Response: Derek Freeman
Australian National University

I shall comment first on the remarks of Fay Ala'ilima and Felix Wendt, both of whom know Samoa, and then on those of Nancy McDowell, who does not.

On the remarks of Fay Ala'ilima. Rather than dealing with the scientific significance of my book, Fay Ala'ilima has chosen to dwell on other matters. I shall, then, do no more than comment very briefly on her essentially personal remarks.

By admitting that the facts I have marshalled in my book refer to realities, Ala'ilima is, logically, also admitting that Mead's extreme conclusion of 1928--that biological variables are of no significance in the etiology of adolescent behavior--is in error.[1] This, for anthropology, is a crucially important recognition, and is, in fact, the principal objective of my book.

However, a refutation, as Ala'ilima fails to understand, must perforce concern itself with the systematic testing of those propositions that its author supposes to be in error, for only in this way can error be exposed and eliminated from the formulations of a scientific discipline. It is therefore pointlessly digressive for Ala'ilima to inquire why, instead of constructing a refutation of Mead's errors, I did not write an account of my personal experiences in the early 1940's as a member of the household of Lauvī

Vainu'u, who was at that time the leading talking chief of Sa'anapu village on the then remote south coast of the island of Upolu. My reply is that this is something I may eventually do. At this juncture I shall merely note that Lauvī Vainu'u came to mean more to me, in some ways, than my own father, and that I have both warm affection and deep respect for the people of Sa'anapu, some of whom I have now known for more than forty years. My association with Sa'anapu is something that means much to me, and I only hope that I shall be able to continue to make whatever contributions I can to the welfare of its people.

The people of Sa'anapu, like other Samoans (as Ala'ilima must know), are devout Christians, and as such they value the truth. All that I have done in my book, in the interests both of Samoan studies and of anthropology, is to speak a modicum of the unvarnished truth. I have not yet heard of anybody being banished from a Samoan village, an Australian town, or, for that matter, a scientific society, for disinterestedly speaking the truth. Indeed, in my view, the speaking of the truth, when it bears on issues of great intellectual and scientific importance as in my book, is a prime responsibility of any scientist or man of good will, and he should be prepared to honor this responsibility at whatever the personal cost.

Ala'ilima's suggestion that my book was written for self-aggrandizement is sheer aspersion, wholly untrue, and quite unworthy of her. I also reject her equally darksome suggestion that my book will act to the disadvantage of individual Samoans. Because some Samoans engage in, say, aggressive behavior, this in no way means that all Samoans engage in such behavior, and there is certainly no warrant in my book for the formation of stereotypes, as Ala'ilima suggests. As for the Samoans of Honolulu, Auckland, and Carson City, people will continue, as in the past, to take them as they find them. This, moreover, is something that these and other Samoans well understand, which is why the great majority of them are so well mannered and so well behaved.

I do not claim, nor have I ever claimed, that Samoans are "tremendously grateful" for my documentation of the "darker side" that exists in Samoan behavior just as it does in the behavior of all peoples. What I do know is that a number of Samoans of my acquaintance fully appreciate the importance, for the future of Samoan studies, of the refutation of the errors in Mead's account of Samoa, even if this involves, as it necessarily does, the facing of the realities of Samoan existence. The Samoans themselves are, of course, no strangers to these realities. They are brought to the attention of the *matai* of all Samoan villages in the courts, or *fono manu,* in which they all sit: from time to time. Further, they are realities dealt with by these chiefs, and the great majority of Samoans, with both firmness and justice.

On the remarks of Felix Wendt. Although he freely acknowledges that "many of the things Margaret Mead said about Samoans were incorrect," which means that her general conclusion based on these incorrect statements is in error, Felix Wendt objects to the evidence contained in my book. This is despite the fact that it is largely verified evidence from official sources, and solely on the ground that it may create a bad "image."

This is an attitude that, given Wendt's avowal of both Christianity and science, I cannot, in all reason, understand. God, on whom the Samoans aver their country is founded, is (Deuteronomy 32:4) above all concerned with the truth. In science too the truth is all-important and is approached, as Sir Karl Popper has shown, by the elimination of error. And so, if as Wendt acknowledges, Mead's *Coming of Age in Samoa* contains numerous incorrect statements, it clearly becomes one's scientific duty, as a serious student of Samoa, to refute those, errors.

Again, it is a cardinal mistake to suppose that because unlawful behavior by Samoans has been recorded at certain rates, that this, in any rational sense, creates an "image" of the Samoan people at large. As Wendt rightly notes, the "darker side" of the Samoans is "no darker than that of any other people." And further, as I emphasize in my book, the Samoans most definitely have their "shining virtues," being, as I note,, devoted to the ethics of Christianity and the ideal of mutual love, or *fealofani.*

This does not mean, however, that unlawful behavior is absent from Samoa, and the facing of this fact without anger or fear is to be desired on both scientific and humane grounds. This, I would note, is something that Professor Albert Wendt has acknowledged in writing of my book: "Derek Freeman's insights into us and our way of life reveal that he has a deep love of Samoa. He sees us *honestly;* he does not try to hide the disturbing side. His work is a major contribution to understanding who and what we Samoans are; in fact, to understanding what people are like everywhere."

I would add that knowing them as I do, in all their human complexity, I indeed do have love and admiration for the people of Samoa, and that it is my belief that if only we Westerners can understand the Samoans in all their human complexity, then we shall also be able to understand ourselves.

Felix Wendt's aspersions that I have, in my researches in Samoa, betrayed secrets and engaged in "purposeful misinterpretation," are quite capricious and wholly untrue. I have throughout striven to behave as a responsible scientist should, and my regard for the people of Sa'anapu is of a kind that will not fade.

Wendt's further aspersion that, after having known them for over forty years, I have "grown old and disillusioned with the changing faces of Samoa," is also entirely unjustified and totally untrue. Indeed, I am now more hopeful about the future of Western Samoa than I have ever been. In particular, I have been impressed by the progress that is being made within the University of Samoa in the field of Samoan studies.

As a number of eminent Samoans, whose views I deeply respect, have remarked to me, my refutation of Mead's depiction of Samoa has been an essential step in the establishment of a serious discipline of Samoan studies. It was, therefore, a source of great satisfaction to me, during my visit to Western Samoa in August 1983, to be able to set up within the University of Samoa, with an initial donation of W.S. $3,000 from the royalties of the German language edition of my book, a special research fund to enable Samoan scholars to do research on the history and culture of Samoa.

As I remarked in my address at the first graduation ceremony of the University of Samoa, in the capacity of Academic Pro-Chancellor, I very much hope that as the field of Samoan studies develops, it will be possible to communicate to the outside world some of the humanly valuable aspects of the *fa'aSamoa.* Some examples are, the dignified *amio fa'aaloalo* of Samoans, the custom of *tapua'i,* the enlightened way in which Samoans deal with convicted criminals, and, perhaps most importantly of all, their expert techniques of achieving, when necessary, *fa'aleleiga,* or reconciliation, between warring social factions.

It is thus very much my view that although Samoa is a small country, it has great significance for the science of anthropology, and that Samoan studies as they develop will contribute greatly to human studies in general. It is to this process that my book is an essential contribution, as I hope all thoughtful Samoans and *papalagi* will come to realize.

On Nancy McDowell and Margaret Mead. Before commenting on Nancy McDowell's defense of Margaret Mead's Samoan researches let me remind my readers of Mead's extreme conclusion of 1928. In Mead's own description (1977: 19) she went to Samoa in 1925 "to carry out the task" that had been "given" to her by her professor, Franz Boas, "to investigate to what extent the storm and stress of adolescence" is "biologically determined and to what extent it is modified by the culture within which adolescents are reared." In 1928, in the fourth paragraph of the thirteenth chapter of *Coming of Age in Samoa* (1961, orig. 1928:197), she came to the scientifically preposterous conclusion that biological variables are of no significance whatsoever in the etiology of adolescent behavior. I say scientifically preposterous because in the light of modern knowledge it is evident that all human behavior is characterized by the interaction of cultural and biological variables. Thus, as Konner (1982:80) has recently expressed it, "any analysis of the causes of human nature that tends to ignore *either* the genes *or* environmental factors may safely be discarded."

On this basis alone any knowledgeable behavioral scientist, then as now, would reject Mead's extreme conclusion of 1928. Yet, as I document in my book, the preposterous conclusion of Mead's *Coming of Age in Samoa* rapidly became pivotal to the doctrine of cultural determinism and, having been recorded in countless textbooks and repeated in university lecture rooms throughout the world, has long been integral to the belief systems of many cultural anthropologists and especially of devoted admirers and associates of Margaret Mead. Nancy McDowell, it is important to realize, is one of these.

In 1980, for example, she published in the *American Anthropologist* a paper entitled "The Oceanic Ethnography of Margaret Mead," which contained, among other things, her evaluations of Mead's *Coming of Age in Samoa* and *Social Organization of Manu'a.* By that time there had been serious questioning of Mead's extreme conclusion of 1928, ranging from Raum's observation (1967, orig. 1940:293) that Mead's assertions were "often contradicted by her own evidence" to Barnouw's critique in the third edition of his *Culture and Personality* (1979:89-94), in the course of which he pointedly cites Jane van Lawick-Goodall's observation (1971:160), "Adolescence is a difficult time for some chimpanzees just as it is for some humans." Again, Mead herself, in the "reflections" she included in the 1969 edition of *Social Organization of Manu'a,* had admitted (1969:227) the "serious problem" of "reconciling" the "contradictions" between her account of Manu'a and "other records of historical and contemporary behavior." And finally, in 1972, in the *Journal of the Polynesian Society,* I had published a detailed study of Mead's far from proficient use of the Samoan language (1972:74ff.) listing over 180 errors (some of them egregious) that occur in the Samoan sections of the text of *Social Organization of Manu'a.*

Yet, in her laudatory appraisal of 1980, McDowell, ignoring entirely all of this substantive criticism, dwelt on Mead's "concern for the precision and accuracy of her data," claiming that the fact that *Social Organization of Manu'a* might have been written in 1980, was "a telling statement" about the "standards and brilliance"' of Mead's work, with Mead's "fieldwork and published reports" still standing as "models for any beginning fieldworker to follow" (1980:278).

These statements by McDowell, given the numerous errors that had by then been shown to exist in Mead's *Social Organization of Manu'a,* can only be classed as examples of uncritical adulation.

In the same paper of 1980, McDowell went on to tell her professional colleagues, without critical comment or qualification of any kind, that in *Coming of Age in Samoa,* Mead had shown that the "storm and stress" adolescence was "a cultural creation." Anyone who could repeat this extreme proposition as late as 1980 is very obviously a cultural determinist, and it is very much to professional believers in Mead's preposterous conclusion of 1928 (like Nancy McDowell) that my book is addressed.

It is very evident however that the ungainsayable evidence I have adduced to demonstrate that, at least with reference to Samoa, Mead's conclusion of 1928 cannot be sustained has greatly agitated Dr. McDowell. In their classic study *When Prophecy Fails,* Festinger, Riecken, and Schacter have remarked on "the variety of ingenious defences with which people protect their convictions" (1964, orig. 1956:3). As an ardent admirer of Margaret Mead and a leading proponent of her views, McDowell is in the position of one for whom prophecy has failed. As an apologist for the scientifically preposterous conclusion that the young Margaret Mead reached in her enormously influential *Coming of Age in Samoa,* McDowell has, at inordinate length and with the fervor of a fundamentalist, mustered every conceivable argument in an attempt to save something from the wreckage of one of her fondest beliefs.

Because McDowell knows nothing in particular about Samoa and, as is obvious from her remarks, lacks detailed knowledge of the histories of both anthropology and biology, her arguments are, except in quite minor matters, entirely ineffectual and in no sense amount to a counter-refutation. Indeed, I am grateful to Dr. McDowell who, by her very detailed defense of Mead's views, has given me an opportunity to demonstrate in even greater detail than in my book the scientific inadequacy of the doctrines propounded by Boas and Mead in the 1920s and 1930s, as well as those of the latter-day cultural determinists like Bradd Shore, whose formulations about Samoa McDowell, in her defense of Mead, has also extolled with uninformed enthusiasm.

My personal relationship with Margaret Mead. To divert attention from the major scientific issues with which my book is concerned, McDowell has, quite inaccurately, claimed that it is really an "attack on Mead." In fact, in the preface to my book, in emphasizing my "high regard" for "many of the personal achievements of Margaret Mead," I specifically note that my concern is with the scientific import of her Samoan researches, and "not with Margaret Mead personally, or with any aspect of her ideas or activities that lie beyond the ambit of her writings on Samoa."[2]

In a scientific refutation of the kind I have essayed one has, perforce, to deal with the statements of another individual. One is, however, dealing only with these statements, and *not* with the individual who originated them. Blame is thus in no way involved.

