultures from black to white does not significantly affect the basic characteristics of human bereavement. Yet there is more to bereavement than color symbolism. As Rosenblatt (1996, p. 548) comments, while grief and mourning are basic human psychological universals,

cultures vary enormously in how expressive people are in their grief and what they express. There are cultures in which the expectation and the general practice is that expression of grief is quite constrained or even looks (to an outsider) as though grief is being denied.

There are also cultures in which grief from some losses is expressed intensely for many years.

He finds that the existence of biopsychological universals does not explain these patterns of cultural variability. Freeman's interactionist approach acknowledges cultural variation, but does not explain it.

The same problem arises in Freeman's explanation of rape and the "cult of virginity" in Samoa, which Freeman relates to dominance hierarchies among Samoan men. These dominance orders do have cultural aspects, according to Freeman (1983b, p. 128), but they cannot be understood unless the relevant biological variables are also taken into account. In rape, the biological variables that Freeman (1983b, p. 128) notes are the age of the rapists and the hormonal states of the relatively few individuals involved, especially their testosterone levels. This is an interesting hypothesis, but Freeman offers no Samoan data on hormonal states or testosterone levels to support it.<sup>4</sup> And at the cross-cultural level, his interactionist approach is not able to address questions about why some cultures have higher rates of rape or why some cultures value virginity more than others do (see Harris, 1983). Cross-cultural studies that attempt to answer these questions have been available for some time (Sanday, 1981; Schlegel, 1991), but Freeman does not cite them or recognize their relevance to the phenomena that he views as important test cases for the interactionist paradigm.

Thus, when the question of cross-cultural variability is raised, Freeman's interactionism is no more theoretically adequate than cultural determinism was in the 1920s. Both recognize variation; neither explains it. Today, the use of sophisticated cross-cultural studies has made the explanation of cultural variability possible and offers a way out of this cul de sac. Empirical answers to questions about cross-cultural variation have gone well beyond noting that culture and biology interact as part of a single system. Indeed, based partly on her fieldwork in several cultures, Mead (1937) actually attempted some early cross-cultural research.

Given Mead's view of the relationship of biology and culture, and given that her interest in evolution was articulated *long before* Freeman developed his current view on these subjects in the 1960s and thereafter, one might have expected Freeman to review and discuss Mead's actual positions as they developed over her

<sup>4</sup>Freeman's initial explanation of rape in Samoa was largely cultural. He discussed rape as involving "culturally transmitted male practices," including the "culturally standardized stratagem" of knocking the girl unconscious (Freeman, 1983a, pp. 248, 249, italics used for emphasis). He concluded that "both surreptitious and forcible rape have long been *intrinsic to the sexual mores* of Samoan men and are *major elements* of their sexual behavior" (Freeman, 1983a, pp. 249, 250, italics used for emphasis). After I noted Freeman's use of culture in his explanation of rape (Shankman, 1983, pp. 49, 50), Freeman (1983b) responded by invoking biological variables. Interestingly, Freeman views rape in Samoa as part of a cultural strategy by males to obtain marriage partners because victims of rape are so ashamed that they prefer silence or eventual marriage to the rapist rather than their public exposure as nonvirgins. This view seems to run counter to the recent biological explanation of rape as a reproductive strategy put forth by Thornhill and Palmer (1999) and may pose an important challenge to it. For further anthropological commentary on Freeman's view of rape in Samoa, see Nardi (1984).

50-year career. Yet Freeman fails to review Mead's work, cites Mead selectively on the roles of culture and biology, and omits discussion of her interest in evolution.

Instead, Freeman attempts to enlist the legitimacy of evolution and interactionism as a weapon in his effort to cast doubt on the work of Mead. But since there never was a "Mead paradigm" of "absolute cultural determinism" to begin with, there are no "paradigms in collision."<sup>5</sup> By misrepresenting Mead's views and by presenting himself as the guardian of evolution and interactionism, Freeman asks his readers to dismiss Mead's work as mistaken, misguided, and anachronistic, and accept his position as accurate, responsible, thoroughly scientific, and a harbinger of the future. The choice, however, is not between Mead on the one hand and Freeman on the other. It is between wondering whether Freeman has read what Mead said and, for whatever reason, omitted entire passages and works that do not support his argument, or whether he did not carefully read Mead and, therefore, is not fully aware of what she has written. Neither of these choices reflects well on Freeman's scholarship.

### COMING OF AGE AS A "SACRED TEXT"

Beyond the issue of biology, culture, and evolution is Freeman's second argument about CA; that is, the book was not only a product of the doctrine of "absolute cultural determinism," but also a primary vehicle for the spread of this doctrine, becoming a "sacred text" in anthropology and enormously influential beyond anthropology. There is no doubt that Mead's first book, a popular book, was a bestseller by any standard. It was used in college courses, praised by scholars and nonacademics alike, and became so well known that it remains a common reference point in public conversations. But was CA a "sacred text" within the discipline?

