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Derek Freeman has two related goals in this book. The first (xii-xiii) is the narrow aim of refuting Margaret Mead's ethnographic descriptions and general conclusions about Samoa and thereby discrediting her assertion that Samoa provides a "negative instance" for the universal presence of storm and stress in adolescence. His second goal is less particularistic and more relevant to general theoretical concerns.

> This book, then, while primarily given to the refutation of the general conclusion that Mead drew from her Samoan researches,

> is also concerned with examining related aspects of the wider myth of absolute cultural determinism, and with arguing that this now antiquated doctrine should be abandoned in favor of a more scientific anthropological paradigm. (xvi)

To this latter end, Freeman examines the rise of modern American anthropology. It is primarily to Boas that he attributes the blame for "absolute cultural determinism" and the supposed denial of the role of biological factors in human behavior which still, according to Freeman, characterizes "many anthropologists" (294).

These, then, are Freeman's stated goals. However, his historical analyses and theoretical discussions are so biased and inadequate that the more general framework quickly collapses to expose what this book really is--an attack on Mead that has almost no general or constructive relevance to contemporary anthropology. I begin my remarks by assessing the adequacy of his presentation of history and theory in anthropology, pause briefly to examine his scholarly methods, and finally address more specific questions and doubts pertaining to Mead and her research.

History and Theory: Boas and Absolute Cultural Determinism

Freeman is certainly correct in describing absolute cultural determinism as an "antiquated doctrine," so antiquated in fact that I doubt many take it seriously (today. While it is true that many anthropologists specialize in and stress culture more than biology in their research (and vice versa), I can think of no anthropologist who would unequivocally deny that human behavior is the result of an extremely complex interplay between both spheres.[1] Freeman cites no contemporary examples and is tilting at nonexistent windmills. He never grapples with the crucial problem of explaining behavioral diversity between populations without at least recognizing a primacy for cultural factors.

By denying us an ethnography, Freeman denies us the context(s) in which his facts and data exist. Since most of the evidence he offers is itself cultural and not biological (a striking fact discussed below), he presents us with cultural facts out of context, and such data are easily distorted and sometimes quite meaningless. He would have come closer to achieving his aim of correcting what he perceives to be Mead's inaccurate picture of Samoan life if he had presented us with an accurate one.

Freeman's entire argument rests on his assumption that in refuting Mead's negative instance, he demolishes forever the idea that adolescence is not necessarily a period of stress. Nowhere does he consider the possi-

bility that other negative instances might have been put forward since 1928; it is as if his ethnographic clock had stopped over fifty years ago and nothing had been published since. In fact, the ethnographic literature is full of examples of societies in which adolescence is not what it was in the 1920s in the United States, including many in which stress is not a necessary concomitant. [2] Further, Freeman himself cites Katchadourian, from a book entitled *The Biology of Adolescence,* who said that "research on ordinary adolescents has generally failed to substantiate claims of the inevitability and universality of adolescent stress" (Katchadourian in Freeman: 255).

Freeman clearly calls for a "more scientific anthropological paradigm" than absolute cultural determinism, and surely no one can fail to be in sympathy with this goal of recognizing the interaction between cultural and biological factors in understanding human behavior. However, his call is hollow for three reasons. First, although the refutation of Mead's data does not logically require that Freeman present biological evidence, it is highly significant that he himself presents predominantly cultural data. Second, his final conclusions seem to be mere lip service to the idea of cultural-biological interaction in human behavior--facile generalizations without discussion or extended example. Third, he ignores the existence of biological anthropology in the United States, both its history and contemporary liveliness. I will amplify each of these statements before going on to explore his version of the history of anthropology.

Freeman's own data are overwhelmingly cultural--in one case so cultural that the mind boggles. [3] He continually asserts that Samoans are not as Mead portrayed them (they have a different ethos, character, etc.), and these differences are the basis for his refutation of Mead. What is ironic is an implicit contradiction Freeman fails to address or even to understand. Why, according to Freeman, are the Samoans not gentle and unassertive as Mead described them? It is not because of biological factors, as one might suspect; it is because of Samoan childrearing practices! Freeman does not seem to realize that his basic approach to the issue of why Samoans are as they are is completely consistent with Mead's major tenets. Freeman is unambiguous: 'Samoan character . . . is very much the product of the way in which discipline is imposed upon young children" (216). He also states that "this behavior, in ethological terms, is a form of redirected aggression, and its prevalence among Samoans is evidence of the tension generated within individuals by the mode of discipline imposed upon them from childhood onward" (219). This second quote is especially interesting because it indicates an attempt to incorporate biological ("ethological") considerations. The behaviors he refers to here are "manner-

isms" such as "the agitated moving of the fingers of the hand in states of frustration. . ." (219). But instead of linking the behavior firmly to any kind of biological factor, he clearly terms it the end result of childrearing, or "mode of discipline."

Freeman does not provide any significant examples of the biological factors that influence human behavior, or how they seriously interact with cultural ones. He does attempt to incorporate biological evidence for aggression, but fails to make the point because he still asserts that childrearing patterns are the genesis of Samoan character.

> Within Samoan society there is very frequent resort to punishment, and I would argue that it is in particular a pervasive dependence on the physical punishment of children that makes Samoans so disturbingly prone to interpersonal aggression. [Several studies] . . . have clearly shown that punishment enhances rather than inhibits the expression of aggression. And this conclusion has been corroborated by D. D. Woodman's finding that physical punishment is allied to aggression outside the home. Woodman's researches also suggest a biochemical component in interpersonal aggression, with an increase in noradrenaline being linked with increasing aggression in personality. It seems likely that it is the regime of physical punishment, and especially of children, that generates the "air of violence on a tight rein" reported by Mackenzie, and that results in Samoans flying "from feathers to iron" at the slightest provocation, to engage in the physical violence that they have come to accept as customary. (275)

The first section of the book is an examination of what Freeman perceives to be the rise of the paradigm of absolute cultural determinism, and the second is a point-by-point refutation of Mead. Only in the last chapter does he finally directly address the interaction of biology and culture. Readers who expect a synthesis with significant exemplary examples will be disappointed.

> We have before us then, a view of human evolution in which *the genetic and exogenetic are distinct but interacting parts of a single system.* If the working of this system is to be comprehended it is imperative . . . that a clear distinction be made between the genetic and the cultural, for only in this way is it possible to understand "the causes and mechanisms of change in

> any organism capable of both cultural and genetical change." This requirement, furthermore, holds not only for the study of mote evolutionary history of the human species, but equally for the analysis and interpretation of cultural behavior in recent historical settings. In other words, specific cultural behaviors, to be understood adequately, need to be related to the phylogenetically given impulses in reference to which they have been evolved, and in apposition to which they survive as shared modes of socially inherited adaptation. (299-300; emphasis in original)

I have quoted this passage at length because it appears to be Freeman's final conclusion although it receives minimal discussion. He does go on to cite a brief example involving Samoan respect language. He argues that this elaborate convention developed as a way to "avoid potentially damaging situations" (300) in which there might be "an extremely rapid regression from conventional to impulsive behavior" (300).[4] (Incidentally, this example, a short one that demeans the complexities of Samoan respect language, is not the only place where Freeman seems to take a Hobbesian view of human nature with more than a dash of Malinowski added.) The example fails to illustrate the point because we see a "cultural convention" (300) responding to presumably innate aggressive impulses--we do not see a complex interaction, only a cultural response to supposed biology.

> It is thus evident that if we are to understand the Samoan respect language, which is central to their culture, we must relate it to the disruptive emotions generated by the tensions of social dominance and rank, *with which this special language has been developed to deal.* (301; emphasis added)

It is important to note two things about these passages. After presenting an example purporting to support the basic theoretical stance of the first, he associates the respect language not with biology at all but with social dominance and rank, which are cultural conventions themselves (he does not try to associate Samoan hierarchy with pecking orders and biological manifestations of ranking). Second, he begins with the interaction of biology and culture but subtly shifts away from interaction to understanding behaviors as evolutionary responses to "phylogentically given impulses" and as things that exist "in apposition" (not in interaction). There is no interaction between biology and culture here at all. By adopting this ap-

proach, Freeman cannot help but find himself (as others have before) without a theoretical means of accounting for the diversity of behavior apparent between human groups.

Freeman was trained at Cambridge and therefore is unfamiliar with American physical or biological anthropology. However, he blames Mead's errors on her adoption of the anthropological paradigm generated by Boas, and his theoretical attack is predominantly against Boas and other early American anthropologists (Mead, Kroeber, Lowie, Benedict). Boas was certainly the prime mover in establishing the importance of culture, but it is odd that Freeman's attack is almost exclusively on American anthropology, which even today maintains basically a four-field approach, which considers biological anthropology essential to the discipline and in which the interaction between biology and culture has always been important.[5] Nowhere does Freeman even mention the existence of biological anthropology--it is as if it did not exist. Considering the reading he did on the history of the discipline, there is no excuse for ignoring the fact that biological factors have always played a role in American anthropology. He could have cited numerous excellent studies to support his position concerning the importance of biological factors, yet curiously he does not. If he had, I think two things would have happened: (1) his call for the new paradigm would have looked foolish because many anthropologists are already doing this kind of research and have been for some time; and (2) he would have been forced to recognize the significant and positive role of Boas himself in establishing the importance of biology in the discipline.

Freeman argues strongly that Boas was an "absolute cultural determinist" but this portrayal of Boas does not adequately represent the complexity of Boas' thinking. Freeman (47, 282) accuses Mead, especially, of "suppressing" evidence contradictory to her main ideas[6] and yet it is Freeman himself who suppresses and distorts, who fails to present all of the significant information concerning Boas and this "absolute cultural determinism." As he seems to have consulted most of the relevant sources, the picture he gives surely involves a conscious manipulation to bolster the weak theoretical framework of the book. The simple truth is that although Boas stressed cultural factors, he was far from being an absolute cultural determinist. Boas' foremost concern was for rigorous science. He himself conducted research on biological factors, especially those affecting human growth, as well as culture. To some extent his own work documented the interaction between heredity and environment despite the fact that he remained skeptical that biology would ever help explain historical process or cultural diversity.

Freeman's discussion begins, as it must, with a consideration of events in biology during the nineteenth and twentieth centuries. He writes at length about Galton and the eugenics movement in the early chapters and makes it clear that Boas reacted against the racist nature of this opposing paradigm. Freeman states that early in his career Boas called for cooperation between biology and anthropology, but argues that Boas later became more extreme and denied the relevance of biology altogether (e.g., p. 5). There are enough half-truths in this presentation to entice an unwary reader into believing that it is the whole picture, but it is not.