In science, the efficacy of a refutation depends solely on the authenticity, relevance, and cogency of the evidence adduced. Thus in my book (the structure of which McDowell has failed to comprehend) my refutation of Mead's depiction of Samoa and of her conclusion of 1928 precedes and is *logically quite separate from* my subsequent discussion of the likely causes of Mead's misconstruction of Samoa. This, I would emphasize, is because any discussion of the likely causes of an error, while potentially of heuristic value, has no direct bearing on a successful refutation because it adds no relevant evidence.

And here again blame is not an issue, nor can a refutation be justly construed as a personal attack, as McDowell would have it. Indeed, in science, as Popper has emphasized, an individual whose conclusions have been refuted has, by virtue of this fact, contributed in a fundamentally important way to the course of scientific progress.

To exemplify this fact and to rebut McDowell's unwarranted aspersion, let me briefly record the course of my personal dealings with Dr. Mead.

I first met Dr. Mead in 1964 when, during a long and formal private conversation in the Research School of Pacific Studies of the Australian National University, I placed before her the evidence that had led me, as early as 1943, to reject the conclusion she had reached in *Coming of Age in Samoa.*

Immediately after this meeting I wrote to Dr. Mead as follows:

> It is plain to me that our conclusions about the realities of adolescent and sexual behavior in Samoa are fundmentally at variance. For my part I propose (as in the past) to proceed with my researches with as meticulous an objectivity as I can muster. This, I would suppose, is going to lead to the publication of conclusions different from those reached by you, but I would very much hope that, however we may disagree, there should be no bad feeling between us. You have my assurance that I shall strive towards this end.

Dr. Mead replied in a letter dated New York, 2 December 1964, that ended with the exemplary words "what is important is the work." During our subsequent correspondence, which extended from 1964 to 1978, Dr.

Mead continued to behave in this exemplary manner. In a letter to the *New York Times* of 13 February 1983, Mary Catherine Bateson observed that although her mother "was vehement in defense of her views, she did not descend to 'the clangorous exchange of insult' precisely because she believed that anthropology was evolving in her lifetime toward an increasingly exact science and that science is everywhere the cumulative work of many minds."

In my judgment it is precisely because Margaret Mead held these views and because she grappled, throughout her life, with anthropological problems of fundamental importance that she is assured an honored and secure place in the history of anthropology.

On the characteristics of an interactionist approach. In the final chapter of my book, having indicated at least as far as Samoa is concerned the inadequacy of the extreme form of cultural determinism that was adopted by Kroeber, Lowie, Boas, Benedict, Mead, and others, I adumbrate the essentials of a more scientific, anthropological paradigm based on the recognition of both cultural and biological variables and their interaction. Unlike the paradigm with which cultural determinists have long operated, in which all biological variables are totally excluded from consideration by arbitrary fiat, an interactionist paradigm makes no such unscientific, a priori assumption, but recognizes, in any particular case, *all demonstrably determining variables,* be they cultural or biological, without any prior assumption as to their relative importance.

This is a scientific point of view that McDowell, as an inured cultural anthropologist, quite fails to appreciate. It is, for example, a complete non sequitur to suppose that recognition of the biological dimensions of human behavior, as in, say, Richard Passingham's *The Human Primate,* in any way involves what McDowell calls "a Hobbesian view of human nature."[3]

Again, she unthinkingly dismisses the revealing instance of the respect language of the Samoans that I give in my book (1983:300) by claiming that "social dominance" has nothing to do with biology, whereas anyone with the most casual acquaintance with the relevant scientific literature (as, for example, D.R. Omark, F.F. Strayer, and D.G. Freedman, eds., Dominance Relations, New York and London, 1980) will know that social dominance is very much a part of human ethology. And, most extraordinarily of all, while fully admitting that "although the refutation of Mead's data does not logically require that Freeman present biological evidence," she nonetheless quite illogically complains that my refutation (based as it is on an interactionist approach) relies, in part, on "cultural data." In fact, it is precisely because Mead's conclusion of 1928 was based on highly inexact and incomplete cultural data that the use of such data is directly relevant to the refutation of her erroneous conclusion. As an interactionist it is wholly in order for me to adduce whatever evidence I choose in my refutation of Mead as long as it is both authentic and relevant. Further, McDowell is also mistaken in asserting that, as an interactionist, I am "without a theoretical means of accounting for the diversity of behavior apparent between human groups." I shall return to this crucially important issue in the concluding section of this rejoinder.

On non sequiturs and American physical anthropology. I next come to McDowell's ill-informed assertions about what she mistakenly supposes to be my ignorance of American physical anthropology. She begins with the breathtaking non sequitur that because I was trained in Cambridge, I am "therefore" unfamiliar with American physical anthropology. The fact of being trained in Cambridge (or, for that matter, anywhere else) in no way necessarily involves an unfamiliarity with American physical anthropology. In fact, before I undertook my doctoral studies at King's College, Cambridge, I had been trained in anthropology--first, in the late 1930s, at Victoria University College in the University of New Zealand, and then from 1946 to 1948 at the University of London, when, in the course of my other training, I studied biological anthropology with Dr. N. Barnicot at University College, London.

In New Zealand, my principal adviser was Dr. Ernest Beaglehole, who had studied anthropology at Yale University under Sapir, himself a student of Franz Boas. As Gladwin (1961:148) has noted, the emphasis of Beaglehole's anthropology was "in many ways similar to that of Mead." I was thus exposed to the Boasian approach to anthropology from the very outset of my anthropological career. Indeed, in Samoa in the early 1940s I had with me, and systematically studied, the 1938 first edition of *General Anthropology,* a textbook edited by Boas and containing chapters by Boas himself as well as by Benedict, Bunzel, Lowie, and other Boasians.

Since that time I have taken a close interest in all aspects of American anthropology, including American physical anthropology, a field I have been familiar with (quite contrary to McDowell's ill-informed assertions) for more than forty years.

On Boas and cultural determinism. McDowell, in her ignorance of what is involved in interactional thinking, claims that because physical anthropology is represented in what she calls the "four-field approach" of American anthropology, "the interaction between biology and culture has always been important." This is a false and misleading claim. As Stocking (1968:264) has documented, the "whole thrust" of Boas' thought was to "separate biological and cultural heredity." [4] It was this separation in description, analysis, and explanation that Kroeber and Lowie, by arbitrary fiat, made the basic assumption of cultural anthropology in 1917. This has meant that while American cultural anthropologists may, in the course of their preliminary training, have had some elementary instruction in physical anthropology, they thereafter operate, with but few exceptions, with the assumptions of the Boasian paradigm and, lacking any training in human ethology, actively ignore the possibility that ethological variables might well be among the determining variables of the phenomena they purport to explain. In other words, they have no theory of human nature and unscientifically assume that human behavior can be fully explained in cultural terms.

In my book I document that it was Franz Boas and his disciples who, during the first four decades of this century, established and actively promulgated the doctrine of cultural determinism. McDowell asserts that in so doing I have failed to recognize "the significant and positive role" of Boas in "establishing the importance of biology" in anthropology. This belief that Boas, during the very decades that saw the formation of the doctrine of cultural determinism, was, at the same time, a positive proponent of biology is, as I shall show, yet another myth.

On baseless accusation. First, however, let me deal with McDowell's baseless accusation that I have knowingly distorted certain of Boas' words. In a supposedly "astounding"' culmination to her opposition to my depiction of Boas, McDowell draws attention to a passage (1983295) in which I note that even as late as 1939, Boas thought that in regard to the human body "a search for genes would not be advisable," as there was some danger that the number of genes would "depend rather upon the number of investigators than upon their actual existence." McDowell then reveals that my quotations of Boas' words do not come from Boas' paper of 1939 "Genetics and Environmental Factors in Anthropology," but "incredibly" from an article entitled "The Tempo of Growth of Fraternities," originally published by Boas in 1935.

Why this situation should be considered incredible I am at a loss to understand. In the note referring to the paragraph in question (1983:359) I cite both Boas' paper of 1939 (in support of my comment on page 95 that he was "opposed to research in human genetics" (an interpretation I shall presently substantiate further) and also the version of his article, "The Tempo of Growth of Fraternities," that was republished in 1940 in a volume entitled *Race, Language and Culture,* while noting it had originally appeared in 1935. The preface to this volume by Boas is dated Columbia University, 29 November 1939. It is thus evident from this fact (as well as from everything else known of Boas' attitude toward genetics from

1935 onward), that in 1939 he still stood by the views expressed some four years earlier in the concluding paragraph of his paper on "The Tempo of Growth of Fraternities." Thus I am *fully justified* in claiming that Boas held the view I have attributed to him about genetics "as late as 1939."

McDowell also accuses me of what she calls a "travesty of scholarship," saying that I have deliberately distorted Boas' attitude toward genetics because Boas recognized in his paper of 1935 that heredity was a variable in the tempo of growth. This accusation I completely reject. If only McDowell had made an informed study of Boas' ideas and attitudes during the last ten years of his life she would know that Boas' notions of heredity were (as I shall presently document) of a decidedly peculiar nature, and that throughout these years, in clinging obdurately to a belief in Lamarckian inheritance, Boas also maintained his long-standing prejudice against both evolutionary biology and the science of genetics.

It remains my view then that the passage in which Boas expressed his scepticism as to the "actual existence" of genes is a clear example of his general prejudice against genetics. Dr. McDowell's far from well-informed defense of Boas affords me an opportunity--which I shall now take--to discuss Boas' attitudes toward genetics and evolutionary biology more fully than was possible in my book. As we shall see, McDowell's wild accusations of deliberate distortion and poor scholarship are groundless and without justification.

On biological research during the last four decades of Boas' life. In support of her claim that Boas played a "significant and important role" in "establishing the importance of biology" in anthropology, McDowell cites Hrdlicka and Krogman on Boas' contributions to physical anthropology. As Krogman notes (cf. Herskovits 1943:39), Boas made notable contributions to the study of race, growth, and development, and to biometrics. These fields, however, are very much peripheral to biology proper and to assess McDowell's claims it is necessary to view Boas and his beliefs in the context of the history of biology, especially during the scientifically momentous first four decades of the twentieth century.

Before he went to America Boas had gained, mainly from Waitz, a belief in Lamarckian inheritance and, from Virchow, a marked disbelief in and antipathy to the theory of biological evolution (see Freeman 1983: ch. 2). These then were the attitudes toward the great biological issues of the day that Boas had firmly espoused by the time he became professor of anthropology at Columbia University in 1899.

The following year three different biologists, de Vries, Correns, and von Tschermak (all of them engaged in studies of plant hybridization) stumbled on Mendel's classic paper of 1866 and what Garland Allen

(1972:v) has called "the age of genetics" began. Boas was to live until December 1942, and so the last forty-two years of his professional life saw both the formation of the science of genetics and the emergence from the early 1930s onward of the evolutionary synthesis (cf. Mayr 1982:567), both of which are central to modern biology. It thus becomes possible to assess Boas' attitudes in relation to these historic events.

As early as 1902 Sutton (Allen 1979:56) had pointed to "the strong similarity between Mendel's hypothesis of segregation and the microscopically observable separation of homologous chromosomes during meiosis." By 1910 it had become evident that "chromosomes were cell structures that acted as the vehicles of heredity," and over the next five years T.H. Morgan and his associates, working in the same university as Boas, in a series of brilliant experiments laid the foundations of modern genetics. In particular, in their book *The Mechanism of Mendelian Heredity* (1915), Morgan and his associates developed the idea that "factors," in Mendel's sense, were physical units (or genes) located at definite positions (or loci) on chromosomes, and by 1920, as Allen notes (1979:65), these discoveries were "almost fully accepted throughout the biological community."

In his 1926 work *The Theory of the Gene,* Morgan presented further evidence to show that the gene represented "an organic entity" (1926:321). In reviewing this book Jennings (1927:184) noted that the day had passed when with respect to heredity "one man's fancies seemed as good as another's"; Dunn (1927:24) remarked that "the theory of the gene or of inheritance by discrete units" was as secure as any was likely to be and was "ready to take its place as one of the major generalizations of biology."

These major advances in genetics also had a profound effect on the theory of biological evolution so that as Huxley (1949: 12) has noted, "about 1920 biologists began to be interested in how natural selection would operate on organisms with Mendelian (particulate) inheritance, and started applying mathematical methods to the problem." This problem was effectively solved with the publication in 1930 of Fisher's *The Genetical Theory of Natural Selection,* which was followed in 1937 by Dobzhansky's *Genetics and the Origin of Species,* a book which, as Mayr (1982:569) records, signalled the decisive emergence of the synthetic theory of biological evolution.