In *Margaret Mead and Samoa: the Making and Unmaking of an Anthropological Myth*, Freeman (1983a, pp. 97-108) asserts that CA had widespread influence in anthropology and offers some support for this assertion. More recently, he concludes that Mead became a "mother goddess" in American anthropology (Freeman, 1997, p. 70) and that her findings in CA were reiterated, rather than challenged, in an "unbroken succession of anthropological textbooks" (Freeman, 1987, p. 392). Although Freeman cites only 5 texts to support this claim, he believes that her Samoan findings were affirmed "without exception" in "countless textbooks" (1999a, pp. 200, 201).

To provide a test of Freeman's claim, Hays (1997, p. 81) conducted a systematic study of CA's influence on 118 introductory anthropology texts published

<sup>5</sup>There is a school of thought within the social constructivist approach that closely resembles "absolute" cultural determinism (see Ehrenreich and McIntosh, 1997), but it draws on a very different intellectual heritage than Mead. Here there are "paradigms in collision" as can be illustrated in the so-called "science wars."

between 1929 and 1990. He summarizes his findings as follows:

A survey of 118 introductory anthropology textbooks published in the period 1929–1990 examines the ways in which Margaret Mead's *Coming of Age in Samoa* has been presented to college undergraduates. In contrast to Derek Freeman's claim that her conclusions about Samoan sexuality and adolescence have been reiterated (approvingly) in an "unbroken succession of anthropological textbooks," it appears that this work has been ignored almost as often as it has been cited. Criticisms of Mead, although relatively few and entirely methodological, have also been incorporated into textbooks, both before and following Freeman's 1983 book, *Margaret Mead and Samoa*. Whether or not Mead has been a "holy woman" in American cultural anthropology, *Coming of Age in Samoa* does not appear to have been a "sacred text."

Hays also compares CA with other Pacific ethnographies from the same era. In a sample of 112 textbooks published between 1940 and 1990, Hays found that Malinowski's *Argonauts of the Western Pacific* (1922) was by far the most cited ethnography, appearing in 109 of the 112 texts. CA finished a distant second, just ahead of Spencer and Gillen's *The Arunta* (1927), which was tied with Mead's own *Growing Up in New Guinea* (1930) with 48 citations in 112 texts (Hays, 1997, p. 96). If there is a sacred text from this period, it is Malinowski's *Argonauts*.

Had CA been a "sacred text" among American anthropologists, it should have appeared prominently in texts on "culture and personality" or "psychological anthropology," the field to which Mead's work most clearly contributed. Indeed, Bock (1980, p. 74) has stated that Mead was "the major figure in the culture and personality school." To test this idea, I reviewed 12 culture and personality texts published between 1945 and 1982. Only volumes written before the controversy were used; they are Barnouw (1979), Bock (1980), Bourguignon (1979), Honigmann (1967), Hsu (1961, 1972), Kardiner *et al.* (1945), LeVine (1973), Spindler (1978), Williams (1972), Wallace (1962, 1972).

In examining these texts for evidence of CA's deep and pervasive influence, I found surprisingly little discussion of the book, either positive or negative. Three books make no mention of CA at all, while 5 others contain either mixed comments or criticisms. Three use Samoan examples very briefly, but do not evaluate the book itself. In only one of these volumes is there a uniformly positive appraisal of CA (Bock, 1980, p. 74), and even here the author does not find Mead to be an "absolute" cultural determinist. In other words, CA receives little attention from the very anthropologists who should have been citing it.<sup>6</sup>

<sup>6</sup>In *The Fateful Hoaxing of Margaret Mead*, Freeman (1999a) also cites several prominent anthropologists who appear to "uncritically" embrace CA. From the 1920s and early 1930s, he refers to Boas, Benedict, and Bronislaw Malinowski as examples. Other more recent figures include George P. Murdock, Raymond Firth, A. Irving Hallowell, George Spindler, and Francis L. K. Hsu (Freeman, 1999a, p. 200). Yet a closer look at what this latter group of anthropologists actually wrote reveals how partial quotation and/or decontextualization by Freeman distort their words.