In reality, Boas made significant contributions to physical anthropology which were recognized by his contemporaries as well as later anthropologists (Boas' pioneering; work in biological anthropology is still being cited; see, for example, Frisch 1975; Johnston, Borden, and MacVan 1975). Hrdlicka (1919:102), a proponent of eugenics and hardly free of racism himself, "regretted" that Boas did not devote all of his time to physical anthropology and wrote that "the published contributions of Professor Boas to physical anthropology are both *numerous as well as important.* They cover a wide range and in general are characterized by a distinct leaning towards a mathematical rather than anatomical treatment of the subject matter" (1919:102; emphasis added). (Others have commented on Boas' statistical contributions to biological anthropology; see, for example, Harris 1968:317.) There follows a list 'of publications in physical anthropology by Boas that Hrdlicka himself thought significant and important, and although the list ends in 1916, it numbers sixty-five entries (102-105).

Krogman, an eminent physical anthropologist, also recognized Boas' significant contributions to biological anthropology, especially in his own field of human growth and development:

> his early work in growth and development was not only well known to me, but it was inherent in Todd's growth-research design as well. Todd felt that Boas's work in the field of growth was incisively innovative. I have never surrendered my opinion that Boas was the "compleat anthropologist". . . . (1976:6)

Krogman summarizes Boas' contributions to physical anthropology by saying that Boas "stresses three major themes: 1. the nature of the physical and behavioral differences between people (races); 2. the physical growth and development of the child; 3. the role of biometrics in areas 1 and 2" (7). Krogman goes on to list Boas as one of the "giants" (11).[7]

Before looking at Boas himself, it is beneficial to examine a contemporary theorist for his perspective on Boas. Marvin Harris, a strong proponent of cultural materialism, is not in accord with any kind of absolute cultural determinism of the kind Freeman attributes to Boas, and he is critical of Boas because Boas never constructed a coherent theoretical framework, was too particularistic, and did not adopt a materialistic approach. However, Harris' interpretation of Boas differs radically from Freeman's. First, he recognizes "Boas' renaissancelike involvement with all four fields of anthropology . . ." (1968:255),[8] and gives Boas substantial credit for having higher scientific standards than his contemporaries (253ff.). Harris firmly maintains that Boas was not an absolute cultural determinist but an ardent advocate of including all potential and possible explanatory factors. Thus he states that "Boas systematically rejected almost every conceivable form of cultural determinism" (283) and refers to Boas' rejection of all monocausal determinisms (284). Harris criticizes Boas because he "rejected *all* coherent (i.e., noneclectic) explanations of sociocultural differences that made any appeal to any deterministic principle whatsoever, with rather marked indifference to whether they were inspired by materialist, idealist, or theistic doctrines" (296; emphasis in original). Harris is quite specific about Boas and evolution: "Boas did not reject evolutionism in any degree whatsoever. What he rejected was (1) biological reductionism; (2) cultural parallelism; and (3) universal standards of progress" (295). Harris certainly does not agree that Boas was an absolute cultural determinist, and yet that is Freeman's main point. Freeman cites Harris' book in other contexts but never mentions these ideas; his interpretation of Boas is clearly at odds with that of Hrdlicka, Krogman, and Harris. In order to evaluate which interpretation of Boas is more appropriate, we must now turn to the words of Boas himself.[9]

As mentioned earlier., Freeman does give Boas credit for initially calling for cooperation between biology and anthropology, but claims that Boas later changed in response to the extremes of the eugenicists:

> in December 1907, Boas had given it as his view that a separation of anthropological methods from the methods of biology and psychology was impossible, and then gone on to express the hope that "the safe methods of biological and psychological anthropometry and anthropology" would help to remove the problems of "race-mixture" and eugenics from heated political discussion and make them subjects of calm scientific investigation. By 1916, however, his attitude had decisively changed. During the intervening years the eugenics movement had effloresced into a

pseudo-scientific cult, and Boas had come to see both eugenics and the racial interpretation of history as irremediably dangerous. The extreme doctrines of the hereditarians, Boas pointed out, had set anthropologists and biologists at odds, and so much so that a "parting of the ways" had been reached. (5)

It is important to note here that Boas' reaction was to the extreme deterministic position and racist implications of the eugenics movement, not to biology in general. Freeman continues:

> These were portentous words. Within the space of a few months, two of the most able and active of Boas' former students, Alfred Kroeber and Robert Lowie, had published intellectual manifestos that conceptually dissociated cultural anthropology from biology. Their solution was the propounding of a doctrine of absolute cultural determinism that totally excluded biological variables. (6)

Apart from the fact that it was Kroeber and Lowie to whom Freeman refers here, his later argument rests on the assertion that this conversion by Boas to absolute cultural determinism was extreme and profound. However, there are significant problems with Freeman's interpretation: (1) he fails to distinguish among several central concepts, such as biological determinism, biological influences, individual innate characteristics, racism, and evolution, despite the fact that the differences among these are not very subtle; (2) he ignores what Boas meant by environment; (3) he exaggerates Boas' position and bolsters his extreme interpretation with quotes wildly out of context.

That Boas was an ardent opponent of biological determinism and racism (two central elements in the eugenics stance), no one can doubt. But Freeman seems to believe that this is the same thing as opposing the inclusion of all biological factors, and it is not. Boas was consistent throughout his life in calling for scientific research on the role of both biological factors and cultural influences, and the interaction between the two; it was his attack on biological determinism and racism that intensified. That Boas was not just paying lip service to an ideal is evident from his own research in physical anthropology. In his classic study, "Changes in Bodily Form of Descendants of Immigrants" (1940:60-75), first published in 1912,[10] Boas documented physical changes within a generation or two and rightly concluded that hereditary factors played little role. However, he did not advance the notion that explaining these changes was a cultural

problem: "These observations, however, merely set us a biological problem that can be solved only by biological methods. No statistics will tell us what may be the disturbing elements in intra-uterine or later growth that result in changes of form" (1940:70). He underscored the biological nature of the problem again in a follow-up article published in 1916 (1940:76-81). In 1913, he published a paper entitled "Remarks on the Anthropological Study of Children" (1940:94-102) in which he compared the development of racial traits among children and concluded that "here we have undoubtedly traits that are determined by a long line of ancestors, not by environment" (102).

Admittedly, these studies are all before the supposed conversion to absolute cultural determinism took place, but they are not uncharacteristic of Boas' later work. Two articles first published in 1935 provide conclusive evidence of Boas' eminently reasonable position and open mind. The first, "Conditions Controlling the Tempo of Development and Decay" (1940:89-93) investigated factors affecting the human life span. Boas argued here that *both* heredity and environment were important factors. He cautions that "the importance of hereditary determinants may not be neglected" (92) and concludes that "each individual has by heredity a certain tempo of development that may be modified by outer conditions" (93). (What these "outer conditions" are concerns his conception of environment, described below.) Finally, in a classic study still cited (see, for example, Frisch 1975) on "The Tempo of Growth of Fraternities" (1940:86-88), also first published in 1935, Boas investigated the rate of growth of children and. concluded, "since the conditions under which these children live are unusually uniform, we may conclude that proof for the heredity of the tempo of growth has been given" (87). Thus based on his own research Boas concluded that the rate of growth was determined by heredity, not environment. Although Boas did not look to biology for an explanation of cultural and historical processes, Freeman's assertion that Boas was not "disposed to explore, in a constructive way, the coexistence and interaction of genetic and exogenetic processes" (32) is simply wrong.

Freeman ignores these contributions of Boas. Instead, he takes Boas' opposition to racism and biological determinism as evidence that Boas was opposed to all consideration of biological or hereditary factors, and even incorporates evolution. Freeman is often tricky, discussing Boas' antipathy to biological determinism or reductionism in one sentence but sliding into his total opposition to biology in the next. Sometimes this jump is obvious. For example:

> the theories of the doctrine of cultural determinism were (in Melford Spiro's words) "developed in the first instance as alternatives to and refutations of *biological determinism.*" *Thus* the Boasians had an antipathy to *biology,* and to *genetics* and *evolutionary biology* in particular. (295; emphasis added)

In other instances, however, the leap between opposition to biological determinism and a denial of biology altogether is more subtle and deceptive. A longer example is warranted here because Freeman relies on this kind of specious reasoning frequently. On page 32, he presents Boas' views fairly accurately:

> Boas went on to examine the assumption that "racial descent determines cultural life" and to conclude that not the slightest successful attempt had been made "to establish causes for the behavior of a people other than historical and social conditions." An unbiased review of the facts, he asserted, showed that "belief in hereditary racial characteristics and the jealous care for purity of race is based on the assumption of non-existing conditions." . . . The whole thrust of Boas' thought, as Stocking has observed, was "to distinguish the concepts of race and culture, to separate biological and cultural heredity, to focus attention on cultural process, to free the concept of culture from its heritage of evolutionary and racial assumption, so that it could subsequently become . . . completely independent of biological determinism. (32; first ellipsis added; second in original)

Note here that Stocking says nothing about denying biology, only biological determinism and racism.[11] Freeman goes on in the next paragraph to describe briefly the rise and basic tenets of the eugenicists (e.g., Davenport) and the "hereditarian" cause, concluding

> there, then, in 1911 [not 1916], were two antithetical intellectual and scientific schools--that of Boas and that of Davenport--with neither disposed to explore, in a constructive way, the coexistence and interaction of genetic and exogenetic processes. (32)

The leap between his (and Stocking's) description of Boas and this conclusion is simply unwarranted.[12]

Boas and his students did deny biological determinism and argue against racism, but Freeman fails to understand that in doing so they were

not denying hereditary influences nor did they reject innate individual differences. In fact, both Mead (1963:292) and Benedict (Mead 1972:195) suggested that innate individual characteristics could partially explain deviance from cultural norms. [13] Kroeber wrote that

> often they [biology and culture] are even intertwined in one and the same phenomenon, as when a person is born with hereditary musical capacity and develops this further by study and training. They are not always easy to disentangle; but they must be separated if the processes at work are to be understood. (1948:2)

Neither Boas nor his students completely denied that individuals were affected by heredity. They argued that biological determinism could not explain sociocultural and historical processes. [14]

An appreciation of what Boas meant by environment is important in understanding his work, especially in relation to Freeman's claim that Boas believed that "environment has an important effect upon the anatomical structure and physiological functions of man" (28). That Boas was naive in today's terms about evolution, genetics, and Lamarckianism is true, but in failing to describe Boas' inclusive concept of environment, Freeman warps Boas' position out of proportion. For Boas, environment was not just social or cultural but also had physical, geographical, and biological aspects. As the environmental conditions that affect growth and physical form and function, Boas lists malnutrition and pathology and disease (1940:36) and "habitual uses to which groups of muscles are put" (1940:370). He includes the prenatal environment as well: "constitutional changes in the body of the mother may bring about modifications in prenatal growth. . ." (1940:37). In his article on the life span, he elaborates:

> even here certain allowances have to be made, for we may distinguish between an hereditary, purely biologically determined element and another one that depends upon conditions of life. Ample or deficient nutrition, more or less exhausting daily labor, abuse of the body, greater or lesser nervous strains are elements that modify the life span as it may be determined by heredity. Even geographical conditions may have their influence. (1940:89)

This is a far cry from the position of absolute cultural determinism Freeman claims for Boas in regard to the effects of environment over heredity, and although in some ways Boas was behind his time in relation to biology, it is clear that in others he was ahead.