All of these crucial advances within biology had occurred *before* the appearance in 1938 of the textbook *General Anthropology* (which was edited by Boas, and contained a section written by him on the "biological premises"of anthropology) and of the second edition of Boas' *The Mind* *of Primitive Man,* originally published in 1911. It is thus possible to gauge with some precision the way in which Boas reacted to the seminal developments within biology that had taken place during the years of his professorship at Columbia University between 1899 and 1937.

The scientific status of Lamarckian theory in the 1930s. First, however, let me refer briefly to the way in which the attitude of informed biologists toward Lamarckian inheritance (in which Boas had long believed) had changed during these same years. Although belief in Lamarckian inheritance had not been uncommon during the first decade of the twentieth century and lingered on in some quarters into the 1920s and beyond, among the vast majority of biologists it did not survive the epoch-making researches of T.H. Morgan and his associates to which I have already referred. Thus, in an article on Lamarckism; published in the fourteenth edition of the *Encyclopaedia Britannica,* Morgan (1929:609) noted that "the most complete disproof of the inheritance of somatic influence is demonstrated in almost every experiment in genetics," and concluded "the facts are positive and unquestioned and contradict thoroughly the claim that germ cells are affected specifically by the character of the individual." And the following year, H.S. Jennings, the Henry Walters Professor of Zoology at Johns Hopkins University, in his book *The Biological Basis of Human Nature,*[5] referred (1930:342) to the fact that by that time an experimenter who put forward a claim that he had "proof of the inheritance of acquired characters" was classified "in the 'lunatic fringe' of biology." It was to this "lunatic fringe," as I shall show, that Boas belonged throughout the 1930s and until his death in 1942.

On Boas' attitudes toward biology. In deploring my depiction of Boas' doctrines, McDowell asserts that I take "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." She then accuses me of trickiness for claiming that the Boasians had "an antipathy to biology and to genetics and evolutionary biology in particular."[6] In this assertion and this accusation McDowell is quite mistaken, and, as one who has (unlike herself) seriously studied the relevant historical evidence, I reject as baseless her accusation of trickiness.

Kroeber, who at the time he was formulating his doctrine of absolute cultural determinism went so far as to refer to those "infected with biological methods of thought" (1916:34), knew Boas well and has recorded that Boas "was not much interested in biological evolution or in genetics both of which he used or related to his own work very little" (1956:156).[7] This is an understatement, for although Boas must have had some inkling of the momentous advances that took place in the theory of biological evolution during the first four decades of the twentieth century, he was antipathetic toward this theory and to evolutionary theory generally throughout the period that he exerted such a decisive formative influence over American anthropology. Thus Radin, another of Boas' students, has recorded (1939:305) that Boas "always took a prevailingly antagonistic position" to the theory of evolution, while Stocking (1968:184), having made a study of the relevant historical evidence, states that Boas was "quite skeptical of natural selection"--the central mechanism of biological evolution discovered by Charles Darwin.

Another measure of Boas' attitude toward biology is his virtually total neglect of the writings of Charles Darwin. Kluckhohn and Prufer (1959:22), in their study of "influences" on Boas during his "formative years," noted that the only citation from Darwin that they had discovered in all of Boas' writings was to *The Voyage of the Beagle.* This, furthermore, was only a reference (Boas 1963, orig. 1911:134) to how a Fuegian, after a sojourn in England, had fallen back "into the ways of his primitive countrymen." There is, in fact, a brief mention of Darwin in Boas' chapter on race in *General Anthropology* (1938:116), though only in the course of a dismissive discussion of natural selection.[8] This book appeared after the publication of Dobzhansky's *Genetics and Origin of Species,* at a time when Boas, had he been in touch with biology, would have had to take an altogether different stance. Again, Boas' *The Mind of Primitive Man,* a second edition of which appeared in 1938, contains a long chapter on "The Emotional Associations of Primitives" in which there is no mention at all of Darwin's classic work of 1872, *The Expression of the Emotions in Man and Animals.*

Boas' pronounced lack of interest in Darwin and the theory of evolution by means of natural selection was actively communicated to others, for Boas was, as Vidich wrote, "personally a powerful figure who did not tolerate theoretical or ideological differences in his students" (1966:xxv). Indeed, Mead herself, in a vivid phrase, described how Boas' influence "spread through American anthropology like an animated veto" (1969:345). And part of this influence, as is evident from his writings, as from other sources, was most certainly an antagonism toward both biological evolution and evolutionary theory in general. Thus Kluckhohn and Prufer (1959:22) record that Boas' students reported that he "did not discuss biological evolution in his seminars," and so marked was his influence that as Professor J.J. Williams noted in 1936, Boas had by that time succeeded in "suppressing the classical theory of evolution among practically the entire group of leading American ethnologists."

Boas' prejudice against genetics. Boas' "prevailingly antagonistic position" toward evolutionary theory on which Radin remarked in 1939 was joined by what Kluckhohn and Prufer (1959:22) have called "a skepticism about Mendelian heredity." Again, in recording that "a relative lack of interest in experiment remained with Boas all his life, and seems to have been a deep-seated quality of his mind," Kroeber (1943:7) also noted that Boas was "long inclined to be suspicious of Mendelian heredity, evidently trusting more in statistical analysis than in experimental findings on selected characters."

These deeply seated attitudes, it is important to realize, were retained by Boas as long as he lived and in the face of decisive scientific evidence to the contrary. As I have already indicated, by about the mid 1930s the science of genetics had, through a series of elegant and precise experiments conducted during the previous two to three decades, decisively illumined the problem of heredity, and Morgan, in recognition of his work in establishing the chromosome theory of heredity, had in 1933 been awarded the Nobel Prize in physiology.

In his book *The Physical Basis of Heredity* Morgan had demonstrated that the presence of genes in chromosomes was "directly deducible" from his experimental results (1919:237), a conclusion (as I have already noted) he further explicated in 1926 in *The Theory of the Gene.* Thus by 1930 Jennings, in surveying the progress of genetics during the previous three decades, could write "positive and inescapable experimental evidence proves that the chromosome is a structure composed of many diverse parts, each part, or gene, having a definite effect on development, and therefore a definite effect on the characteristics of the individual produced" (1930:73).

However, despite the "positive and inescapable experimental evidence" that had been widely published by the 1930s there were still obscurantists, Boas among them (most of them idealists who were opposed to the materialistic implications of genetic research), who, in defense of their own antiquated beliefs, argued that genes were no more than figments. It was to this supposition that Boas gave voice in 1935 in suggesting that if genetic methods were applied to the study of human growth there was a danger that "the number of genes" would "depend rather upon the number of investigators than upon their actual existence." When it is understood in historical context, this remark by Boas in a serious scientific paper is the clearest evidence of that antipathy to genetics which, as we know from other evidence, colored his thinking throughout the 1930s.

By 1930, through the researches of Landsteiner and others, it had become apparent, as Jennings put it, that humans have "the same genetic system, operating in the same manner, as have other higher organisms," and further, that for many human characteristics, there was "no doubt of the applicability of modern genetic science" with these characteristics "being inherited in the same way as are the characteristics of other organisms" (1930:154ff.).

It was to these propositions (that have been fully substantiated by subsequent research) that Boas was most rootedly opposed, as is evident in a brief article he contributed to the November 1939 issue of the journal of the New York Association of Biology Teachers. According to Boas (1939:17ff.), although the study of genetics had "attracted so much attention in recent times," the subject received "perhaps more attention" in the school curriculum than a "well rounded presentation of the facts of biology" justified. There was little doubt, Boas thought, that as time went on and the novelty of the study of genetics wore down, other aspects of "the problems presented by life" would receive "greater attention." It was "particularly unfortunate," Boas felt, that "the data of genetics obtained from the study of lower forms are too readily applied to man." "The application of genetic data to man," Boas declared, should, on account of its social implications, "be made most guardedly."

These statements are direct expressions of the suspicions about Mendelian heredity and the "actual existence of genes" that ruled Boas' thinking throughout the 1930s. They are evidence, in my judgment, both of an antipathy to genetics in general and of opposition to research on humans based on Mendelian principles.

Professor Boas and the woodpeckers. Among the principal arguments that Boas advanced against the "application of genetic data to man" was the supposition that "man cannot be compared to wild animals," as man is a "domesticated form" who has undergone modification in the "process of domestication." As I show in chapter two of my book, Boas was much influenced in his anthropological thinking by Theodore Waitz, an out-and-out Lamarckian. Further, as Kluckhohn and Prufer (1959:22) show, Boas persisted in his belief that Lamarck "was still to be reckoned with" as long as he lived, even though by the late 1930s the evidence of experimental biology had shown Lamarckism to be an unscientific doctrine.

In 1932, in the course of his presidential address to the American Association for the Advancement of Science, Boas (1940, orig. 1932:246) asserted with reference to humans as well as to some other animals, "one series of changes brought about by external conditions are undoubtedly hereditary . . . those developing in domestication." And some years later, in 1938, in the second edition of *The Mind of Primitive Man,* in arguing for his essentially Lamarckian theory of domestication, Boas (1963, orig. 1911:87) stated that while this process can be studied "in its results only," the "direct influence of environment may be investigated experimentally and statistically." He then went on to quote at some length from a paper by O.F. Cook, originally published in 1907, an action that reveals convincingly just how extreme an environmentalist Boas was and how much out of touch he was with the biological thought of the late 1930s, which is epitomized in Dobzhansky's *Genetics and the Origin of Species* of 1937.

Cook, said Boas, quoting from Cook's paper of 1907, had made "observations" as follows:

> Zoologists speculate on such questions as whether the eggs of Vancouver woodpeckers, if transferred to Arizona would hatch Arizona woodpeckers or whether the transferred individuals would gain Arizona characteristics in a few generations. What the woodpeckers might or might not do depends on the amount of organic elasticity which they may happen to possess, but the experiment is unnecessary for answering the general question, since plants show a high development of these powers of prompt adjustment to diverse conditions. It is not even necessary that the eggs be hatched in Arizona.

Boas then proceeded, in a way that one would not have thought possible as late as 1938, to assert that Cook's ludicrously unscientific speculation "shows" that the "form" of a "species" is "determined by environmental causes."

The Arizona woodpecker, I am informed by Ernst Mayr (1969:pers. comm.), is now considered a subspecies of *Dendrocapos stricklandii,* while the Vancouver woodpecker (referred to by Cook) is probably a subspecies of the hairy woodpecker. By having in 1938, placed the credence he did in Cook's "observations" as scientific evidence, Boas has given us a telling glimpse of the quality of his biological thought, for to suppose that one subspecies of woodpecker might be transformed into another (in the way suggested by Cook and accepted as possible by Boas) is, in Ernst Mayr's words, "total nonsense." Indeed, in 1969 Ernst Mayr informed me that the paper by O.F. Cook relied on by Boas as proof of the environmental determination of the form of a species was "the weirdest, and most abstruse nonsense" he had ever read.

Such then was the quality of the biological understanding of Franz Boas. When reading the pages of *The Mind of Primitive Man* to which I have just referred, one is impressed anew with the significance of Kroeber's testimony that Boas "was not much interested in biological evolution or in genetics, both of which he used or related to his own work very little." Indeed I am puzzled beyond measure as to what Dr. McDowell, in her secret heart, imagines "the significant and positive role" of Boas--the Lamarckian and extreme environmentalist--might conceivably have been in "establishing the importance of biology" in anthropology.

On Boas' extreme environmentalism. In her ignorance of the paucity of his biological knowledge McDowell asserts that I am "simply wrong" in stating that at the time Boas published the first edition of *The Mind of Primitive Man* in 1911 he was not "disposed to explore, in a constructive way the coexistence and interaction of genetic and exogenetic processes." In fact, the complete absence of any such exploration from the first edition of *The Mind of Primitive Man* fully substantiates my statement. Nor, I would add, was there any trace of such an exploration twenty-seven years later in the second edition of this most influential of Boas' books. Indeed, by 1938, as I have just shown, Boas' extreme environmentalist beliefs had hardened and had 'become even more extreme than they were in 1911. Moreover, because of his lack of knowledge of both genetics and evolutionary biology, Boas was in no position to undertake, at any point in his career, any constructive exploration of the "coexistence and interaction of genetic and exogenetic processes."

Boas and cultural determinism. Yet another of McDowell's errors is her mistaken notion that the argument of my book rests on the supposition that following the propounding in 1917 by Kroeber and Lowie of "a doctrine of absolute cultural determinism that totally excluded biological variables," Boas underwent a "conversion" to this doctrine that was "extreme and profound." This is by no means the case.