- Murdock (1934) wrote a chapter on Samoa in *Our Primitive Contemporaries* and clearly borrows from CA in describing adolescent sex. Yet Murdock notes that the daughters of chiefs, and especially the village princess (*taupou*), were carefully guarded and that there

But if CA was not a "sacred text" for anthropologists, then what kind of influence did it have on the discipline? CA made Mead famous. It did not make her or the book immune from criticism. A number of anthropologists responding to Freeman's critique readily acknowledged that CA contained overstatements, simplifications, and errors (Hays, 1997, p. 84). Yet most of these same anthropologists were not sympathetic to Freeman's critique, faulting him for misunderstanding cultural determinism, misrepresenting American anthropology, and overstating and simplifying his own portrait of Samoa. These anthropologists often drew a distinction between Mead's professional contributions in contrast to her popular work such as CA. As one senior colleague put it, CA was assigned to undergraduates as a popular book to draw their interest; it was not assigned to graduate students as a model for professional research. The very success of the book made it a popular text rather than a "sacred text," underscoring the difference between popularity and theoretical influence (see Lutkehaus, 1995; Mitchell, 1996).

More generally, most American anthropologists regard Mead as an important pioneer in the early twentieth century. Today her stature is not directly tied to either her popular work or her theoretical contributions. A review of Mead's career indicates increasing public recognition over her life, but declining theoretical influence within the profession (diLeonardo, 1998). By the 1960s and 1970s much of her earlier theoretical work on child socialization, national character, and other aspects of "culture and personality" research had been modified, revised, and even

were severe sanctions for incest and "sleep-crawling" (Murdock, 1934, pp. 72–73). Murdock uses over 30 other standard sources on Samoa (Murdock, 1934, pp. 82–84) and states that the chapter on Samoa benefited from the authoritative criticisms and suggestions of Professor Peter H. Buck (Te Rangī Hiroa), who had done fieldwork in Samoa in the 1920s. Four sources on Samoa are marked with an asterisk as "the most reliable comprehensive works" (Murdock, 1934, p. viii); CA is not in this set although Mead's *Social Organization of Manu'a* (Mead, 1930) is. Among this group of anthropologists that Freeman alleges uncritically accepted Mead's writings on Samoa, Murdock is the only one who actually uses CA.

- Firth's *We the Tikopia* (Firth, 1936) mentions CA in a brief footnote concerning kinship. There is no evaluation of Mead's book anywhere in his anthropological classic.
- Hallowell's essay (Hallowell, 1939) mentions CA as demonstrating the importance of culture. But if Mead's message was antievolutionary, as Freeman argues, then Hallowell must not have understood it, for the essay is about the relationship of evolution and culture. In fact, Hallowell was an important thinker about evolution and psychology.
- Hsu (1980) wrote a very complimentary essay about Mead's contributions to psychological anthropology for a special issue of the *American Anthropologist* devoted to Mead following her death. But in Hsu's essay, CA is referred to only in passing and receives no systematic analysis or evaluation.
- Spindler (1978, p. 87) is quoted twice by Freeman as stating that CA was "the epitome of American anthropology." This partial quotation is taken from Spindler's brief biographical introduction about Mead for her chapter in his edited volume, *The Making of Psychological Anthropology* (Spindler, 1978). The full sentence from which Freeman partially quotes reads as follows: "She has written books that are for many Americans the epitome of anthropology, such as *Coming of Age*, *Growing Up in New Guinea*, and *Sex and Temperament in Three Primitive Societies*" (Spindler, 1978, p. 87). Thus, Spindler was not saying that he or other anthropologists regarded CA as "the epitome of anthropology" but simply remarking that this was the case "for many Americans" regarding these 3 books, including CA.

rejected.<sup>7</sup> Most of it had diminished in importance. Mead would continue to do research and publish, but more often as a public intellectual than as a cutting-edge scholar.

Freeman's failure to systematically review the reception of CA and its assessment within anthropology has not only led him to inaccurately evaluate Mead's place in the history of anthropology, but also leaves the impression that American anthropology has hardly changed since the era of Boas, Benedict, and Mead in the 1920s and '30s. Freeman's critical brush strokes are so broad that they tarnish the most basic histories of the discipline, neglecting the complexity of anthropology's development and the progress that has been made in exploring the relationships of biology, culture, and evolution. No wonder Freeman's scholarship has lost credibility among anthropologists.

### THE CONTROVERSY AND THE MEDIA

Although Freeman's critique of Mead and his history of the discipline are deeply flawed, the same argument that has disenchanted anthropologists has attracted the media, and intelligent people have been drawn to Freeman's appeals to truth, science, and evolution. This is unfortunate because these are not the real issues involved. The truth about the controversy is more prosaic. Evolution and interactionism are old news. CA was published in 1928. Mixing partial quotations together and then adding words like "hoax" and "paradigm" may be enticing, but it does not make for good scholarship. By exaggerating Mead's theoretical place in academic anthropology, Freeman implicitly magnifies his own importance and neglects her real contributions. By relying so heavily on caricature, omission, and overstatement, Freeman, far from rescuing anthropology, has become an intellectual impediment in the way of understanding Samoa, the work of Margaret Mead, and the state of anthropology today.