Freeman's picture of Boas is distorted despite the fact that it contains partial truths. One of the most flawed aspects of his whole presentation is the ease with which he takes quotes out of context and presents them as complete and unambiguous truths, with no concern for even the most blatant misrepresentations. In one astounding case, Freeman takes Boas' words and uses them in almost complete opposition to their original meaning. This travesty of scholarship occurs on page 295 where Freeman says:

> Boas, for example, was opposed to research in human genetics and thought, even as late as 1939, that in respect of the human body, "a search for genes would not be advisable," there being some danger that the number of genes would "depend rather upon the number of investigators than upon their actual existence."

This is indeed a remarkable passage. First, Boas never opposed true scientific research on human heredity. The quotes from Boas do not come from the 1939 article mentioned but incredibly from the classic article in which Boas asserted that the tempo of growth was determined by heredity. What Boas argued here was not that genetic factors were irrelevant (in fact, his whole point was just the opposite) but that the whole phenomenon was very complicated and not likely to be governed by any single or simple set of genetic factors. Here is what Boas really said, and a comparison of his words with Freeman's distortion well illustrates my point about selective and decontextualized quotes:

> It is obvious that a phenomenon of such complexity as length of body and tempo of development must be governed by many hereditary factors and that we are dealing with a phenomenon of general organization of the body and that a search for genes would not be advisable. Is not there some danger anyway, that the number of genes will depend rather upon the number of investigators than upon their actual existence? (Boas 1940:88; originally published 1935)

What Freeman did here is so obvious that further comment is unnecessary. It is important to stress that this kind of distortion is common in the book, that this example is not an isolated one.[15]

Although space does not permit more extensive discussion, it is necessary to mention that those scholars Freeman labels "the Boasians" (Kroe-

ber, Lowie, Benedict, Mead) receive the same treatment in his hands. He never considers the complexities of their thought, giving only one-sided pictures, and presenting their ideas out of context. He lumps them together and seems to imply that they (and Boas) were very much alike in all ways (e.g., if Kroeber said something, it must have come from Boas). Again, there are enough partial truths to entice an unwary reader into believing in the veracity of the whole picture.[16] Freeman fails to stress that they too were reacting to eugenics and racism and trying to provide an alternative theoretical explanation for behavioral differences between human populations.

On Scholarship

Before turning to the real heart of this book, the attack on Mead, I want to pause to discuss its scholarly style. The book's citation form is unprofessional and surely has caused faces to redden among editors at Harvard Univerity Press (and if it hasn't, it should have). There is no bibliography or list of references cited. Nowhere can one learn the name of the publisher of a book cited. Inclusive page numbers are not given for articles in journals or chapters in books. Sometimes the specific page number for a quote is not precisely indicated at all, the note merely saying that material was taken from some page and "ff."[17] When Freeman uses the same quotation more than once, especially phrases, he frequently does not repeat the reference; this is acceptable as long as they are in close proximity but unacceptable when they are separated by as much as one or two entire chapters.[18] Works mentioned by name or author in the text are sometimes not cited in the note at all (and readers who flip between text and notes will be surprised at how often he mentions *Corning of Age in Samoa* but cites some other work).[19] Much that should be footnoted is not.[20] Freeman frequently deletes words and phrases from direct quotations without indicating to the reader with ellipses that he has done so.[21] He walks across the line between adequate citation or paraphrasing and appropriating the words of others as his own.[22] His notes sometimes contain references to specific pages that are not relevant to the text or do not contain the quotes used.[23] In sum, he violates almost every canon of good scholarship.

Most frustrating is that in almost all cases Freeman provides a single note listing all of the references pertaining to an entire paragraph. Thus although sometimes it is clear where he is getting his material, it frequently is not. He strings together several quotes from a variety of sources (not only in the same paragraph but also in the same sentence) and pro-

vides a single note which can include as many as a dozen or more different sources; and these references are not necessarily in the order relevant to their use in the text. It is frequently impossible for a reader to be certain what came from where, or who said what, even after looking up the note. Here is one typical example that illustrates the problem well:

> elsewhere in her writings, Mead elaborates this picture of the background that, for Samoans, "makes growing up so easy," the leitmotif of her depiction being the notion of *ease.* Samoan life, she claims, is above all else "characterized by ease"; Samoan society is "replete with easy solution for all conflicts." She remarks, for example, on "the ease with which personality differences can be adjusted by change of residence," on "the easy acceptance of innovation," and on a prevailing "ease in sex relations." Adolescence is "the age of maximum ease," and Samoans develop into "easy, balanced human beings" in a society that "emphasizes a graceful, easy, diffuse emotional life, a relaxed dependence upon reliable social forms." (84; emphasis in original)

Putting these quotes together to make a point is certainly acceptable, but lumping all of the sources together is not. Note that there are nine quotations in this paragraph, and a reader who does not turn to the single note will never know that they come from five different 'sources (published, of course, at different times). The note cites seven different specific page numbers, so one must assume that some (which?) of these quotes occur together. The most significant thing here is that even after referring to the note, the reader still has absolutely no way of knowing which passage was taken from which source. [24]

I chose the above example because it is relatively straightforward (and short) and clearly shows the inherent difficulties of the style used. [25] These quotes were at least all from Mead; however, the style also lends itself to misuse in Freeman's hands because it allows him to obscure the author of particular quotes. For example, on page 20 there is a paragraph about Boas and the "relationship between culture and biology." The note pertaining to the paragraph (which appears on page 308) lists only two sources, one by Boas and a secondary source by Spier. There is one quotation in the paragraph: is it from Boas or Spier? It is impossible for the reader to know.

One of the worst aspects of this style is that it does not allow the reader to differentiate what Freeman says from what his sources say. For ex-

ample, the paragraph that begins on page 99 and continues on page 100 has only one source cited--Stocking (1968). It begins,

> as George Stocking has shown, "the working out of all the anti-biological [sic] tendencies in behavioral science and the complete dissemination of Boasian thinking were not accomplished until after 1930." In this working out, such as it was, Mead's assertion of the absolute sovereignty of culture, in answer to the problem that Boas had sent her to Samoa to investigate, was of quite pivotal importance. (99)

Freeman's paragraph goes on about the significance of Mead's research, and one might think that: it was Stocking who argued that Mead's Samoan research was "of quite pivotal importance." In fact, Stocking suggests no such thing; nowhere in the book does he discuss Mead's importance. Stocking's next sentence even denies Freeman's point about the significance of Mead's work, since most of the separation between the social and biological sciences had occurred before Mead even went to Samoa.

> In short, the working out of all the antibiological tendencies in behavioral science and the complete dissemination of Boasian thinking were not accomplished until after 1930. Nevertheless, as Kroeber implied in retrospect, the emancipation of the social from the biological sciences, in principle if not in all areas of practice, had been accomplished by 1917. (Stocking 1968; 267)

A reader who does not go beyond the note to the original source here might easily conclude that Stocking implied or said things which he simply did not.[26]

Although most of the notes refer to an entire paragraph, some appear internally within a paragraph. A few of these contain additional information (not references) and thus seem to make sense. However, Freeman is not consistent: others contain only references, and often additional information is merely included in the encompassing paragraph note. This is an unnecessary source of added confusion.

In the above discussion of Freeman's treatment of Boas, I mentioned that he took quotes out ad context, using them without regard for their intended meaning. This kind of scholarship, aided by the ambiguous citation style, characterizes the entire book. Some of the cases are serious misrepresentations while others involve subtle changes of tone and tenor.[27] One of the more deceptive ones occurs on page 282 where Freeman writes:

> it was of this attitude of mind [cultural determinism] that Mead became a leading proponent, with (as Marvin Harris has observed) her anthropological mission, set for her by Boas, being to defeat the notion of a "panhuman hereditary human nature." She pursued this objective by tirelessly stressing, in publication after publication, "the absence of maturational regularities."

The first thing to note here is that both quotations come from Harris; Freeman never cites Mead herself (much less in "publication after publication") as saying that there are no "maturational regularities." These are the words of Harris writing about Mead. It is also critical to note the context in which Harris was writing: Mead's relationship to orthodox Freudianism.[28] After outlining Freud's basic "biopsychological" approach, Harris goes on to say that

> everything in Mead's approach weighed against such compulsory psychic freight. It was her mission, set for her by Boas, to defeat the notion of a narrowly fixed racial or panhuman hereditary human nature. And it was for this reason that she tirelessly stressed the absence of maturational regularities: adolescence is not always a time of stress; children are not necessarily more imaginative than adults; women are not necessarily more passive than men. (Harris 1968:427)

(Note here again that Freeman uses Harris' words without including them as quotations.)

Far more examples of the unacceptable scholarship in this book could be provided (more are included below), but surely these are sufficient to induce readers to treat the work with caution. Freeman's treatment of the history of anthropology, especially Boas and the culture-biology issue, is riddled with half-truths and distorted arguments and should not be taken seriously.