As I document in my book (1983:47), in his address on "The Mind of Primitive Man" given during the year following his 1899 appointment to the chair of anthropology at Columbia University, Boas explicitly argued for culture as a construct to which the laws of biology did not apply. He adhered to this view for the rest of his career. During the year before Kroeber and Lowie made their doctrinaire pronouncements, Boas himself (1916:473ff.) declared that it had to be assumed that "all complex activities are socially determined," and that "in the great mass of a healthy population, the social stimulus is infinitely more potent than the biological mechanism." Boas is here directly comparing exogenetic and genetic variables, and his belief that, in general, the first of these two sets of variables is "infinitely more potent" than the second, is but a very short step from the absolute cultural determinism of Kroeber and Lowie with its total exclusion of biological variables. There was thus no occasion for any "extreme and profound" conversion, for Kroeber and Lowie, who were Boas' former students and admiring disciples, had merely taken to its apogee the extreme environmentalism of which Boas had long been a leading advocate.

What I would next emphasize is that the conclusion that biological variables are of no significance whatsoever in the etiology of adolescent behavior, reached by Mead in *Coming of Age in Samoa,* is completely in accord with the doctrine of absolute cultural determinism, with its total exclusion of biological variables, that Kroeber and Lowie had propounded in 1917. Furthermore, it is known from Mead's own testimony that Boas accepted Mead's extreme conclusion without question.

As I have argued earlier in this rejoinder, this conclusion of Mead's is, in scientific terms, preposterous, and the fact that it was fully accepted by Boas is the clearest possible evidence that in this crucial instance Boas was indeed a proponent of absolute cultural determinism. Further, his unqualified acceptance of Mead's extreme conclusion is equally an indication of how little Boas appreciated the biological bases of behavior, a fact that is fully confirmed by the analysis of his other attitudes toward biology (as, for example, his citing in 1938 of O.F. Cook's ludicrous flummery about the woodpeckers of Vancouver and Arizona).

McDowell on "good scholarship." I do not propose discussing in any great detail the section of her review that Dr. McDowell has called "on scholarship." Here, with unmitigated pedantry, she has piled Pelion on Ossa in expressing her disapproval of such scientifically momentous issues as the "citation style" that has been followed in my book, as though this, in some magical way, might lessen the cogency of my refutation of Mead.

I have, naturally, referred Dr. McDowell's criticisms to individuals at Harvard University Press of whose scholarly judgment and editorial skills I have the highest regard. Although their very definite advice to me was *not* to reply to Dr. McDowell's exaggerated criticism, I have decided, because this criticism is to appear in a scholarly journal, to comment briefly on the pedantic stance Dr. McDowell has adopted, beginning with part of the advice I received in this matter from Harvard University Press. My advisers write:

> We are very aware (and Ms. McDowell should be) that no form of citation is perfect, and that any decision to use one form rather than another entails both gains and losses. The forms of citation suggested to you as most appropriate for your book are ones that we and other university presses often use, for example,

> choosing not to have a separate bibliography when the sources are all included in the notes, consolidating notes where possible, not repeating citations for frequently used phrases, and reducing the number of quotation marks for phrases so brief that their use in sentences essentially constitutes a paraphrase rather than a quotation.

It is to these well established and widely accepted editorial practices, all of which I personally accept and for which I take full responsibility, that Dr. McDowell has pedantically objected, as though the laborious procedures to which she has become inured should be obligatory for all.

We reject her pedantic strictures both because we disagree with them in principle and because, when examined in detail, they are seen to be utterly trivial or to have no substance whatsoever.

Some examples of pedantry. McDowell complains that in the second paragraph on page 20 there is only one quotation while in the note to this paragraph on page 308 two sources are listed. The reader is thus left in doubt, so McDowell would have it, as to which is the source of the quotation In fact, the first source listed is Boas' paper "The Mind of Primitive Man," published in *The Journal of American Folklore* in 1901; being listed first, it obviously refers to my reference to Boas' presidential address to the American Folk-Lore Society in December 1900, mentioned in lines six and seven of the paragraph under discussion. In contrast, the quotation appearing in lines twelve and thirteen of this paragraph is obviously from the second source listed, Spier's paper of 1959 in volume 89 of the *Memoirs of the American Anthropological Association.*

Even readers of but middling intelligence would be able to work this out for themselves, but, if they found it beyond their capabilities, they could readily solve what is really no problem at all by consulting the sources listed. I do not recollect, in five decades of academic life, having come across a more trivial complaint than that which has been ponderously elaborated in this instance by Dr. McDowell, nor shall I, I would hope, ever hear a more preposterous accusation than that I am guilty of deliberately obscuring the author of a particular quotation. Of such stuff is the "scholarship" of Dr. McDowell.

McDowell also cites a paragraph from page 99 in which I quote Stocking on the dissemination of Boasian thinking and then give my own views on the significance of Meads assertion, on the basis of her researches in Samoa, about the sovereignty of culture. No intelligent reader could suppose, as McDowell suggests, that this opinion was that of Stocking for there is no continuation of quotation marks.

In a similar fashion, McDowell, in her note 19, criticizes the passage from page 74 of my book, but it is an outright non sequitur to suppose that the quotations in this passage come from *Social Organization of Manu'a;* anyone inclined to make this illogical inference would at once be apprised of his error if he consulted note 24 on page 319.[9]

I had not imagined that anyone could be so pedantic as to enumerate these and other trivialities as examples, as Dr. McDowell would have it, of the violation of the canons of "good scholarship."

Again, to suggest that I should not have cited Mead's clearly stated view of human nature as "the rawest, most undifferentiated of raw material," without also citing the long passage of over 120 words cited by McDowell herself, is, in my judgment, quite exorbitant pedantry. I might as well censure McDowell on the grounds that the 124 words she cites do not adequately convey Mead's meaning in that they are arbitrarily taken from a single paragraph of some 297 words in the course of which Mead states her view of human nature. If one were to behave with the extreme pedantry that Dr. McDowell advocates, the writing of readable books would be impossible. Readers if they wish, may check the sources for my citations themselves and reject the construction I have put on the words in question if they consider this warranted.

I can only say that I have written a book about anthropological issues of great moment that, while it may not, despite my best efforts, be entirely free from minor literal errors,[10] is based on painstaking and honest research. This being so, I reject as unprincipled McDowell's repeated resort to aspersion, as in her use of such epithets as "devious" and "deceptive," which being merely her peculiar personal opinion and entirely unsubstantiated, are examples of the *odium scholasticum* which is both out of place and of no probative consequence in scholarly and scientific controversy.

What does matter, however, in both scholarship and science, is the prefering of evidence over dogma and assumption. If only Dr. McDowell, in 1980, had given attention to the then well-known evidence of the errors in Mead's Samoan ethnography rather than uncritically extolling Meads concern for "precision," "accuracy," and "exactness," I might now have greater regard for her present pontificating on the canons of "good scholarship."

McDowell on Samoa. As is apparent from her apologia for Boas, McDowell has never made a detailed study of the relevant sources, relying instead for her "conclusions" on such secondary sources as Marvin Harris, who himself has no adequate appreciation of Boas' standing in relation to the biological theories of the early twentieth century. When it comes to Samoa, a complex world of which McDowell has no firsthand knowledge, the case is even worse. Yet she has not hesitated to lay down the law about intricate matters of which she knows nothing in particular.

I must now, therefore, deal with the arguments she has put forward in an attempt to evade my refutation, the logic of which she quite fails to understand. For example, it is in no sense my objective, as McDowell mistakenly claims, to have my readers "believe" that Mead was "100 percent wrong" in her account of Samoa. I have simply offered evidence to demonstrate that Mead was not justified in categorizing Samoa as a "negative instance"--and this, as I shall presently show, I had not the slightest difficulty in doing, on either purely internal evidence, or on the contemporary historical evidence for those parts of Samoa in which Mead worked.

The Samoan archipelago. McDowell begins her defense of Mead by wondering about "the comparability of data gathered from different places" in the Samoan archipelago. Although Mead's investigations in 1925-1926 were confined to the islands of eastern Samoa, she fully recognized (Mead 1937:282) that these islands were part of the Samoan archipelago, which prior to European contact was a "closed universe" whose inhabitants conceived of "the Samoan people as all members of one organization." Furthermore, in *Coming of Age in Samoa* (1961:11) Mead specifically notes that "in an uncomplex, uniform culture like Samoa" she felt "justified in generalizing." So, as Richard Goodman (1983:9) has pointed out in his critical study, *Mead's Coming of Age in Samoa: A Dissenting View,* Mead's book contains more than 150 generalizations that are all explicitly about, as Mead puts it without qualification, "Samoans."[11]

That Mead should have generalized about all Samoans in this way is understandable, for although she worked only in eastern Samoa, she had repeated contact with native residents of the western region of Samoa whom she encountered in Tutuila and Manu'a. Indeed, the talking chief Lolo who (as Mead notes in her acknowledgements in *Coming of Age in Samoa*) taught her "the rudiments of the graceful pattern of social relations which is so characteristic of the Samoans," came from Salani, a settlement on the south coast of Upolu in western Samoa. Talala, whom Mead (1977:48) saw a great deal of in Manu'a during the first few months of 1926, came from Mulivai, a village of the Safata district of Upolu to which Sa'anapu, the main site of my own researches, belongs. In these circumstances, one is certainly justified in drawing on appropriately relevant evidence from anywhere in the Samoan archipelago.[12]

The time factor. While she has obviously made no significant study of Samoan history, McDowell raises the issue of the "comparability of data" gathered "at different times," arguing, in defense of Mead, that "surely there was at least some sociocultural change" between the time of the completion of Mead's researches in 1926 and the beginning of mine in the early 1940s. According to McDowell I do "not trouble with this issue."

This is completely untrue. I have studied the history of the Samoans for over forty years, consulting the primary sources wherever possible and giving special attention to the history of the 1920s and 1930s. Thus, when I state that "there is no reason to suppose that Samoan society and behavior changed in any fundamental way during the fourteen years between 1926, the year of the completion of Mead's inquiries, and 1940, when I began my own observations of Samoan behavior" (1983:120), this judgment is based on the most detailed historical research.

Mead herself (1961:273), writing in 1927, considered that "given no additional outside stimulus or attempt to modify conditions, Samoan culture might remain very much the same for two hundred years." No such "stimulus or attempt" was effective during the 1930s, and in November 1937, Roger Duff, of the Canterbury Museum, New Zealand (an expert witness in this matter, who had just returned from two years spent in the Native Affairs Department in Western Samoa), was reported in the Christchurch *Press* as stating that "the outstanding characteristic of the Samoans had been their ethnic resistence to the intrusion of white civilization." "Europeans," said Duff, "had been about the islands for many years but there was no fundamental change in the Samoans principal economic and social customs." Again, Holmes (1957:230) concluded from his comparison of western Samoan culture in the mid-nineteenth century and from his own inquiries in eastern Samoa in 1954 that "cultural change" had been "relatively minimal over a period of a century." So, while considerable sociocultural change has taken place, particularly in American Samoa, during the second half of the twentieth century, my own researches in the early 1940s were conducted (although there had been a higher level of political activity in the 1920s) under conditions that were, in general, very similar to those experienced by Mead only fourteen years or so previously.

The evidence on which my refutation of Mead primarily depends. It is important to realize, however, that my refutation of Mead depends primarily not on my observations in either the 1940s or the 1960s, but on (i) internal evidence, i.e. evidence provided by Mead herself, and (ii) on historical evidence from the 1920s.

The internal evidence, especially that referring specifically to female adolescent behavior, I shall review later in this rejoinder.

My historical evidence for the 1920s is drawn from such unimpeachable sources as the reports of the Royal Commission of 1927 on Western

Samoa and the United States Congressional Commission of 1929-1930 on American Samoa, both of which are concerned specifically with the 1920s, including the period of Mead's researches; from court archives; from contemporary newspapers like *The Samoa Times;* and from the observations of scientists and other investigators like A.F. Judd, Dr. Peter Buck, Francis Flaherty, B. Cartwright, and N.A. Rowe, who were in Samoa (including Manu'a in the cases of Judd and Buck), either at exactly the same time as Margaret Mead or within a few years of her brief stay there. Any reader who has given this historical evidence the attention it deserves will have discerned that it decisively refutes numerous aspects of Mead's romantic depiction of Samoa.