The general approach that Freeman employs to promote his argument and himself was described in broad terms by Sahlins (1979) well before the Mead-Freeman controversy began. At the outset, Sahlins comments, Professor X publishes a theory despite evidence to the contrary. When scholars familiar with the evidence question Professor X's work, he denounces them "in the highest moral tones" and engages in attacks on them personally. The battle lines are drawn, and he has now become "the controversial Professor X." His ideas are discussed seriously

<sup>7</sup> After her death in 1978, the *American Anthropologist* (1980, volume 82, number 2) devoted an entire issue to an appreciation of Mead and her work. A review of the articles in this issue indicates how much Mead had given to the profession. At the same time, the articles suggest that her theories were of greater historical significance than of contemporary relevance. In a similar manner, recent anthropological research on adolescence acknowledges a historical debt to Mead, but moves well beyond her work (see, for example, Schlegel and Barry [1991] and the chapters in *Adolescence in Pacific Islands Societies* [Herd and Leavitt, 1998]). For critical assessments of Mead's career, see diLeonardo (1998) and the essays in Foerstel and Gilliam (1992).

by nonprofessionals, especially journalists. Soon he has become a familiar figure in the media where nonprofessionals and the lay public have difficulty challenging him and demonstrating the weaknesses in his argument. It is through this process that Freeman, like Professor X, has gained popular attention.

With each new version of his critique of Mead, Freeman has escalated his rhetoric and his importance in this self-made controversy. What began 17 years ago as a critique of the "Boasian paradigm" has become a critique of the "Mead paradigm." Mead's alleged "mistake" in Samoa has become her "fateful hoaxing." Freeman himself has gone from a critic of Mead to a self-styled "heretic" in pursuit of truth against the conventional wisdom of a discipline. The title of a second edition of *Margaret Mead and Samoa*, released in Australia, is now *Margaret Mead and the Heretic* (Freeman, 1996). And Freeman's use of "Paradigms in Collision" in the titles of 2 recent articles (Freeman, 1992, 1997) implies near-mythic proportions of his critique.

For Freeman, the controversy is over, and he believes that he has prevailed. Yet for most anthropologists, the controversy has been a media event and not much more. It is no longer about Samoa or theory in anthropology or even Mead's work. These worthwhile issues could be discussed had Freeman accurately quoted and contextualized his sources. But Freeman's scholarship often does not adhere to this minimum standard for academic discourse. What might have been a productive discussion about significant issues has become a custody battle over Mead's reputation. As a famous woman, liberal and now long deceased, Mead has been vulnerable in the arena of public opinion. The media have often echoed Freeman's unsupported arguments, while those academics still actively engaged in the controversy are relegated to the housekeeping task of reviewing original sources and reporting Freeman's errors, mostly to other scholars. This is hardly a productive use of time, but it is necessary, given the inability of many journalists to investigate Freeman's arguments and given that there is no sunset clause on this controversy.

### CONCLUSION

What if Freeman had been more accurate? His argument would be mundane and far less interesting. In this alternative scenario, Freeman would have concluded that Mead was a pioneering anthropologist, a cultural determinist although not an "absolute" one, who encouraged thinking about biology and evolution, but who was not in the front ranks of the profession on this issue, and whose popular book was widely read and discussed, but was hardly a "sacred text" within anthropology. He could have noted that CA did pose important questions for the general public at the time it was written in 1928 and for decades thereafter. That is why it became a popular classic, not because it was a model for professional scholarship. Although Mead and CA were enormously popular with the general public, within the profession neither she nor her book led generations of anthropologists



to embrace an antievolutionary paradigm. Freeman might have concluded that her very popularity led academic anthropologists to treat her work with caution, recognizing its limitations as well as its strengths. Of course, this is not how it turned out.

When the dust eventually settles on this protracted controversy, how will Mead and Freeman be remembered? Freeman will be remembered for his tireless critique of Mead, which has received widespread attention, in part because his misrepresentations are so boldly stated that relatively few people have bothered to check the historical record. But Freeman's contributions are limited and marred by inaccuracy.

Anthropologists will remember Mead as more than the object of Freeman's critique simply because, over the course of her long career, she did make a number of lasting contributions. In a span of 15 years between 1925 and 1939, she made 5 field trips to the South Pacific, studied 8 different cultures, and published popular and professional works on most of them. While these works, including *CA*, are not particularly important today, they became part of the foundation on which anthropologists built. Mead was also among the first anthropologists to focus on childhood, adolescence, and gender as important topics of research; today anthropologists approach these issues more wisely because of her work. Mead's efforts to bring teams of male and female anthropologists to the field, instead of individuals, improved data collection and analysis. She was among the first to use still photography and film in the field. Most importantly, Mead almost single-handedly popularized anthropology for the general public, putting the discipline on the map. And whatever her shortcomings, she did all of these things at a time when women were not expected to be professionals. It is for these reasons, and many more, that Mead deserves recognition.

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