Freeman, Mead, and Samoa

It is neither my intention nor my role to defend Mead against Freeman's attack. There is no doubt that she made some mistakes, especially in interpreting the data she recorded.[29] But Freeman's book is a profound disappointment because anthropology needs professional, unprejudiced, and critical analyses of Mead and her contemporaries, things this book does not provide.[30] Mead was not 100 percent right but neither, as Free-

man would have us believe, was she 100 percent wrong. We get only a one-sided view from Freeman, a view that does not do justice to Mead, to Samoa, and ironically enough to Freeman's own work. That his data on Samoa do not totally agree with hers is not simply a case of Freeman being right (because he considers biological influences or whatever) and Mead being wrong (because she is a cultural determinist or whatever). Specifically, in this section I will discuss five major points: (1) there are significant questions about the comparability of the data, questions that Freeman glosses over; (2) there are serious flaws in his own methodology and reasoning; (3) he does to Mead what he did to Boas, that is, quotes her out of context, ignores evidence that contradicts his position, and exaggerates; (4) he fails to see that Samoans are neither precisely as Mead portrayed them nor exactly as he pictures them, but both; (5) in those instances where Mead was wrong, he suggests simple and naive reasons for her errors, whereas in reality the causes were more profound (and have little if anything to do with a dichotomy between culture and biology).

Mead studied a village in American Samoa in 1925-1926 while Freeman did research in Western Samoa beginning in the 1940s. [31] One cannot help but wonder about the comparability of data gathered from different places at different times That Western Samoa and American Samoa have had different colonial histories is obvious, and surely there was at least some sociocultural change between the 1920s and the 1940s. Freeman is not troubled by this issue. Informants assured him (xiv) that things were the same in both places, not only where he was there but in the 1920s as well: "As one of my informants remarked [in 1967], the happenings of the mid 1920s were still fresh in their memories" (xv). "Fresh" memories after more than forty years are cause for skepticism, but Freeman has none. However he does cite other sources for the cultural and historical uniformity throughout Samoa.

> As George Turner notes [writing in 1861 (Freeman 327)], the Samoans have but one dialect and have long been in free communication from island to island; in Bradd Shore's words, "culturally and linguistically, the entire Samoan archipelago reveals a remarkably unified identity and striking homogeneity" (117).

He continues on the same page with a statement from a Samoan chief to a United States Congressional Investigation Commission in 1930, a statement that stressed the uniformity. [32] But he does not consider that linguistic and cultural identity do not necessarily mean complete social unifor-

mity. Shore (1983:3) indeed says that Samoa is characterized by uniformity, but he also underscores the difference between cultural uniformity and social diversity:

> the nature of this homogeneity and the variations within it is an important issue in understanding Samoa. By cultural homogeneity, I refer to a shared commitment to a large number of political and kinship institutions, a common consciousness among Samoans of . . . a set of understandings and categories which serve as common premises for interpreting and orienting behavior. This sort of homogeneity does not preclude wide divergences in specific practices and beliefs between villages, members of different descent groups, and different individuals, or within the same individual on two different occasions. (1983:303)

(See also Shore 1983:128.) Furthermore, in Shore's excellent analysis the fact that the community he studied possessed an unusual title configuration for Samoa had significant explanatory power. He also notes that "different degrees of colonization" (3) had occurred and that Manu'a (where Mead worked) historically had an important degree of political autonomy from the rest of Samoa (4). Moreover, Freeman himself cites a source that contradicts his point--Manu'ans were different from the rest of Samoa in the fierceness of their warfare (Freeman 169). In sum, although Samoa does exhibit cultural and linguistic uniformity, Freeman does not adequately establish that his data are completely comparable to those of Mead; there is ample room for doubt and he should have addressed the issue more carefully.

Freeman's own reasoning and methodology are not always rigorous and precise. In his fervor he fails to make significant analytical distinctions (just as he did when he neglected to note the differences between biological determinism, racism, evolution, and a denial of the relevance of biology altogether). His analysis contains subtle shifts in which he begins discussing one thing and finishes with a conclusion about something else. The following passage, which contains an implicit and unwarranted assumption that adolescent stress and delinquency are somehow the same, aptly illustrates this problem.

> Is it in fact true, as Mead claimed, that the behavior of Samoan adolescents is untroubled and unstressed and lacks the conflicts that are so often characteristic of this period of development? As Herant Katchadourian notes, "research on ordinary adolescents

> has generally failed to substantiate claims of the inevitability and universality of adolescent stress." Nonetheless, the findings . . . [of several investigators] have clearly shown that the years of adolescence are hazardous for many, with delinquency in the United States and elsewhere reaching a peak at about age 16. To what extent, then, is adolescent delinquency present in Samoa? (255)

(Note here Freeman's flagrant disregard for the conclusion of his own sources.)

The problem of distinguishing between statements of cultural ideal and the reality of human behavior is also prevalent in this book. Freeman cites rules and verbalized ideals as evidence that Mead's descriptions of behavior were totally in error (e.g., p. 188). He is especially prone to citing the moral lessons of myth and legend to substantiate his points without providing behavioral evidence (e.g., pp. 182-83, 191-92, 235). Surely the difference between the ideal and the actuality has at least some relevance to the issue of premarital sex in traditional Samoa. Mead reported that it was common and expected, but Freeman offers evidence for the reverse. He seems to be correct that it was not ideal and approved behavior, but that does not necessarily mean that it did not occur, a possibility he never seriously considers. In fact, despite her interpretation, Mead herself provides some clues that this might be the case. She described love affairs as "hazardous adventures" (1961:61), "clandestine" (77), and "strictly *sub rosa*" (80). She also wrote that conditions were very different in traditional Samoa: "aboriginal Samoa was harder on the girl sex delinquent than is present-day Samoa" (199).

> Deviations from chastity were formerly punished in the case of girls by a very severe beating and a stigmatising shaving of the head. Missionaries have discouraged the beating and head shaving, but failed to substitute as forceful an inducement to circumspect conduct. The girl whose sex activities are frowned upon by her family is in a far better position than that of her great-grandmother. (Mead 1961:200) [33]

Freeman himself cites a source that claims that "virginity was 'a social asset rather than a moral virtue' " (230), and notes that although virginity was valued by all, it was a value "especially characteristic of the higher levels of the rank structure. . ." (236). He also says that "in many cases the defloration that precedes an avaga [elopement] is the culmination of a seduction that the girl herself has actively encouraged" (240). Neither does

Freeman recognize that his emphasis on a Samoan "cult of virginity" [34] (e.g., pp. 234, 239) and a "puritanical Christian sexual morality" (239) is not completely and wholly consistent with his assertion that "while in all the Samoan communities I have studied a few girls remained virgins until they married in a religious ceremony, most of them lost the status of virgin by eloping from their families with the man who succeeded in deflowering them" (240).

Freeman's own methodology is frequently questionable. He presents a table in which he claims to report the incidence of virginity among adolescent girls in one village (239). Ages ranged from fourteen (100 percent virgins in a population of four) to nineteen (40 percent virgins in a population of five), with a total of thirty of forty-one girls (73 percent) who were virgins. He never quite tells us how he gathered these data, however. In the text he says that "we collected information on whether these girls and young women were virgins and whether they were members of the Ekalesia [communicants in the church]" (239). In the note that pertains to this paragraph, it is suggested that membership in the Ekalesia (which theoretically required that unmarried girls be virgins) was his prime determining factor: "Membership in the Ekalesia by an unmarried adolescent girl is based on acceptance by other members, who exercise a very strict surveillance in this matter, that she is a virgin. The classing of a girl as a virgin is based on this and all other available relevant evidence" (350). He never specifies what "all other available relevant evidence" might be. In a matter as sensitive as virginity in contemporary Samoa, these data are clearly unreliable and Freeman is revealed as very naive. On page 290 he notes that "because of their strict morality, Samoans show a decided reluctance to discuss sexual matters with outsiders or those in authority, a reticence that is especially marked among female adolescents:" He goes on to question how Mead could have gotten her data, given this reticence, but never really answers the question of how he himself overcame it and acquired data on virginity. [35] After presenting the table, he goes on in the next paragraph (239) to calculate age at first conception, as if to suggest that these data corroborate his material on virginity. However, if Freeman really knew his biology, he would surely have mentioned that the phenomenon of adolescent infertility might have some relevance here.

In an appendix to *Coming of Age in Samoa* (1961:190) Mead reminds her readers that the numerical data she gathered were not suitable for quantitative analysis; [36] but Freeman has no such qualms about his or her data, and the result in one case is absurd. He correctly reports (237-38) that in Mead's sample of twenty-five adolescent girls, thirteen were vir-

gins. He concludes by saying that "this situation . . . is obviously incongruent with her generalizations about Samoan female adolescents: more than half of the adolescent girls about whom she wrote in *Coming of Age in Samoa* were in fact virgins. . ." (238). He is right--thirteen out of twenty-five is certainly "more than half"; but it is also true that almost half (twelve out of twenty-five) were not virgins. One could easily ignore Mead's warning about performing quantitative operations on these data and do all sorts of interesting things with them. For example, of the eleven girls in the sample (Mead 1961:209) who were three or more years past puberty, eight of them (a full 72 percent) were not virgins.

This is not the end of Freeman's statistical shenanigans with Mead's data, for from an even smaller corpus of material (still based on these twenty-five adolescent girls) he extracts rates of juvenile delinquency. He analyzes the qualitative data Mead presents on deviance and goes on to suggest that "if we assume, conservatively, on the basis of Mead's reports, that among the twenty-five adolescents she studied there was *one* delinquent act per annum, this is equivalent to a rate of forty such acts per thousand" (257; emphasis in original). Then he ignores the definitional differences by which the two sets of data were generated and compares this rate with juvenile delinquency rates in England and Wales. He concludes that delinquency among Samoan girls in 1928 was "about ten times higher than that which existed among female adolescents in England and Wales in 1965" (258). He recognizes that "this comparison is only approximate" (258) but nevertheless immediately asserts that "it does, however, indicate that among the girls studied by Mead in 1925-6 delinquency was in fact at quite a high level" (258). And in, the very same paragraph, Freeman has the hubris to criticize *Mead* for making what he claims was a "decidedly unscientific maneuver"!