Again, because Mead made unqualified pronouncements on major aspects of Samoan behavior, such as their "unaggressiveness" as she would have it, I have also included in my book a range of historical evidence so that readers can place her pronouncements in historical perspective. Moreover, some of Mead's pronouncements, I would emphasize, were of a historical kind and therefore have to be tested in the light of the relevant historical evidence.[13] So, when Mead, in support of her depiction of the "unaggressiveness" of the Samoans, states without qualification that formerly in Manu'a the "casualties were low" in warfare with "only one or two individuals" being killed, I refute this by showing that, on the contrary, warfare in Manu'a, as elsewhere in Samoa, commonly resulted in a heavy loss of life. For example, in the second half of the nineteenth century, between 1866 and 1871, in a war in Manu'a for which verified evidence is available, some fifty-five men were killed. In comparative terms, this is a very severe loss, representing 11.7 percent of the adult male population of Manu'a at this period.

As this example shows, a consideration of the relevant historical facts is crucially important to my refutation, for, as in this instance, it demonstrates conclusively the extreme inexactitude of some of Mead's statements.

On the memory of things past. As part of her defense of Mead, McDowell asserts that " 'fresh' memories after more than forty years is cause for skepticism." She is here referring to the testimony I collected in Manu'a in 1967 about conditions there in the mid-1920s.

The information I collected, in the Samoan language, was both detailed and specific and came from individuals who, like Mead, in the mid-1920s were in their early adulthood. Some of it was sworn testimony, which had been carefully cross-checked, and is thus of a kind that could be submitted in a court of law. One of my informants described the mid-1920s as being *lata mai nei,* or still close, and I have no hesitation in describing the memories I recorded of the events of that period as "fresh" in the sense that they were still vivid and circumstantial. This, however, should be no surprise to Dr. McDowell, for Margaret Mead in her seventies often wrote and spoke of events that she remembered having taken place in Samoa and elsewhere well over forty years previously.

On the sexual morality of the Samoans. I am, of course, thoroughly familiar with the distinction McDowell makes between ideal and actual behavior, and obviously this distinction is of critical importance in any discussion of the sexual morality of the Samoans.

The gravest defect of Mead's account of this aspect of Samoan life is her failure to report correctly the strictness of the sexual morality of the Samoans, particularly in respect to adolescent girls. [14] In this matter all other observers of Samoa are, to the best of my knowledge, in agreement. In Professor Albert Wendt's words (1983:4), for example, the Samoans in their public morality "forbid premarital and extra-marital sex and promiscuity."

This, it will be noted, is the antithesis of Mead's depiction of Samoa as being (Mead 1959:74) one of those societies that "permit an easy expression of sexuality at puberty," for "permit" is undoubtedly a term with moral connotations and is antithetical in meaning to "prohibit." [15]

That the prohibition of premarital and extramarital sexual intercourse was also the public morality of Samoa in the 1920s (the period to which Mead's writings on the Samoans specifically refer), with sexual intercourse between unmarried persons being held as both a sin and a crime, is demonstrated by cases in the archives of the courts of American Samoa. For example, on 6 May 1929 in the District Court at Fagatogo on the island of Tutuila, Lafitaga, a male, having admitted that he knew it was wrong for a man and woman to have "intercourse with each other unless they were married," was accused of committing "the crime of fornication" by "lewdly and lasciviously co-habiting" with a woman while not being "legally married to her."

Further, this severe sexual morality means that "if an unmarried girl is discovered by her brother in an illicit sexual relationship, he will beat her" (Schoeffel 1979: 168). This is a far cry from the condoned permissiveness that Mead erroneously reported.

Let me at once go on to say, however, that the existence in Samoa of this strict sexual morality does *not* mean that departures from it do not occur, as in the example I have just cited from Tutuila in 1929. In my book I give cases of adultery, surreptitious rape, and the like, in addition to presenting the results of an inquiry into the sexual experience of sixty-seven Samoan girls.

On virginity and adolescent girls. In reporting this inquiry I am said by McDowell to have been "very naive," and its results she has not hesitated to describe as "clearly unreliable." These comments I reject for they have been made in virtually complete ignorance of the issues involved.

Mead herself (as I mention on page 237) reported that in her sample of twenty-five adolescent girls, thirteen, or 52 percent, had had no "heterosexual experience." In order to test Mead's depiction of Samoan sexual mores and behavior, it was obviously important to repeat the kind of inquiry she had undertaken in 1925-1926.

I did this toward the end of 1967 for a sample of sixty-seven girls varying in age from twelve to twenty-two years, and all members of a village in Upolu, Western Samoa. At the time I conducted this inquiry I had been studying the village community in question over a period of some twenty-five years and had recorded and analyzed the family and kinship relationships of its members, many of whom had become my close friends. Furthermore, during that particular period of field research, I had been continuously resident in the village in question for over twenty months, and had numerous sources of information, young and old, male and female, with all of whom I was able to communicate freely in the Samoan language. My method was to make separate, discreet, and repeated inquiries about each of the sixty-seven individuals in my sample, and if, from any of my diverse sources of information, there was *any* indication of "heterosexual experience," the girl in question was listed as a nonvirgin. In other words, I took fully into account not only the overt status of the girls in question as members or nonmembers of the Ekalesia, but also all other reports, including rumors.

In any such inquiry, as in all investigations of intimate sexual behavior, there is obviously an ever present possibility of error, for no one can be privy to the clandestine behavior of others, and it is always open to individuals to lie about that which they wish to conceal. These, however, are possibilities of which I was well aware and did all that I could to circumvent.

Samoan society takes an intense and widespread interest in the virginity of adolescent girls, so that if there is the slightest evidence that a girl has had sexual contact with a male this very swiftly becomes public knowledge. Further, if a rumor of such contact is maliciously false it is commonly contested, also in public. For example, R. B. Lowe, who was the governor of American Samoa from October 1953 to October 1956, has reported a case in which am argument developed between two families regarding the virginity of a girl belonging to one of them. "The father of the girl," Lowe reports (1967:72), insisted that the Attorney General make a statement to the effect that the girl was virgin." "This the Attorney General could not without more evidence than that brought to him by the statements of the father and the girl, so that the girl was sent," according to Lowe, "to hospital where it was established that she still retained her maidenhead., and thus she was able to become a certified virgin."

This example, like that which I give on page 233 of my book, is evidence supporting the statement made at the constitutional convention of Western Samoa in 1954 that, compared with Samoa there is "no country under the sun," where "the question of virgins" is "so upheld." This concern with virginity, and especially with the virginity of adolescent girls, is very much connected with the prohibition on premarital sexual intercourse remarked on by Professor Albert Wendt and is further evidence that Mead, somehow or other, fundamentally misreported the realities of Samoan sexual mores and behavior. [16]

It can be fairly stated that my inquiry of 1967 was conducted with both systematic care and a keen awareness of methodological and other difficulties, and that the results (while they do, as in the case of all such inquiries, contain the possibility of some degree of error) are pertinent as an approximate indication of the likely parameters of the phenomena under investigation.

The sexual experience of adolescent girls. I would particularly note that the inquiry just discussed also produced (as shown in the table on page 239 of my book) information on the extent to which adolescent girls break the prohibition against premarital intercourse, information McDowell mistakenly asserts I "never" consider. In fact, my inquiry indicates that premarital intercourse has been engaged in by about 20 percent of fifteen-year-old girls, about 30 percent of sixteen-year-old girls, and about 40 percent of seventeen-year-old girls. Thus, while in Wendt's words the sexual morality of the Samoans prohibits premarital sex and promiscuity, it is evident that departures from this strict morality do occur and to a far from inconsiderable extent. It is, however, crucial to realize that these departures are viewed--in terms of the public morality of the Samoans--as *illicit,* and are liable, if detected, to *social disapproval and punishment,* a situation that most certainly generates "storm and stress" in the lives of numerous Samoan adolescents. Samoa is thus very far from being, as Margaret Mead erroneously reported, a libertarian sexual paradise where dalliance is all.

On the value of quantitative statements. McDowell tells us that in the appendix to *Coming of Age in Samoa* Mead took the view that the "numerical data" garnered from her sample of adolescent girls "were not suitable for quantitative analysis."

This is an opinion that I totally reject. Mead's conclusion in *Coming of Age in Samoa* that biological variables are of no significance in the etiology of adolescent behavior turns on what is plainly a quantitative statement: the assertion that "storm and stress" is, for the effects and purposes of Mead's inquiry, *absent* from the behavior of Samoan adolescent girls. However, as will be apparent to any percipient reader of *Coming of Age in Samoa,* this assertion is achieved by Mead's having relegated those cases in which disturbance did occur to a separate chapter and by then totally failing to make any quantitative statement about the rates of disturbance and delinquency in the sample she was studying.

This extraordinary maneuver must surely rank as one of the most unscientific to be found anywhere in the literature of the behavioral sciences, and its exposure makes it clear that Mead's main conclusion can be refuted on purely internal evidence.

It is, therefore, understandable that McDowell, in her defense of Mead, should once again attempt to deflect attention from this reality by accusing me of what she quaintly calls "statistical shenanigans." In fact, I have simply posed that most pertinent of questions: How does the rate of delinquency existing in Mead's own sample of twenty-five adolescent girls compare with the delinquency rates for adolescent girls in other countries? All that we are interested in is an approximate comparison as a test of Mead's claim that adolescence in Samoa is free from "storm and stress"; to achieve this I extrapolated a rate from Mead's sample of cases. This procedure is certainly preferable to a merely qualitative comparison and is justified because no precise conclusion is based on it, only a very general comparison.

Such extrapolations are, moreover, a standard procedure. For example, in a paper entitled "The Alleged Lack of Mental Diseases among Primitive Groups," that was published in the *American Anthropologist* in 1934 and based on information contained in *Coming of Age in Samoa,* Ellen Winston (1934:236) wrote: "Considering the five definite cases for Manu'a in terms of a population but little in excess of two thousand individuals, we arrive at a rate of mental disorder of between 225 and 250 per 100,000 of population." Winston then went on to note that in the rural U.S.A. there was a rate of mental disorder of "approximately not more than 100 per 100,000 or about the same as that of Manu'a."

When delinquency rates based on Mead's own data are compared in this same general way (as in chapter 17 of my book) it is at once revealed that adolescence in Samoa is quite as disturbed, on this criterion, as adolescence in Western society, and Meads improbable assertion that in respect of adolescent behavior Samoa was a "negative instance" is seen to be unfactual. [17]

Lowell D. Holmes and Margaret Mead. In her note 37 castigating my attitude toward science, namely that scientific knowledge progresses as we succeed in eliminating error from our formulations, McDowell comes to the defence of Lowell D. Holmes. As a graduate student in anthropology from Northwestern University, Holmes did fieldwork in Manu'a in 1954, and in his Ph.D dissertation of 1957 asserted that the reliability of Mead's account of Samoa was "remarkably high." McDowell would have it that I treated Holmes "shabbily" in my book. I reject this accusation. As I thoroughly document (1983:104), Holmes' ethnographic reports, based on his fieldwork in 1954, provide "substantial grounds" for seriously questioning the validity of Mead's classing Samoa as a "negative instance." Indeed, I would argue that the evidence reported by Holmes in the 1950s indicates clearly that Samoa was definitely not a "negative instance" in the sense that Mead claimed.

In 1961, Professor Donald Campbell (1961:340) of Northwestern University observed that Holmes' findings were in "complete disagreement" with several of the broader aspects of Mead's account of Samoa. These differences, in Campbell's judgment, could not be explained by cultural change between 1926 and 1954 but had to be interpreted as "disagreement in the description of 'the same' culture."

Here then was a scientific issue of major importance. In 1967, having made a detailed study of Holmes' Ph.D dissertation, I drew his attention to a long list of the facts (reported by him) that were markedly at variance with Mead and inquired how, given these facts, he could possibly assert that the "reliability" of Mead's account of Samoa was "remarkably high."

Holmes replied (1967, pers. comm.) that while he disagreed with Mead on "many points of interpretation," he did believe that "the majority of her facts were correct." He then went on to state (these being his exact words): "I think it is quite true that Margaret finds pretty much what she wants to find. While I was quite critical of many of her ideas and observations I do not believe that a thesis is quite the place to expound them. I was forced by my faculty adviser to soften my criticisms." To which he added: "The only tragedy about Mead is that she still refuses to accept the idea that she might have been wrong on her first field trip."

We are here concerned with anthropological issues of quite fundamental importance.