Freeman himself presents little reliable quantitative data. There are some data about juvenile offenses drawn from his own observations and Western Samoan police: records (see pp. 259-68), but he never adequately describes his research design or methodology (especially his "samples") and thus his study is not replicable (and violates his own standards for scientific research).[37] After reading all of the prepublication hoopla in the press, I expected statistical or at least quantitative data from those High Court Records of American Samoa, to which he was supposedly denied access until 1981. It was said, for example, in the New York Times that

> although he published a number of papers about Samoa on technical subjects, Professor Freeman said, not until 1981 was he fi-

> nally granted access to the archives of the High Court of American Samoa. "I had tried in the 1960s but was refused, and when I was finally allowed in, the evidence was conclusive," he said, referring to the statistics about rape, assault and other crimes that appear in both the text and in the book's 55 pages of notes. (E. McDowell 1983: C21)

Those High Court records loomed large in why he waited so many years to publish his account, why he published almost five years after Mead's death (xvi). As I began to read the book my expectations were high that he would present documented statistical evidence of violence, rape, murder, and so on in American Samoa in the 1920s, evidence he waited years to acquire. It is not there. In fact, there are only four references in the entire book to data from these court records, all tangential and none quantitative.[38] It is certainly conceivable that he waited to get access to these records, thinking that they would be more useful than they turned out to be. But to suggest that they warranted the wait or provided "conclusive" evidence for anything is nonsense.[39]

In his analysis of rape (which he argues was common as opposed to Mead's stance that it was almost unknown), Freeman again fails to make significant definitional distinctions before comparing data and violates good scientific methods. He presents some very dubious statistics for rape in Western Samoa[40] and then. compares them with data from other countries (e.g., 248). He does not consider that what he defines as rape is not the same as the definition of rape used to generate, for example, the U.S. statistics. The Samoan concept of rape is simply not the same as the American legal definition and the data are not comparable.[41] Again, in disputing Mead's portrait he takes what might have been a relevant point to extremes, and even asserts that "both surreptitious and forcible rape have long been intrinsic to the sexual mores of Samoan men and are major elements in their sexual behavior" (249-50) and describes rape as a "recognized social practice" (352). It is interesting to note that Shore describes rape as thought to be: "evil" (1983:112) and classified along with murder as a serious offense by Samoans (115).

I do not intend to belabor Freeman's now-familiar habit of quoting others out of context, but this practice very much affects the adequacy of his portrayal of Mead's work and some examples are required to illustrate its seriousness. On page 74 he discusses the basic approach of Benedict and correctly points out the similarities between Mead and Benedict. He includes some phrases Mead used to describe Samoans, then continues: "these descriptions could well have been applied by Benedict to the Zuni,

and indeed Mead, on a later occasion, specifically noted that in both Zuni and Samoa it was 'the individual endowed with a capacity to feel strongly' who was 'maladjusted' " (Freeman 74). This quote comes not from a major source on Samoa but from *Sex and Temperament in Three Primitive Societies* (1963; originally published in 1935) and is very much out of context. Mead is discussing deviants and argues against the then-current psychiatric lumping of what she perceived to be two different kinds of deviants. She continues in what is surely an odd vein for an absolute cultural determinist.

> Modern psychiatric thought tends to attribute all of his [he who deviates from cultural norms] maladjustment to early conditioning and so places him in the invidious category of the psychically maimed. A study of primitive conditions does not bear out such a simple explanation. It does not account for the fact that it is always those individuals who show marked temperamental proclivities in opposition to the cultural emphases who are in each society the maladjusted person. . . . It does not explain why . . . it is the individual endowed with a capacity to feel strongly who is maladjusted in Zuni and Samoa. Such material suggests that there is another type of unadjusted person, whose failure to adjust should be referred not to his own weakness and defect, not to accident or to disease, but to a fundamental discrepancy between his *innate* disposition and his society's standards. (Mead 1963:291-92; emphasis added)

Freeman might be excused in this example because he was trying to make a different point (although how he could ignore an absolute cultural determinist attributing deviation to innate differences is difficult to understand). But examples exist as well which indicate that he could not have been other than conscious that he was changing the meaning to suit himself. I offer an example here without comment (it is so blatant that it needs little explanation) but will refer to it in later discussion. One of Freeman's main points is that in characterizing the Samoans as gentle, easy, unassertive, etc., Mead failed to note that in actuality Samoans are assertive, competitive, violent, and so forth. On page 88, he says:

> just as Samoan culture has eliminated strong emotion, so also it has eliminated any interest in competition. Samoan social organization, claims Mead, places "each individual, each household, each village, even (in Western Samoa) each district, in a hier-

> archy, wherein each is dignified only by its relationship to the whole," each performing tasks that "contribute to the honor and well-being of the whole," so that "competition is completely impossible."

Here is what Mead really says:

> these illustrations will show the *two tendencies* in Samoan social organizations, the tendency to place each individual, each household, each village, even (in Western Samoa) each district in a hierarchy, wherein each is dignified only by its relationship to the whole, each performs tasks which contribute to the honor and well-being of the whole, and competition is completely impossible. *The opposite tendency, the rebellion of individuals within the units against this subordination to a plan and their use of a place in a component unit to foment trouble and rivalry with other units, while not so strong, is always present.* (Mead 1976a:301-2; emphasis added)[42]

These examples are not isolated.[43] I provide just one more because it bears directly on Mead's depiction of Samoan adolescence. On pages 93-94, Freeman writes that

> adolescence in Samoa, according to Mead, is thus "peculiarly free of all those characteristics which make it a period dreaded by adults and perilous for young people in more complex--and often also, in more primitive societies." What is the most difficult age in American society becomes in Samoa the age of maximum ease, "perhaps the pleasantest time the Samoan girl will ever know."

The first sentence in this passage is essentially correct, although Freeman does not indicate to the reader that he deleted the first part of the quote and neglects to insert a dash between "primitive" and "societies." The second sentence comes from two sources, *Cooperation and Competition among Primitive Peoples* (Mead 1976a; first published in 1937) and another article by Mead. I have no quarrel with the quote in this second sentence, presumably from the article, but Freeman does not present Mead's more complex notion from *Cooperation and Competition* (and again note the way in which he appropriates her words).

> In understanding the role which age plays in the life of a Samoan child, it is necessary to bear in mind the large households in

> which . . . no child for long has a fixed status as the oldest, the only child, or the youngest. . . . The pressure of the children whose births soon after his own rapidly rob him of the position of youngest, push him slowly upward in the relative scale, until at adolescence a girl or boy is near the center of pressure, with as many younger people who can be ordered about and disciplined as there are older people who can order him about and discipline him. What is the most difficult age in our society becomes in Samoa, *because of this point of relativity,* the age of maximum ease partly because it is the age of most equal pressure. (Mead 1976a:308; emphasis added)

Freeman does not adequately differentiate between Mead's own data and her interpretations of that data; and because she did overgeneralize, especially in later work, there is a significant difference between the two. What made Mead a good ethnographer was her capacity to see and record what was happening around her (and although they sometimes missed the mark, her intuitive leaps were often insightful and sometimes brilliant). The important point is that Mead recorded and published data that did not fit precisely with her interpretation--but the data are there. even for Freeman to use in support of his own interpretation.[44] Although he could almost evade quoting Mead about the "two opposing tendencies," he frequently cites her work as evidence for his own position.[45] One need only read carefully either of her two books on Samoa to find evidence of conflict, hostility, competition, the centrality of ranking, etc.; after all, she did report and support two opposing tendencies even if her interpretation stressed one more than the other.

Freeman's main point in this book is that Mead got her Samoan ethnographic facts all wrong, that Samoan society and Samoans were not and are not as she depicted them. There is no question that Mead did overgeneralize and neglect some aspects although she did consistently mention contradictions and did not ignore or suppress all evidence contrary to her interpretation. In his fervent attack and frequent claims that Mead was all wrong, Freeman goes too far and neglects his own evidence that Samoans in fact seem to be both as Mead and Freeman describe them. I have already cited Mead's comment about the "two opposing tendencies" in Samoa. The point I wish to make here is that Freeman is forced to recognize that Mead was right, there are these two aspects even if he too believes one to be more important than the other. The following passages from Freeman all substantiate Mead. (1) "However, while this system of punctilious social intercourse operates effectively most of the time, it does

on occasion fail to prevent the tensions generated by the Samoan rank system from breaking out into violent conflict" (123); (2) "the elaborate conventions of the rank system are usually sufficient to contain its tensions" (137); and (3) because of this system of child rearing and the stringent demands that those in authority make upon the growing individual, Samoan character . . . has two marked sides to it, with an outer affability and respectfulness masking an inner susceptibility to choler and violence" (276).[46] By Freeman's own admission, then, Samoans are indeed both. He can quarrel with particular ethnographic facts or argue that Mead's emphasis was incorrect, but he cannot legitimately claim that she was all wrong. That both Mead and Freeman are both right and wrong, that Samoans are a wonderfully complex mixture of both, is one of the main points Shore makes in his recently published and superb book, *Sala'ilua: A Samoan Mystery* (1983). Shore offers a dispassionate and critical evaluation of Mead's Samoan research in the context of his ethnographic analysis, giving credit for her insights and criticizing her errors. What is most relevant here is his very good discussion of the duality in Samoan character (especially pp. 150-53). One comment is germane here:

> While some observers appear to have viewed Samoan personality with one eye shut, observing half truths, whether polite passivity without aggression or an aggressiveness lacking reserve and control, other observers have noted the strongly contradictory tendencies of Samoan personality. (152)

Readers who want to learn about Samoans should read Shore's book, not Freeman's.

The final section of Freeman's discussion of Mead is a consideration of why she made the errors he: thinks she did. Although I do not agree with him that Mead was as wrong as he maintains, it is a legitimate question.[47] Some errors, as he suggests., may have occurred because she was young, inexperienced, not fluent in Samoan, and lived with an expatriate family. No one can deny these facts.[48] However, they are hardly significant theoretical points today and not worthy of much attention. That is why, I suggest, Freeman was forced to construct his biology-culture framework, so that his attack on Mead would have a semblance of contemporary significance. In doing so, Freeman's fervor led him to miss the chance to make two very significant contributions, both of which he is unaware but which are inherent in the issues he discusses. Both also relate to the complexity of culture--in general as well as in Samoa.

The first point is the simple fact that Mead was a woman and Freeman a man. Recent anthropological literature abounds with discussion and illustration of the fact that male and female fieldworkers often focus on different kinds of data, or theories, or have access to different sources for data (see especially Ardener 1975a, 1975b). (Mead, of course, always believed this and only worked without a male counterpart once--in Samoa.) The fact that Mead went to study a specific problem and spent most of her time with adolescent girls was bound to have some impact on the data she gathered., just as Freeman's attendance at courts and predominantly male events had to influence his perspective. I am not surprised that her data on female adolescent sexuality differ from his, nor is it any wonder that her views of the ranking system are different (adolescent girls surely did not perceive it in the same way as adult men). Mead astutely considers this possibility. She notes that "village rank hardly affects the young children" (1961:48) but later states "nevertheless, rank not of birth but of title is very important in Samoa" (49). Mead also notes that the typical (or at least not highest ranking) Samoan female

> treats the lore of the village, the genealogies of the titles, the origin myths and local tales, the intricacies of the social organisation with supreme indifference. It is an exceptional girl who can give her great-grandfather's name, the exceptional boy who cannot give his genealogy in traditional form for several generations. (1961:71)

Freeman had a chance to recognize the significance of male-female differences (e.g., pp. 269, 288), but in the end it truly eludes him and he fails to realize what might be one aspect of the true complexity of Samoan culture. At least Mead admits that she views Samoa through the lens of only one of its segments (how many ethnographers admit to using an adult male lens, or accord any other lens equal validity?), and Freeman's suggestion, that her informants tricked her (289-90), does not adequately resolve the numerous contradictions. If Freeman had seriously considered the issue, or if he had even been aware that culture is not necessarily a single whole possessed uniformly by every individual, that different groups or segments in a society might see things in different ways, he could have made a significant contribution. But he did not.

The second point Freeman could have made but failed to perceive also has to do with the complexity of human sociocultural behavior. Shore (1983) brilliantly illustrates how Samoan life contains paradox, contradiction, and opposition, and he subtly and deftly resolves the dis-

crepancies between Mead's point of view and Freeman's. If Freeman had been less anxious to discredit Mead and more concerned with a legitimate understanding of her errors and the realities of Samoan life, he might have made a similar contribution. He attributed what he perceived to be her errors partly to the factors listed above but also to the theoretical paradigm in which she worked--supposed absolute cultural determinism and a denial of biological influences in human behavior. In truth, biology has nothing to do with it and Freeman himself presents a cultural analysis. The errors in Mead's interpretation have nothing to do with Boas or cultural determinism. They derive from a more far-reaching problem in the wider anthropological paradigm that characterized most of our work until recently: an inability to recognize that cultures and social organizations are not necessarily consistent, in equilibrium, and perfectly logically integrated in Western terms. Mead saw the contradictions, the "opposing tendencies," but anthropology had not at that time theoretically incorporated the notion of contradiction. Explaining deviation from the pattern was Mead's problem, in the same way that finding functions for witchcraft was a problem for British social anthropologists and seeking positive roles for conflict was a problem for social science (particularly sociology and anthropology) in general. If Freeman had been conversant with recent anthropological recognition of the nature of paradox and apparent contradiction in human sociocultural systems, he might have seen what the deeper issues here are and made a significant contribution to anthropological theory. But he did not, and the book remains little more than a curiosity.

NOTES

1. Introductory textbooks recently published in the United States are a good barometer of this trend. See, for example, Greenwood and Stini 1977; Johnston and Selby 1978.

2. As Murphy, Alland, and Skinner wrote in a letter published by the New York Times on February 6, 1983,

> whatever may be the Samoan facts, subsequent research in other parts of the world has substantiated her essential theoretical stance. . . . There are hundreds of societies in which girls are married shortly after menarche. Virginity, repression and teen-age problems are hardly important issues in female socialization in such groups, for they have liquidated adolescence, making this a moot question.

3. For additional examples of the basically cultural argument Freeman presents, see pp. 130, 208, 211, 216-17, 220, 225, 249, and 276.

4. The entire passage is relevant here:

> By intently observing their [Samoan chiefs] physiological states [during assemblies], and especially their redirection and displacement activities, I was able, as their anger mounted, to monitor the behavior of these chiefs in relation to their use of respect language. From repeated observations it became evident that as chiefs became angry they tended to become *more and more polite,* with ever-increasing use of deferential words and phrases. Thus, by resort to cultural convention they could usually avoid potentially damaging situations. Occasionally, however, the conventions of culture would fail completely, and incensed chiefs, having attained to pinnacles of elaborately patterned politeness, would suddenly lapse into violent aggression. . . . In such cases there was an extremely rapid regression from conventional to impulsive behavior. For our present purpose the significance of such incidents is that when the cultural conventions that ordinarily operate within chiefly assemblies fail, activity does *not* suddenly come to an end, but rather the conventional behavior is replaced, in an instant, by highly emotional and impulsive behavior that is animal-like in its ferocity. It is thus evident that if we are to understand the Samoan respect language, which is central to their culture, we must relate it to the disruptive emotions generated by the tensions of social dominance and rank, with which this special language has been developed to deal. In this case, as in other domains of their society, impulses and emotions underlie cultural convention to make up the dual inheritance that is to be found among the Samoans, as in all human populations. It is evident, therefore, that the cultural cannot be adequately comprehended except in relation to the much older phylogenetically given structures in relation to which it has been formed by nongenetic processes. Further, it is plain that the attempt to explain human behavior in purely cultural terms, is, by the anthropological nature of things, irremediably deficient. (300-301; emphasis in original)

5. In a footnote, Freeman does admit that

> inasmuch as it has, in accordance with Durkheimian precept, totally excluded biological variables, social anthropology in Great Britain and elsewhere, despite various differences in emphasis, has operated within the same basic paradigm as American cultural anthropology. (313)

Thus one must assume that Freeman's critique is as applicable to British social anthropology as it is to American cultural anthropology, if not more so. At least American anthropology in general included a biological subfield parallel to cultural anthropology.

6. Freeman is unequivocal in his accusations:

> it [the paradigm of absolute cultural determinism] was, indeed, essentially a *system of belief,* which, in claiming to represent something like revealed truth, required the suppression of whatever did not conform with its central dogma. And it was to such suppression . . . that the principal conclusion of Mead's Samoan researches was directed. (47; emphasis in original)

(Note the evasive use of the passive here, however.) Also, see pp. 282-83.

7. Krogman also notes that "in my student days at Chicago I was literally weaned on 'The Mind of Primitive Man' " (1976) "and describes Boas' research on human growth as "pioneer" (7). He added that Boas' "height-weight tables of Worcester children were for many years accepted as physical growth standards" (7).

8. Stocking (1974:14) also gives Boas credit for maintaining the four-field approach.

9. For another excellent discussion of Boas, one that places him firmly in historical context and that analyzes his work fairly, see Stocking 1968.

10. When Freeman cites this reference on page 28, he gives its publication date as 1911 (and place as Washington). However, Boas' professional publication of the results was not until 1912 in the *American Anthropologist* (n.s. 14, no. 3). Freeman must be referring here to (and quoting from) Boas' report to the government, which Stocking (1968:342) lists as Senate Document 208, 61st Congress, 2nd Session. The actual report was submitted to Congress in 1909 (Boas 1940 n. 60). Stocking (1968:176-80) discusses this report in detail and notes that Boas became more cautious about his conclusions and suggestions over time. Certainly, the line Freeman (28) quotes about "financial panics" is not to be found in the version of the article Boas chose to include in *Race, Language and Culture* (1940), which was based on the 1912 *Anthropologist* version. I have not been able to locate a copy of the 1911 report in the time available to write this review and therefore cannot comment on the context in which Boas made the statement about physical changes possibly being affected by "financial panics." Stocking discusses the report in some detail but does not mention the statement.

11. What is missing from the Stocking quote here is not insignificant. It should read:

> the whole thrust of his [Boas'] thought was in fact to distinguish the concepts of race and culture, to separate biological and cultural heredity, to focus attention on cultural process, to free the concept of culture from its heritage of evolutionary and racial assumption, so that it could subsequently become the *cornerstone of social scientific disciplines* completely independent of biological determinism. (Stocking 1968:264; emphasis added)

It is also interesting that Stocking immediately continues by saying that "this is not to suggest that Boas was solely responsible for this process, or even that he was fully conscious of it" (264-65).

12. Here is another example in which Freeman goes from evolution (whether biological or social is not clear) to a demeaning of biological factors in general:

> it is evident that much of the disdain that Virchow had for evolutionary thought was communicated to Boas, for, as Boas' student Paul Radin has noted, Boas "always took a prevailingly antagonistic position" to the theory of evolution. This antagonism was undoubtedly Boas' great shortcoming as an anthropologist, for while it spurred him to oppose the unwarranted application of biological principles to cultural phenomena, it also caused him to underestimate the importance of biology in human life, and to impede the emergence of a scientifically adequate anthropological paradigm based on recognition of the pervasive interaction of biological and cultural processes. (26)

13. "Even earlier, she [Benedict] had made the point that it is those individuals whose innate characteristics are too far removed from the norm of their culture who find their culture deeply uncongenial" (Mead 1972:195). Also see later discussion.

14. Freeman sometimes uses the tactic of implying guilt by association. On pages 98-99 he writes,

> in 1924, when the nature-nurture controversy was at its height, J. B. Watson had baldly asserted that there was "no such thing as an inheritance of capacity, talent, temperament, mental constitution and characteristics," and in subsequent years he had repeatedly spoken of human nature as having "limitless plasticity." However, as the hereditarians were quick to point out, Watson's sweeping assertions were unsupported by any . . . evidence, and in this highly insecure situation Mead's depiction of Samoa became of fundamental significance, not only for the proponents of cultural determinism but equally for the wider environmentalist movement. . . .

Although it is left unsaid here (not elsewhere), the implication is that Boas and his students, because their work was cited by extremists such as Watson, were somehow identical in approach to Watson. But the passages I have cited (and will cite) indicate that Boas, Benedict, Mead, and Kroeber would not agree.

15. Although it is not as theoretically significant, Freeman is also inaccurate in quoting Boas on page 24 (and takes some of Boas' words for his own). Here is Freeman:

> after noting that among these Eskimo, as among the rest of mankind, the fear of traditions and old customs was deeply implanted, he [Boas] added the revealing comment that it was "a difficult struggle for every individual and every people to give up tradition and follow the path of truth."

Here is Boas:

> The fear of tradition and old customs is deeply implanted in mankind, and in the same way as it regulates life here, it halts all progress for us. I believe it is a difficult struggle for every individual and every people to give up tradition and follow the path to truth. (Boas in Stocking 1968:148)

(Note the change Freeman made from "to truth" to "of truth.") Another example is relevant here. On page 28 Freeman makes much of Boas' purported Lamarckianism and cites Stocking. What he does not tell a reader is that although Stocking (1968:184) does say that "there is much in Boas' work to tie him to the tradition of neo-Lamarckian direct environmentalism . . . ," he also says that Boas was neither a "committed Lamarckian" nor Darwinian. Freeman never mentions that Stocking (186) notes that Boas changed his mind about the inheritance of acquired characteristics.