Being one who believes, with Bronowski (1956:66) that in science "the test of truth is the known factual evidence" and that in respect of this most crucial of all scientific values "no glib expediency" can justify "the smallest self-deception," I was then and am now appalled by Holmes' extraordinary admission. Indeed, his admission made it crystal clear to me in 1967 that both for the sake of Samoan studies and of anthropology it was vitally important for me to continue with my investigation of the whole context of Mead's Samoan researches. And I felt it was equally important to publish my findings when they were complete whatever might be the opprobrium and vilification from those for whom prophecy would have failed. In fact, the opprobrium and vilification on the part of some cultural anthropologists has indeed been intense, but my integrity, I would hope, remains intact, and I in no way regret behaving in this whole affair as I have behaved.

The archives of the High Court of American Samoa. As one who makes such a fuss about scholarship, McDowell should know that newspaper reports cannot be relied upon *unless independently verified.* Yet she does not hesitate to place reliance (for the purpose of impugning my veracity), on a report in the *New York Times* that is, in fact, garbled to the point of being completely false.[18]

If McDowell had referred to the preface to my book rather than such newspaper reports she would have discovered that the only claim I make there is that the researches on which my book is based "were not completed until 1981, when I finally gained access to the archives of the High Courts of American Samoa for the 1920s."

Because of the unorganized state of these archives there was no prospect in the time available to me of extracting statistical information, nor was this my objective. I was primarily seeking cases relevant to various of Mead's assertions about the sexual mores and behavior of the Samoans in the 1920s. These I did find, and they were by no means "all tangential" as McDowell, in her ignorance of things Samoan, has asserted, but rather of crucial importance in refuting certain of Mead's ethnographic errors, as will become apparent in the ensuing sections of this rejoinder.

On rape. McDowell's discussion of rape in Samoa is a particularly revealing illustration of the rhetorical devices of denial and prevarication with which she has sought to evade the cogency of my refutation. I therefore propose to discuss the issue of rape in some detail, citing empirical evidence that provides clear proof of the nature and scale of one of the most glaring errors in Mead's depiction of Samoa.

Mead's stance on rape in Samoa. According to McDowell, Mead held that in Samoa rape was "almost unknown." This is a highly misleading reporting of Mead's actual stance on rape in Samoa. In *Coming of Age in Samoa* (1961:93) Mead, it is true, states: "Ever since the first contact with white civilization, rape, in the form of violent assault, has occurred occasionally in Samoa." Here, however, Mead is specifically associating such rape as may have occurred in Samoa with the presence there of European males. That this was her view is confirmed by the quite unequivocal generalization she made in the very year *Coming of Age in Samoa* was published (Mead 1928:487): "The idea of forceful rape or of any sexual act to which both participants do not give themselves freely is *completely* foreign to the Samoan mind" (emphasis added).

Mead repeated this generalization in 1950 in *Male and Female,* asserting of the Samoans (Mead 1962, orig. 1950:220): "Male sexuality was *never* defined as aggressiveness that must be curbed, but simply as a pleasure that might be indulged in, at appropriate times, with appropriate partners" (emphasis added).

I would add that this unequivocal view that aggressiveness and rape were completely absent from the sexual behavior of Samoan males was also affirmed by Mead in her conversation with me in 1964, and again in correspondence in 1967. Here then we have a definite case of an unambiguous assertion by Mead, of a supposedly factual kind, that is central to her depiction of Samoa as a "negative instance" and so basic to her general conclusion of 1928.

It is a view that I myself, giving credence to Meads account, accepted as factually correct at the outset of my own researches in Samoa. Very soon, however, I became aware from newspaper reports of convictions for rape in the High Court of Western Samoa (as, for example, in the *Western Samoan Mail* of 28 September 1940 and 18 January 1941) that rape *was* indeed part of the behavior of Samoan males, and when I began to attend courts *(fono manu)* in Samoan villages, I quickly discovered that rape--both forcible and surreptitious--was, in fact, quite common. Moreover, it was apparent from reports of the proceedings of the High Court in the newspapers of those years, that cases of rape had occurred in Western Samoa throughout the 1920s. I therefore sought out Samoans who had lived in American Samoa, including Manu'a, to inquire if rape had occurred there in the 1930s and 1920s. I was assured that it had, and this assurance has been fully substantiated by all of my subsequent research, including my investigations in the archives of the High Court of American Samoa to which I shall presently refer.

On the nature of rape in Samoa. McDowell complains that I do not, in my discussion of rape, make "definitional distinctions." In fact, I cite J. M. Macdonald's *Rape Offenders and their Victims,* where any interested reader may find that "rape is usually defined as unlawful carnal knowledge of a woman by force and without her consent" (1975:24). A somewhat fuller definition may be found in Amir's well known *Patterns in Forcible Rape* in which he states: "as a general rule the term 'forcible rape' means the carnal knowledge of a woman by a man, carried out against her will and without her consent, extorted by threat or fraudulence" (1971:17).

This definition certainly applies to the cases of forcible rape by Samoan males that I discuss in chapter 16 and is in close accord with the definition of rape under Samoan law. For example, in the Criminal Laws of 1892 of the Malietoa Government that related specifically to "offences of Samoans, not those of foreigners," it is laid down that "if any man goes by force to a woman or deceives her that she may go with him, but the woman is not thoroughly consenting, this is rape."

Although there are no statistics available for the nineteenth century, this law of 1892, applying only to Samoans, is evidence that rape was, in the nineteenth century (in contradiction of Mead's assertion of 1928) clearly recognized as existing in Samoan society. The penalties for forcible rape in 1892 were imprisonment for "not less that four nor more than eight years, with or without hard labour," or, if the body of the woman was "injured," imprisonment for "lifetime or ten years." As these penalties indicate, rape is widely regarded with abhorrence in Samoa.

In a vain attempt to defend the inaccuracies of Mead's account of Samoan sexual behavior, McDowell (note 41) has gone so far as to argue that rape in Samoa is different from rape elsewhere because "Samoan men are trying to acquire wives." As Amir (1971:131) has remarked "rape has many motives but only one intent," and the fact that some Samoan rapists have, as their motive, so they say, the acquiring of a wife, does *not* mean that the rapes they commit are not rapes in the full sense of Amir's definition.

Thus, my study of a sample of thirty-two cases of forcible rape and attempted rape showed that while threat is very occasionally sufficient to enable a Samoan rapist to carry out his criminal intent, there is, in over 90 percent of the cases, a bodily attack on the female victim. In not one case in this sample, let me add, did forcible rape result in the acquiring of a wife. Not infrequently a rapist's attack results in the infliction of major bodily injury, as I well know from having read, to my distaste, the medical reports on Samoan women who have been the victims of rape. I do not propose to cite any of these distressing reports here as evidence that forcible rape in Samoa is indeed forcible rape, but if Dr. McDowell would like me to send to her a sample report, together with a sworn statement by the victim describing the brutal attack made upon her, I shall at once accede to her request.

I would add that Mead's totally erroneous statements about the absence of male sexual aggressiveness in Samoa have in significant ways impeded the liberation of Samoan women from male sexual violence. In these unfortunate human circumstances I regard it as deplorable that a female cultural anthropologist, as in the case of Nancy McDowell, should in an intellectually and morally frivolous way seek to condone Mead's dangerously misleading errors by a denial of the realities with which Samoan women and girls have to live.

I would further note that the prevalence of rape is a major cause of stress among unmarried Samoan women and especially among Samoan girls. An unmarried Samoan nurse, then about twenty-five years old, with whom I discussed this matter at great length in 1943, said that she could never sleep soundly when staying in a strange village out of fear of surreptitious rape, even when sleeping in a pastor's house. Also in 1943, a girl of fifteen stated that because of her fear of being raped she would never leave the immediate precincts of her village, except in the company of another girl. Indeed, all of the Samoan girls with whom I have discussed this mater have confessed to considerable anxiety at the possibility that they might be raped, and I have observed this *fefe i le toso* (fear of being raped) in Samoan girls as young as eight years of age.

On the incidence of rape in Samoa. When it came to presenting an estimate of the incidence of forcible rape in Samoa I might well have cited the judgment of Sir Charles Marsack, Chief Justice of Western Samoa during the 1950s and early 1960s. In 1964 he wrote of Samoa: "Cases of rape and attempted rape are very numerous, much more so in proportion to the population than in any country of which I have seen the criminal statistics" (1964:91). By the mid-1960s however, criminal statistics on rape had become available in the annual reports of the Police and Prisons Department of the Government of Western Samoa, and I decided to make use of these statistics to give a more exact measure of the incidence of rape in Samoa than Sir Charles Marsack's estimate of 1964. McDowell, who has no firsthand experience of Samoa, has, in her purblind defense of Mead, asserted that the statistics on which I have relied are "very dubious." This can only be described as a gratuitous insult to the Police and Prisons Department of the Government of Western Samoa, in whose methods (which in this case I have studied at close quarters) I have a high degree of confidence.

McDowell also asserts that my comparison of rape rates in Samoa with those in some other countries "violates good scientific methods" as the data I use are "not comparable." This assertion I also reject. In fact, as I have indicated, the definitions of rape in the countries concerned are genuinely comparable, and further, the comparisons I make are only of the most general kind and are intended to do no more than demonstrate that rape behavior exists in Samoa at what is unquestionably a high rate (the figure I cite for Western Samoa in 1966 is that of 60 forcible rapes per 100,000 females per annum) and is not, as Mead erroneously reported, "completely foreign to the Samoan mind."

Surreptitious rape, or moetotolo. In Samoa forcible rape is termed *tosogāfafine* (woman dragging). There is also, however, as I explain in my book (cf. p. 244ff.), a form of rape in Samoa known as *moetotolo* (sleep crawling), often called surreptitious rape in English and classed as indecent assault and a serious criminal offence by the police.

When caught, a surreptitious rapist is severely beaten by the male kin of the female he has raped and then heavily fined by the *fono* of his village. Should he be taken to the government court he is often imprisoned for several years.

This form of rape--which Mead, in her ignorance of the realities of Samoan existence, totally misconstrued, claiming that it "involved no force, only deceit"--in fact involves the forceful manual penetration by a male of a female's vagina without her consent. Thus, moetotolo, or surreptitious rape, like tosogāfafine, or forcible rape, involves what is termed *fa'amalosi* in the Samoan language or the use of force. This fact was noted by the Chief Prosecutor of the Independent State of Western Samoa when I discussed the matter with him in 1978. Furthermore, moetotolo is accompanied about 25 percent of the time by a bodily attack on the female victim, as is shown by my detailed study of a series of cases.

Moetotolo, when carried out in the way I have described, is a conspicuous example of male sexual aggressiveness, and it is thus directly relevant to my refutation of Mead's erroneous account of Samoan sexual behavior to note the annual incidence of this form of rape. Because moetotolo is peculiarly Samoan, no comparisons with other countries are, in this case, possible. However, it is certainly pertinent to note that the two forms of rape found in Samoa produced in 1966 a rate of 160 rapes--either forcible or surreptitious--per 100,000 females per annum, for this is further evidence of the gross inaccuracy of Mead's account, which is part of her fanciful depiction of Samoans as being given to "free love-making."

Rape in Samoa in the 1920s. The figures on rape just noted are from the mid-1960s when the first criminal statistics became available. What was the situation in Samoa during the 1920s--the period to which Mead's assertions about the complete absence of male sexual aggressiveness specifically refer?

Here, as already noted, I have had also to test (by the relevant documentary evidence) Mead's supposition that there may have been during the 1920s a period markedly different from the rest of Samoan history. Prior to 1981 I had established, through my study of reports of the proceedings of the High Court of Western Samoa in *The Samoa Times,* that rape behavior occurred in Western Samoa in the 1920s. I also had obtained a number of statements from Samoan informants that this was also the case in American Samoa. This did not, however, amount to documentary evidence, which I was unable to obtain until October 1981 when I gained access to the archives for the 1920s of the High Court of American Samoa.

Because of the unorganized state of these archives, I was not interested in attempting to compile statistics, but in locating verified evidence bearing on Mead's depiction of Samoan sexual behavior in the 1920s. Nonetheless, the cases I did locate in these archives (which constitute only a small sample of the total number of cases) do prove quite conclusively that--contrary to Mead's claim--rape behavior did occur among Samoans in American Samoa in the 1920s, just as it did in Western Samoa.

For example, my investigation of the proceedings of the high courts of American and Western Samoa established that during the years 1920 to 1929, twelve Samoan males (five of them in American Samoa and seven of them in Western Samoa) were tried and convicted for forcible rape, or (in two cases) of attempted rape. I would add that my study of the pertinent court records for the 1920s is far from complete, and as the majority of rape cases are settled within the villages in which they happen, these totals of rape and attempted rape are certainly only a minor proportion of the cases that occurred in Samoa in the 1920s.