16. Freeman recognizes that Kroeber was more cultural than Boas yet asserts that the difference between them was only of a "slight degree" (46). He fails to note that Kroeber too was reacting to the excesses of the social evolutionists and eugenicists, and that Kroeber did not completely exclude biological factors. In the 1948 edition of his textbook, Kroeber wrote that

> the drift of this discussion may seem to be an unavowed argument in favor of race equality. It is not that. As a matter of fact, the anatomical differences between races would appear to render it likely that at least some corresponding congenital differences of psychological quality exist. These differences might not be profound, compared with the sum total of common human faculties, much as the physical variations of mankind fall within the limits of a single species. Yet they would preclude identity. (Kroeber 1948:204)

And here is how Kroeber differentiates anthropology from other disciplines:

> could it be that the specific subject of anthropology is the interrelation of what is biological in man and what is social and historical to him? The answer is Yes. Or, more broadly, anthropology does at least concern itself with both organic and social factors in man, whereas nearly all other sciences and studies deal with one or the other. Anthropology concerns itself with both factors because these come associated in human beings in nature. (1948)

Kroeber undoubtedly stressed the significance of cultural factors, especially in his own work; no one would deny that. But it is not possible to assert with impunity that Kroeber was completely and unalterably opposed to the inclusion of biological factors in understanding human beings. Kroeber's "intellectual manifesto" of 1917 is nowhere near as simple as Freeman indicates. In this article, Kroeber makes the following statements:

> Everyone is aware that we are born with certain powers and that we acquire others. There is no need of argument to prove that we derive some things in our lives and make-up from nature through heredity, and that other things come to us through agencies with which heredity has nothing to do. (165)

> That heredity operates in the domain of mind as well as that of the body, is one thing; that therefore heredity is the mainspring of civilization, is an entirely different proposition, without any necessary connection, and certainly withont any established connection, with the former conclusion. (192)

Freeman's portrayal of Lowie is equally one-sided. Lowie diverged significantly from Benedict and Mead, and although he did partly follow Kroeber's idea that culture was "a thing *sui generis* " (Lowie in Freeman:45), again Freeman fails to accord any real significance to the fact that Lowie was arguing against eugenics and racism (as well as geographical determinism). See, especially, Eggan's Introduction (1966) to Lowie's *Culture and Ethnology* (originally published in 1917) as well as Lowie (1966) himself.

17. For example, pp. 67-68 n. 6, 74 n. 24, 118-19 n. 11.

18. For example, see pp. 59-60, 104, 119, 249-50.

19. For example, see pp. 24-25 n. 12, 26-27 n. 15, 84-85 n. 4. This practice of mentioning a specific work in the text is especially devious because it gives the reader the impression that information and even specific quotes come from one source when in reality they come from another. For example, on page 74 Freeman writes the following sentence:

> It thus transpired that the first written application of Benedict's new theory appeared in Mead's account, in *Social Organization of Manu'a,* of the "dominant cultural attitudes" of the Samoans, "every detail of the phrasing" of which was "thrashed out" by Benedict and Mead, as they "discussed at length the kind of personality that had been institutionalized in Samoan culture."

None of these quotations come from *Social Organization of Manu'a.*

20. See the paragraphs on these pages for examples: 30-31, 31, 38-39, 67, 80-81, 85-86, 164-65.

21. The examples of this practice are so numerous that it is fair to characterize it as typical and to say that when Freeman does so indicate, it is exceptional. The practice facilititates taking quotes out of context without arousing the reader's suspicion. Some examples have already been given and more will be provided in the discussion of Mead's work and in further notes.

22. Several examples of this practice have been mentioned, but additional examples are required. If one compares passages from Freeman with his original sources, it is easy to see that although a citation is usually present, he uses words and phrases that are not his. Sometimes he changes a tense or rearranges word or phrase order, but this is hardly good scholarship.

Example # 1

> On the morning of 31 August 1925, "remembering Stevenson's rhapsodies," Mead was up early for her Matson liner's arrival in the romantically remote islands of Samoa. The "whole picture," alack, was badly skewed by the presence of numerous battleships of the American Pacific fleet, with airplanes screaming overhead, and a naval band playing ragtime. (Freeman 61)

(Note here that Mead never said "remembering Stevenson's rhapsodies"--at least not in the source Freeman cites. The paragraph mentions W. Somerset Maugham, and cites him, so perhaps it belongs to him; see Freeman 316.)

> The presence of the fleet today skews the whole picture badly. There are numerous battleships in the harbor and on all sides of the island, mostly not in the harbor because they make the water oily and spoil the governor's bathing. Airplanes scream overhead; the band of some ship is constantly playing ragtime. (Mead 1977:23)

Example #2

> Again, although the Christian church required chastity for church membership, in actual practice, according to Mead, no one became a church member until after marriage. . . . (Freeman 91)

> Indeed, the sterner tenets of protestant Christianity had been so "remoulded," according to Mead, that there was "passive acceptance" by religious authorities of the premarital promiscuity which was, so she claimed, customary among female adolescents, with the result that, as she asserted in 1929 "no one" became a church member "until after marriage." (Freeman 185)

> No one becomes a church member until after marriage, and young widows and widowers whose bereaved state is protracted usually fall from membership. (Mead 1929:269)

> This passive acceptance by the religious authorities themselves of pre-marital irregularities went a long way towards minimising the girls' sense of guilt., (Mead 1961:123)

Example #3

> Occasionally, adults will "vent their full irritation upon the heads of troublesome children" by soundly lashing them with palm leaves or dispersing them with a shower of small stones. . . . (Freeman 88)

> If a crowd of children were near enough, pressing in curiously to watch some spectacle at which they are not wanted, they are soundly lashed with palm leaves, or dispersed with a shower of small stones. . . . (Mead 1961:33)

Example #4

> "Love, hate, jealousy and revenge, sorrow and bereavement," we are told, are all matters of weeks. . . . (Freeman 212)
>
> Love and hate, jealousy and revenge, sorrow and bereavement, are all matters of weeks. (Mead 1961:146)

23. See, for example, pp. 87 n. 7; 87-88 n. 10; 88-92 (n. 18, 19, 20, and 21); 131 n. 1; and 188 n. 25.

24. The full text of Freeman's note, which appears on page 321, is as follows:

> 3. M. Mead, *Male and Female* (Harmondsworth, 1962), 100, 201; idem, *Coming of Age* 122, 170; idem, "The Role of the Individual in Samoan Culture," *Journal of the Royal Anthropological Institute* 58 (1928): 418; idem, "The Samoans," in M. Mead, ed., *Cooperation and Competition among Primitive Peoples* (New York, 1937), 308; idem, "1925-1939," in *From the South Seas* (New York, 1939), xxvi.

25. Except when a whole paragraph comes from one source, or in the few cases of clarity, almost any paragraph could illustrate the ambiguity inherent in this style.

26. There are other instances of this. See, for example, pp. 229 n. 5 and 80 n. 36.

27. Examples of the way in which Freeman takes quotes out of context are more than numerous. In order to substantiate that it is a frequent occurrence and not restricted to the examples given in my text, I will provide a few further instances.

Example # 1

> As we have seen, Mead's Samoan researches gave apparently decisive support to the movement that (in George Stocking's words) sought "an explanation of human behavior in purely cultural terms," and so sustained the antibiological orientation of the Boasian paradigm. (Freeman 294-95)
>
> At numerous points, I have emphasized that internal (although to a large extent parallel) developments within *each* of the social sciences conditioned the movement toward an explanation of human behavior in purely cultural terms, and that these developments in turn must be viewed in the context of changes in scientific disciplines *outside* the social sciences. (Stocking 1968:303; emphasis in original)

Example # 2

> Not only is competition muted and covert within village communities, but also, Mead claims, "competitiveness between villages usually does not reach important heights of intervillage aggression *[sic]*." (Freeman 89)
>
> Pride, in Samoa, is permitted an outlet through the high valuation which is put upon the village, its honor, its prestige. Because of the intricate kin ties between villages, and the freedom of choice which makes membership in each cooperative group ultimately voluntary, competitiveness between villages usually does not reach important heights of intervillage aggressiveness. But intervillage warfare was a possibility which increased with the number of people who lived within easy reach of each other. (Mead 1976b:474)

Example #3

> These were the attitudes that the young Margaret Mead came to adopt, and which led her, when embarking on her inquiries in Manu'a, to assume that human nature, being "the rawest, most undifferentiated . . . raw material," could be shaped by culture into any form. (Freeman 295)

> At the beginning of the decade, using language quite as extreme as that of Watson [see above], she advanced the view, on the basis of her researches in Samoa and New Guinea, that human nature was "the rawest, most undifferentiated of raw material". (Freeman 101)

> There are theorists to-day [1930] who, proceeding upon the assumption that all children are naturally good, kind, intelligent, unselfish and discriminating, deprecate any discipline or direction from adults. Still others base their disapproval of disciplinary measures upon the plea that all discipline inhibits the child. . . . All of these educators base their theories on the belief that there is something called Human Nature which would blossom in beauty were it not distorted by the limited points of view of the adults. It is, however, a more tenable attitude to regard human nature as the rawest, most undifferentiated of raw material, which must be moulded into shape by its society, which will have no form worthy of recognition unless it is shaped and formed by cultural tradition. (Mead 1975:211-12)

Example #4

> In 1961 she wrote of "the absoluteness of monographs of primitive societies," which "like well-painted portraits of the famous dead . . . would stand forever for the edification and enjoyment of future generations, forever true because no truer picture could be made of that which is gone." *Coming of Age,* she indicated, was just such a monograph, and she dwelt on "the historical caprice which had selected a handful of young girls on a tiny island to stand forever like the lovers on Keats' Grecian urn." (Freeman 106)

> This was the period [pre-World War II] when we emphasized the absoluteness of monographs on primitive societies, valuable precisely because they were the records of an order which would soon vanish never to return. Like well-painted portraits of the famous dead, these monographs would stand forever for the edification and enjoyment of future generations, forever true because no truer picture could be made of that which was gone, We were conscious of the historical caprice which had selected a handful of young girls on a tiny island to stand forever like the lovers on Keats' Grecian urn. There was, in spite of the dynamic content of our subject matter, a certain static quality about our approach. (Mead 1961: 13-14)

28. Freeman's position with regard to Freud is an interesting one. He actually appears to be somewhat Freudian himself. He asserts in a note (320) that "my own researches in Samoa, during the years 1966-1967, revealed the Oedipus situation to be decidedly present." His discussion on pp. 208-11 also reveals a Freudian bent. He seems to take the position that orthodox psychoanalytic theory is basically grounded in biology and innate impulses, and thus anyone who debates traditional Freud is taking an antibiological stance.

29. Freeman is very careless about noting changes in Mead's presentation of her Samoan materials, although he does take the opportunity to note exaggerations (e.g., 102). He quotes

freely from all of her works without concern for when they were published or who the intended audience was. If he were seriously intent on establishing the fact that Mead's work on Samoa had a definitive impact on the nature-nurture debate of the 1920s and early 1930s, he should have predominately restricted himself to a consideration of her early work only, especially *Coming of Age in Samoa* (first published in 1928) and *Social Organization of Manu'a* (first published in 1930). But he does not. For example, the paragraph (200-201) that introduces chapter 14, "Childrearing" contains eleven quotes from Mead, none of which is from these two central sources. This is not unusual. In the critical chapter 6, "Mead's Depiction of the Samoans," there are twenty-seven references to *Coming of Age,* only eight to *Social Organization of Manu'a,* and more than seventy-five references to other works by Mead--about two-thirds of which were published in 1935 or later.

30. See Shore (1983) for a more insightful evaluation of Mead's work in Samoa.

31. Freeman did visit American Samoa in 1967 (xv), but does not state the duration of the visit.

32. The full quote from the Samoan chief is as follows:

> All the Samoan people are of one race. Our customs, genealogies, legends and languages are the same. The chiefs and village maids *(taupou)* of American Samoa when they visit British Samoa are recognized as chiefs and *taupous* of certain villages in accordance with their genealogies. Their visitors from British Samoa are likewise recognized in the chief councils of Tutuila and Manu'a. (Freeman 117)

33. There seems to be a constant refrain coming from all Samoan informants: things used to be stricter than they are today. Mead's informants said it, Freeman's informants said it, Gerber's informants (Freeman 108) said it, but nowhere does Freeman consider that this consistency might be a clue to something; he just accepts it at face value as truth unaffected by contemporary Samoan morality and worldview. Gerber (Freeman 108) saw that indeed people see their past from the perspective of the present, but Freeman, despite describing her work as "excellent ethnography" (326), just ignores her.

> She [Gerber] construed the unequivocal statements of her Samoan informants as a "rewriting of history," so accepting Mead's fanciful account of Samoan sexual behavior in preference to the unanimous and direct testimony of the Samoans. . . . Could any myth, one wonders, have acquired, within the confines of a scientific discipline and during the second half of the twentieth century, a greater potency? (108)

34. Freeman never addresses the issue of the nature and definition of "cult," and his usage of the term is clearly at odds with accepted anthropological practice.

35. One must also wonder, given this reticence, about Freeman's explanation of Mead's "error." He claims her informants tricked her and just told her stories (e.g., 290), but why invent sexy stories if they were so reticent about sexual matters?

36. Mead (1961:190) says that "as there were only sixty-eight girls between the ages of nine and twenty, quantitative statements are practically valueless for obvious reasons: the probable error of the group is too large; the age classes are too small, etc."

37. Freeman holds scientific standards such as replicability apparently in high regard. He makes this especially clear on page 291 where he chastises Boas for not subjecting Mead's

finding to a thorough comparison with other sources. While it is more than legitimate to criticize Mead if she inadequately prepared herself with background and comparative reading, it is not appropriate to criticize her for not writing a book on Samoan history. Nor is it appropriate to begin with the assumption that if Mead differs from another source, it is Mead who is necessarily in error. Freeman goes to ridiculous lengths here, claiming that she returned from the field "with tales running directly counter to all other ethnographic accounts of Samoa. . ." (291). As Freeman himself notes many times, other ethnographers (including Freeman himself) have at least on occasion agreed with her. In evaluating ethnography, Freeman's criteria are not necessarily scientific or consistent, and he is incapable of other than black-or-white reasoning on this issue. For example, Lowell Holmes is treated shabbily by Freeman despite his excellent ethnography which disagreed with Meads depiction of Samoa; Holmes' error was to say that "the reliability of Mead's account of Samoa was 'remarkably high' " (Freeman 105; see also 103-5). Freeman chides Holmes for having what Freeman thought was irrefutable evidence against Mead but not arguing that "the central conclusion she had reached in *Coming of Age in Samoa* about the sovereignty of nurture over nature was false" (105). Freeman seems unable to understand that Holmes could present solid evidence that contradicts Mead's data and yet still believe her account was reliable and not argue against nurture over nature.

38. In the first reference to these High Court data, he states "that in the prudish Christian society of Samoa in the 1920s, sexual intercourse between unmarried persons was held to be both a sin and a crime is confirmed by cases in the archives of the high court of American Samoa" (238). Despite his use of the word "cases" here, he gives only a single example. The second reference does not even appear in the body of the text but in a footnote in which he gives a single example of fines being imposed for adultery (351). The third reference is again to a single example, this time of surreptitious rape (246). The final reference to these court records offers promise but gives us no numbers.

> The court records of American Samoa, which begin in 1900, note numerous cases of rape having been committed by Samoans during the first three decades of this century, and the jail statistics included in the exhibits attched to the hearings of the congressional commission on American Samoa of 1930 show that at the end of the 1920s rape was the third most common offense after assault and larceny. . . . (249)

Note that the only reference to the High Court records here is that there were "numerous cases"--the other material is from another source.

39. Freeman (xvi) finished an early draft of this manuscript in August of 1978, obviously without these High Court data. He clearly thought that this early manuscript was substantial because he offered to send a copy to Mead. He claims that he received no reply to this offer, but of course neglects to mention that although not hospitalized, Mead was dying at the time. The innuendo was picked up and made much of by the popular press (see, for example, Rensberger 1983:37).

40. Here in detail is what Freeman does.

> In 1966, when the total population of Western Samoa was about 131,000, the number of forcible and attempted rapes reported to the police . . . was thirty-eight, which is equal to a rate of about sixty rapes per 100,000 females per an-

> num, a rate twice as high as that of the United States and twenty times as high as that of England. Further, if cases of surreptitious rape, or indecent assault, reported to the police be included, then the Western Samoan rate becomes approximately 160 rapes per 100,000 females per annum. These figures, while only very approximate (for in Western Samoa a very considerable proportion of forcible and surreptitious rapes are, in fact, not reported to the police), do indicate that rape is unusually common in Samoa; the Samoan rape rate is certainly one of the highest to be found anywhere in the world. (248-49)

Note the curious generation of these statistics. Also note that in order to get the rate up to 160, he includes "surreptitious rape, or indecent assault"--clearly he has no idea what would happen to the U.S. rates if indecent assault were included. His parenthetical comment about the underreporting of rape in Samoa also of course applies to the United States where it is estimated that only one in five to only one in twenty *actual* rapes is ever reported (Brownmiller 1975:175).

41. Here Freeman describes surreptitious rape:

> Surreptitious rape, or *moetotolo* (literally "sleep crawling") is a peculiarly Samoan custom in which a man, having crept into a house under cover of darkness, sexually assults a sleeping woman. (244)
>
> The intention of the sleep crawler is, in fact, to creep into a house in which a female virgin is sleeping, and before she has awoken to rape her manually by inserting one or two of his fingers in her vagina, an action patterned on the ceremonial defloration of a taupou. (245)

Freeman never addressses the significant problem here: how does this occur without waking up the girl or one of the many other people sleeping in the partitionless house? Mead (Freeman 245) thought that the girl was expecting him or another lover and therefore did not raise a hue and cry, but Freeman dismisses this possibility entirely by saying that "as anyone who had studied the phenomenology of rape will know, successful personation by a rapist is an extremely rare event . . ." (245). He cites no source here on the "phenomenology of rape.) Freeman describes forcible rape this way:

> . . . 60 percent of the victims were virgins. In the typical case . . . a girl of from 15 to 19 is alone and away from the settled parts of her village when accosted by a male of from 19 to 23 years of age. Often he is known to the girl, and he believes her to be a virgin. When she tries to escape, her assailant commonly resorts to the culturally standardized strategem of knocking her unconscious with a heavy punch to her solar plexus. After inserting one or two fingers into his victim's vagina, the rapist usually also attempts penile intromission. . . . (248)

As anyone familiar with the literature on rape ought to know, these descriptions do not fit with the American legal definition of rape (see Brownmiller 1975). One of the most significant differences, of course, is that in raping, Samoan men are trying to acquire wives. "Many Samoans aver that the principal aim of a male who engages in either surreptitious or forcible rape is to obtain for himself a virgin wife" (Freeman 247).

42. Freeman does, in a later context (142), reluctantly acknowledge that Mead presents both sides.

43. One example is of some historical interest. On page 79, Freeman tells us that "in Sydney, Australia, in October 1928 she [Mead] proudly told A. R. Radcliffe-Brown that it was Boas who had planned her work in Samoa. . . ." What Freeman doesn't relate is how the subject arose.

> [Radcliffe-]Brown is awfully funny. He approves of my work and today, after he had been reading the monograph [Social Organization of Manu'a], he began tea by saying he didn't see how I had gotten away with doing work which was so awfully contrary to the spirit of the American school, the kind of work which Boas didn't believe was possible. I retaliated by saying that Boas had planned my work. (Mead 1959:309)

44. See, for example, Mead 1961:32, 33, 34, 39, 40, 49, and Mead 1976b:310 for just a sampling.

45. For example, see Freeman pp. 84 n. 2, 88, 89, 93, 100, 172, 186, 191, 212-13, 222, 238, 244-45, 255ff., 268.

46. See also pp. 142, 147, 166, 216ff., 273, 278. Freeman also cites authors who support this as well; for examples, see pp. 163, 234, 376.

47. Mead recognized that perspectives on old data can and do change. In the 1962 introduction to *Male and Female: A Study of the Sexes in a Changing World,* she wrote:

> also, there have been many developments in anthropological theory since this book was completed. Fifteen years have elapsed within which the vivid interaction between cultural theory and observations and experiments on other living creatures, primates, ungulates, and birds, have given us new insights into biologically given behavior and possible types of more specifically instinctive behavior in man. (Mead 1967:xiii)

48. Mead herself recognized her own inexperience and youth (e.g., 1961:15) and argued strongly for more pre-fieldwork training and preparation (e.g., 1972:141-44).

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