Additionally in the 1920s, in the reports of the proceedings of the High Court of Western Samoa alone, there are instances of some forty-three cases of surreptitious rape and sexual abduction and some ten cases of carnal knowledge and indecent assault.[19]

Surreptitious rape, or moetotolo in American Samoa in the 1920s. In the archives of the High Court of American Samoa I also discovered a detailed report on a case of moetotolo that occurred in the 1920s, which demonstrates conclusively the inaccuracy of Mead's account of this form of behavior.

The case, heard before the District Court at Fagatogo, Tutuila, American Samoa, 27 September, 1922, concerned the surreptitious rape of Selesa, of the Lesina District of northwestern Tutuila. At the time she was raped, she had held the *taupou* title of Fuiamaono for about one year. On the night of 31 August, 1922, Selesa, who was still a virgin, retired to sleep in her father's house with an old woman of the family to guard her. At about midnight Selesa awoke to find, to her distress, that a man named Teleti, who was holding her down, had with his fingers forcibly ruptured her hymen. When asked in court whether it was not possible to "scream or shove" Teleti off of her, Selesa replied "it was impossible because my mouth was blocked by him." Selesa then described how, knowing that she had been raped and was no longer a *taupou,* or ceremonial virgin, she "sat up and weeped." She also explained that in shame at her plight she agreed to *avaga,* or elope, with her assailant by going "with him to his family."

This verified evidence from the 1920s in American Samoa demonstrates yet again how erroneous is Mead's statement that moetotolo, or surreptitious rape in Samoa "involved no force, only deceit," with a man counting on "a girl's waiting for her lover" and slipping in ahead under cover of darkness, to take "advantage of her receptivity" (Mead 1963:20).[20] Rather, as the case of Selesa indicates, moetotolo involves the unlawful and forced penetration of a female's vagina entirely without her consent and is, therefore, in all such instances, a form of rape and an undoubted instance of male sexual aggressiveness. This revealing case of surreptitious rape in American Samoa in the 1920s taken together with the totals of rape and attempted rape in both American and Western Samoa and with the cognate evidence contained in my book, are certainly more than sufficient to refute conclusively Mead's unfactual assertion that "the idea of forceful rape is completely foreign to the Samoan mind," and to demolish McDowell's ineffectual attempt to defend the validity of Mead's defective ethnography.

On rape as a social practice. Dr. McDowell, with no firsthand experience of Samoa, has nonetheless had the effrontery to charge me, a student of Samoa for more than forty years, with having gone "to extremes" in reporting 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," and for describing rape, as it exists in Samoa, as a "recognized social practice."

Here, as elsewhere in her review, McDowell is, by sheer fiat, generating her own reality by denying the pertinence of well established facts. When the adherents of a belief system let themselves fall into this state they cease to be either scientists or scholars.

As verified facts in the proceedings of the High Courts of both American and Western Samoa demonstrate, both surreptitious and forcible rape are major elements in the sexual behavior of Samoan males. A culture, as Margaret Mead wrote in 1959, "shapes the lives of those who live in it," and this process involves the social transmission of information. In 1943 when, having had its *manaia* title conferred upon me, I became a member of the *aumaga* of Sa'anapu (which was then comprised of virtually all the untitled men in that village), I was systematically instructed in many things; prominent among them were the techniques used by Samoan males in both surreptitious and forcible rape. Again, I have, in all-male groups in Samoa, on several occasions witnessed the giving of instruction in these techniques by one male to another. It is therefore a fact that while regarded with abhorrence by women and older men, rape was, nonetheless, among some Samoan males a "recognized social practice." And this remains a fact, as does the presence of both surreptitious and forcible rape in Samoan society both today and in the 1920s and earlier, however much Dr. McDowell, in her ignorance of things Samoan, seeks to deny it.

On adultery in Samoa in the 1920s. Yet another behavior in respect of which Mead's ethnograpy of Samoa is markedly at error is adultery. For example, Mead (1969, orig. 1930:84) states of Manu'a in the mid-1920s: "A man who seduces his neighbour's wife will simply have to settle with his neighbour. The society is not interested." Statements like this led Bertrand Russell in his *Marriage and Morals,* after reading *Coming of Age in Samoa,* to state quite erroneously that Samoan husbands "when they have to go on a journey, fully expect their wives to console themselves for their absence" (1961, orig. 1928: 108).

As I show in my book (cf. 1983:241-43), in Samoa adultery is regarded as a most serious offence and one about which society at large is most definitely concerned. For Dr. McDowell's information, Mead's erroneous statements about adultery were among those I tested in my investigation in October 1981 of the archives of the High Court of American Samoa.

Prior to that time I knew that Section 23, Adultery, of the *Codification of the Regulations and Orders for the Government of American Samoa* (Noble and Evans 1921) that was in force during the time Mead was in Samoa, stated: "If any man and woman not being married to each other, shall lewdly and lasciviously associate, bed and cohabit together, they shall be fined not more than one hundred dollars, or imprisoned not more than twelve months, or both."

My examination of the archives of the High Court of American Samoa showed that, during the 1920s, Section 23 was regularly enforced. For example, in 1927, in the District Court, Fagatogo, Tutuila, it was charged than on 18 January of that year Peresetene did, in violation of Section 23, sleep with Ta'e, the wife of Patolo. For this offence Peresetene was fined $25 and Ta'e $15. Such cases fully confirm the testimony of my informants in Manu'a who stated that in the 1920s all those found guilty of adultery were heavily fined, with in some cases "the land of an offender being taken from him." Similar regulations concerning adultery also existed in Western Samoa, and reports in *The Samoa Times* record some forty-three convictions of Samoans for adultery during the 1920s in the High Court of Western Samoa.

It should now be obvious to Dr. McDowell that her ill-informed assertion--that it is "nonsense" for me to claim that my researches in the archives of the High Court of American Samoa provided me with conclusive evidence of anything--is quite wide of the mark. What these researches in fact provided me with, in respect of the behaviors of rape, adultery, and fornication, is verified evidence that Mead's depiction of Samoan sexual behavior in the *1920s,* in *Coming of Age in Samoa* and her other writings, is made up of a series of flagrant errors.[21]

The Duping Issue. It is the presence of *these* errors in Mead's writings that has, in my view, led many Samoans to give credence to the claim emanating from Manu'a that Mead must, in these matters, have been duped by her informants. Other Samoans, as has been reported by Shore (1982:213 n.2), insist with anger "that Mead lied" in her account of their sexual mores and behavior. In note 35 McDowell asserts of me: "he claims" that Mead's informants "tricked her." An accurate reading of my book will show that I make no such claim. In fact, after having dismissed the Samoan view reported by Shore, I discuss the report of another American cultural anthropologist, Elenor Gerber (1975:126); she was told by Samoans in American Samoa in the early 1970s that Mead's informants "must have been telling lies in order to tease her." Gerber's informants, I explain, were referring to the common Samoan pastime of *taufa'ase'e* in which someone, including on occasions a visiting European, is deliberately duped.

Let me now go on to say that since my book was published another American research scientist has recorded the same kind of information as did Gerber in 1975. Thus P.A. Cox, of the Department of Botany at the University of California, Berkeley, writing in the *American Scientist* (1983:407) states:

> Several years ago during an ethnobotanical survey in **Ta'ū**, I asked several older Samoans for their opinions on the Samoan

> studies of Margaret Mead. They told me she could not speak Samoan; this, coupled with, "teasing," *(taufa'ase'e)* on the part of her informants, had led her into serious error in her characterization of Samoan culture. They resented some of the implications of her studies and wished that the record could be set straight.

That Samoans hold these views cannot then be doubted, and this certainly deserves to be reported and discussed. What can be said is that the claim that Mead was duped into mistakenly believing that Samoa was a paradise of freelove is highly plausible to the Samoans themselves.

However, I state (1983:291) that while it may be likely that some of the adolescent girls on whom Mead relied for information resorted to taufa'ase'e (as has been suggested in the reports of Gerber, Cox, and others), "we cannot, in the absence of detailed corroborative evidence, be sure about the truth of this Samoan claim that Mead was mischieviously duped by her adolescent informants. Moreover, because this "detailed corroborative evidence" is lacking, I completely reject Felix Wendt's complaint that I ought to accept his view that Mead was "duped," and that "she must have purposely, deliberately, and knowingly given incorrect information on Samoa."

I would emphasize then that the claim that Mead was duped is *not* a claim that I myself make, nor does it have any bearing on my refutation of Mead's depiction of Samoa, which depends on quite other evidence.

On quotation and context. Having been unable to deal effectively with the substantive content of my refutation of Mead's depiction of Samoa, McDowell has belabored what she claims is my "habit of quoting others out of context." Thus, she has given great emphasis to my allegedly "blatant" practice of citing Mead out of context on competition in Samoan society. Let us then examine this particular accusation to see if, in any significant way, it invalidates my refutation of Mead's depiction of the uncompetitiveness of the Samoans.

The instance about which McDowell so expostulates occurs on page 88 in the chapter entitled "Mead's depiction of the Samoans." In this chapter, I attempt to provide those readers unfamiliar with Mead's writings with a general summary of her depiction of Samoa before essaying, as I do in chapters 8 to 18, a detailed refutation of Mead's actual statements.

As everyone who has read the volume entitled *Cooperation and Competition among Primitive Peoples* (Mead 1937) will know, Mead classified Samoa as a markedly uncompetitive society. Mead (1937:301) refers to "two tendencies in Samoan social organizations," the first of which is "the tendency to place each individual, each household, each village, even (in Western Samoa) each district in a hierarchy, wherein each is signified only by its relation to the whole, each performs tasks which contribute to the honor and well-being of the whole, and competition is completely impossible."

It is from this passage that I quote on page 88, and I do so because it is to this supposed tendency that Mead herself gives *markedly predominant emphasis* in her general characterization of Samoan society. For example, in 1931, in discussing the possibilities of "if not eliminating" jealousy, "at least of excluding it more and more from human life," Mead (1931:45) asserted, without mention of any countervailing tendency, that "Samoa has taken one road, by *eliminating* strong emotion, high stakes, emphasis on personality, *interest in competition* " (emphases added). This unqualified assertion by Mead that Samoan society has taken the road of "eliminating" interest in "competition," *fully justifies* my having mentioned, as I have on page 88, Mead's statement of 1937 that one of the chief characteristics of Samoan society is a form of organization that makes competition "completely impossible."

In chapter 10, entitled "Cooperation and Competition," I adduce evidence to show that there is, in fact, in Samoan social organization (in which competition is explicitly present at all levels) *no tendency,* as Mead erroneously claimed, either toward "eliminating" an "interest in competition," or toward making competition "completely impossible."

If, after having directed the reader's attention to this crucial issue in my general summary of Mead's depiction of Samoa, I had then in my detailed discussion of competition in chapter 10 failed to mention the countervailing tendency toward "rebellion of individuals" of which Mead made specific mention in 1937, I would indeed have been remiss, and McDowell would have had genuine cause for complaint. However, as readers of this rejoinder can establish for themselves by turning to page 142 of my book, I do there make *specific mention* of the other tendency noted by Mead in 1937. Rather than admitting this openly and honestly in the main text of her review, where it would have invalidated her insubstantial accusation, McDowell has relegated admission of this fact to an obscure note; in it she makes the further, and totally untrue accusation, that my full citation of Mead in chapter 10 was made "reluctantly"!

Here then, instead of concerning herself with substantive issues, McDowell is making unwarranted accusations in a futile attempt to deflect attention from the grave errors in Mead's account of competition in Samoa.[22]

As Holmes (in what McDowell has termed his "excellent ethnography") has noted of Samoa, "the whole pattern of oratory is based upon a competition in order to win prestige both for the orator himself and for the village or family he represents," and "competitive behavior and efforts to gain praise through excelling one's peers is believed to be one of the traditional aspects of Samoan culture" (1957:225-26).

That the young Margaret Mead, living in Ta'ū with an expatriate American family and relying for her information mainly on adolescent girls, should have failed to comprehend the centrality of competition in Samoan society is understandable however, for as Mead (1972:151) has noted (and as was fully confirmed, in statements to me by the chiefs of Ta'ū in 1967) she did not have, for the whole of the brief time she was in Manu'a, "any political participation in village life."

On the mistaken supposition that I claim that "Mead was all wrong." Having attempted to deflect attention from substantive issues by asserting quite falsely--as I have shown--that I do not give adequate mention to Mead's views of 1937 about competition, McDowell goes on to make the entirely false assertion that the "main point" of my book is that "Mead got her Samoan ethnographic facts all wrong." Nowhere in my book, or for that matter anywhere else, have I made such an absurd claim, for to many of the ethnographic facts reported by Mead there cannot possibly be any reasonable objection.

What I have done in my book is to present evidence showing that Mead's account of Samoa contains *sufficiently substantial and numerous mistakes and inaccuracies* to demonstrate conclusively that her extreme conclusion in respect of the etiology of adolescent behavior is *in error,* and cannot be sustained.

On male and female fieldworkers. In attempting to dismiss the significance for contemporary anthropology of my refutation of Mead and my advocacy of a more scientific anthropological paradigm, McDowell argues that my book has two main shortcomings. I shall deal with each of these in turn, beginning with McDowell's argument that because I am a man, and, as she would have it, have participated in "predominantly male events," this "had to influence" my "perspective on Samoa" and has prevented me from making a "significant contribution."

This woefully unfactual argument has been advanced by Dr. McDowell from a state of gross ignorance about the nature of my experiences in Samoa from the 1940s onwards. This ignorance has, however, in no way deterred her, for, as is the case with cultural anthropologists of her persuasion, the detailed investigation of the relevant facts is just not a consideration. A set of theoretical assumptions (as in the case of Mead in Samoa in the 1920s, and of Bradd Shore in the 1970s, as we shall presently see) tells them *in advance of any investigation* what an answer is going to be.

In Mead's case it is known from her own statements that her information was derived mainly from adolescent girls, and that, as Mead has specifically stated (1969:228) it was from the "vantage point" of the adolescent girl that she "saw" Samoan society. This was because of the problem Boas had given her to study, and if Mead had stayed on in Manu'a for more than just a few months, she certainly could have widened her perspective and learned more than she did of the preoccupations of men.

In particular, it is a complete non sequitur to suppose, as has McDowell, that because a fieldworker is a man he thus participates in "predominantly male events"; nor does it remotely follow that being a male cuts one off from contact with females.

In my own case, as a young man in Western Samoa in the early 1940s, having had a *manaia* title conferred on me (cf. 1983:235), I was afforded contact with many young Samoan women, some of whom, as I was able to speak their language fluently, became my very close friends. Indeed, it was through my firsthand experiences with some of these young women that I first became aware of the facts that demonstrate the errors of Mead's account of Samoan sexual behavior and values.

During my years in Samoa, from the early 1940s onward I have observed firsthand on numerous occasions the activities of the *aualuma,* and of all the other women's groups in Samoan society. And, because I have found them such intelligent and knowledgeable informants, much of my time has been spent in the company of middle-aged women like the forty-four year old daughter of a titular chief whom I mention on page 219. Again, during the years 1966-1967, I spent much time in detailed studies of the psychology of young girls, using techniques learned at the London Institute of Psychoanalysis from Dr. D.W. Winnicott and others.

Given these facts, which I can substantiate in detail if required, I can only dismiss as unfactual, ideological, and sexist the extraordinary argument to which McDowell has resorted, while deploring that loose thinking of this kind, which goes under the rubric of "the sociology of knowledge," has become quite common in cultural anthropology in recent years. In most instances, as in the present case, it is, rather, "the sociology of ignorance."

On complexity and Dr. B. Shore. McDowell's second argument is also the product of her great ignorance of things Samoan. I have failed, she claims, to comprehend "the complexity of human sociocultural behavior." This is a ludicrous claim for my whole refutation of Mead depends on my having documented numerous aspects of Samoan behavior that were ignored by Mead in creating her romantically fanciful picture of Samoa.

Yet, neglecting this fundamental point, McDowell at once goes on to argue that the "complexity" that eludes me has been grasped by an American cultural anthropologist, Dr. Bradd Shore, whose book, *Sala'ilua: A Samoan Mystery,* is, in McDowell's judgment, "superb": "readers who want to learn about Samoans should read Shore's book not Freeman's." According to McDowell, Shore has "subtly and deftly" resolved the "discrepancies" between "Mead's point of view and Freeman's."

Nothing, in fact, could be further from the truth, for Shore's book--which is, if anything, a more extreme exemplification of cultural determinism than was Mead's *Coming of Age in Samoa* --contains at its center an egregious error and quite fails to come to terms with the "mystery" it purports to explain.

According to Shore, most of what he finds "valuable in anthropology" he has derived from Professor David Schneider, and in the main analytical section of *Sala'ilua* we are in fact dealing with Schneider's notion (1980, orig. 1968:1) of "culture as a symbolic system purely in its own terms," and with what Shore calls "the power of cultural templates to guide action and shape experience."

This concept of culture as a "template" is another version of the notion, on which Benedict and Mead so relied, of culture as a "mold." Shore prefaces his analysis of what he calls the "fundamental Samoan categories of action" by a series of citations from Mead, on whose shoulders he has recently described himself as standing (Shore 1983), and by suggesting (1982b:153) that by her interpretation of Samoan culture Mead was committed to a "paradigm" essentially similar to that which he himself has adopted. In fact, Shore goes well beyond Mead in his avowal of "the power of cultural templates" by purporting to explain an impetuous and violent murder by one drunken chief of another in terms of "cultural structures."

Shore's view of culture, like Schneider's (1980:135), is emphatically dualistic; and central to his whole argument is an analysis of what, so he claims, is "really a kind of Samoan ideology distinguishing human nature and culture." Indeed, Shore asserts that the "nature/culture distinction, which Lévi-Strauss has made famous in anthropology as a basic intellectual problem underlying many social institutions is an important Samoan assumption."

According to Shore this assumption, which is evinced in "a fundamental cultural template . . . for ordering contexts," is expressed in two basically important "categories," the Samoan terms for which are *āmio* and *aga,* with *āmio* referring to nature, and *aga* to culture. It is in these terms that Shore's whole analysis proceeds.

This, no doubt, sounds entirely convincing to someone like Dr. McDowell who has no specific knowledge of Samoa. However, as I demonstrate in detail in a review article entitled "The Burthen of a Mystery" (soon to appear in the journal *Oceania*) Shore has the basic connotations of āmio and aga, in terms of which his whole anlaysis is couched, completely reversed. I do not know of another error of this magnitude in the entire ethnographic literature on Samoa, or indeed, in the ethnographic literature at large.

This means, ineluctably, that Shore's account of the "dual structure" on which he predicates his "distinctively anthropological solution" of the murder he is trying to explain is, by being based on erroneous information, fundamentally flawed. Further, Shore's account of this murder, as I show in my review article, is in various respects seriously defective.

We thus have in Shore's *Sala'ilua: A Samoan Mystery* a telling example of how a cultural anthropologist with an enthusiasm for a particular doctrine (as, for instance, a form of dualism "popular with anthropologists") joined with a belief in "the power of cultural templates," is apt to find exactly what he, or she, is hoping to find--as has happened before in the history of the beguiling islands of Samoa.

Sir Edmund Leach (1983:478), in his review of my book, found it a pity that I was "so solemnly committed to the revelation of the scientific truth," and suggested that I might have "made my points just as well by writing "a satire on the frailty of academic researchers." This is a possibility I did at one stage consider before deciding that such an approach would bring down on me too great a measure of ire. I must, however, confess that the spectacle of the worthy Dr. McDowell, in her impassioned defense of Mead, extolling as an object lesson to me this "superb" book that is both culturally deterministic to the hilt and flawed by a quite egregious error, is truly comic and a fit subject for the kind of satire that Edmund Leach had in mind.

On "the errors in Mead's interpretation" of Samoa. According to McDowell "the errors in Mead's interpretation have nothing to do with Boas or cultural determinism" but with an inability by Mead to incorporate theoretically "the notion of contradiction." This is both a breathtaking denial and a demonstrably false special pleading.

As I have documented earlier in this rejoinder, we know from Mead's own testimony, which she repeated several times,[23] that she went to Samoa in 1925 to investigate at the behest of Franz Boas "to what extent the storm and stress of adolescence" is "biologically determined and to what extent it is modified by the culture within which adolescents are reared." And we also know from Mead's own testimony (1977: 19) that she regarded Samoa as "a most felicitous choice" for the investigation of this particular problem, and that in chapter 13 of *Coming of Age in Samoa* she reached the extreme conclusion that biological variables are of no significance whatsoever in the etiology of adolescent behavior.

Boas accepted this conclusion without question, and in his *Anthropology and Modern Life,* published in 1928 a few months after Mead's *Coming of Age in Samoa,* stated without qualification that in Samoa "the adolescent crisis disappears" (p. 186). A few years later in *Patterns of Culture* (1945, orig. 1934:21) Ruth Benedict, Mead's other mentor at Columbia University, declared that among Samoan girls the adolescent period was "quite without turmoil."

As I have already remarked and as Raum long ago noted, these assertions are contradicted by Mead's own account, for at least four of her sample of twenty-five adolescent girls were delinquents. This means that in the mid 1920s, delinquency, with its attendant storm and stress, was present among Samoan adolescent girls at about as high a level as has been established for Samoa in later decades and for other twentieth century societies for which delinquency rates are available such as the United States and Australia.

In *Coming of Age in Samoa* (1961:157) Mead, as we have already seen, diverts attention from this reality by relegating her delinquent girls to a separate chapter and by arbitrarily excluding them from her theoretically all-important generalization that in Samoa adolescence represents "no period of crisis or stress." This, however, is a conspicuously unscientific maneuver, for the delinquent girls described by Mead are obviously *as much the products of the Samoan social environment* as are the other members of her sample.

It is, then, a matter for continuing astonishment, in view of the evidence provided by Mead (1961: chapter 11), that Benedict could have asserted without qualification that among Samoan girls the adolescent period was "quite without turmoil," and that virtually the entire anthropological establishment, following the lead of Boas and Benedict, came to give credence to this demonstrably erroneous conclusion. Its fatal appeal was that it so advantageously confirmed a pre-existing assumption.

As I point out (p. 86) Mead was obliged by the logic of her central argument "to depict the whole social life of Samoa as being free of happenings that might generate tension and conflict"--and it is from this situation that her erroneous depiction of Samoa really stemmed.

In some instances this involved (as can be demonstrated, once again, by internal evidence) a neglect of known facts that amounts to the suppression of crucially significant data, as the following examples show.

On affrays and the like. In her depiction of the ease and casualness of their society, Mead, as I have already noted, gave special emphasis to the "unaggressiveness" of the Samoans, describing them as "one of the most amiable, least contentious and most peaceful peoples in the world." As I have also indicated earlier in this rejoinder, an invaluable source of information on American Samoa in the mid 1920s is the manuscript journal of A. F. Judd, who, with an expedition from the Bernice P. Bishop Museum, did research in Manu'a early in 1926 at the same time as Mead was carrying out her own inquiries there. In the course of these researches Judd made a brief visit to the island of Ofu, which had long been in a state of emnity with the people of the island of Ta'ū, among whom Mead was then living.

In his journal, Judd (1926:78) describes a recent incident in which a new pastor, Iakopo, arriving in Ofu was "literally stoned" out of the village by the people, who resented the treatment to which their former pastor had been subjected.

Such an incident, involving a violent attack on a Christian pastor, is most serious for Samoans. It is also the clearest possible evidence of contentiousness and aggression. That Mead knew of this affray is obvious from her letter dated Ta'ū, 16 January 1926 in which she mentions (Mead 1977:47) as proof of the fact that she was "becoming a part of the community," that she had argued with members of the *aumaga* of Ta'ū about the advisability of "burning down" what was left of Ofu after the devasting hurricane of 1 January, 1926, "because the people of Ofu stoned the meddlesome pastor of Ta'ū" out of their village. Further, Mead was acquainted with the pastor who had been stoned, for she mentions him as the Samoan pastor Iakopo in the acknowledgements in *Coming of Age in Samoa.* Finally, in March 1926, Mead visited Ofu and so was in a fully favorable position to investigate and report in detail on the affray of which she had prior knowledge.

That this was not done is characteristic of Mead's whole approach to the problem she had been given by Boas, which was to make of Samoa, as she has admitted, a "negative instance." Yet as must be obvious to even the most doctrinaire cultural anthropologist, if Mead had fully reported the affray that, from Judd's evidence, we know took place in Ofu while she was in Manu'a, together with the history of the severe conflict between the people of Ta'ū and those of Olosega and Ofu (cf. Freeman 1983:169), she could not possibly have made the erroneous statements she did about the "unaggressiveness" of the Samoans, and her so-called "negative instance" would have been revealed as no negative instance at all.

On suffering for one's convictions. Another of the ingredients of Meads depiction of Samoa as a "negative instance" was her claim (1961:198) that Samoa was a place where "no one suffers for his convictions." This claim, as I demonstrate (1983:270ff.), is directly contradicted by the facts of Samoan history, including the history of the 1920s. Thus not long before the period of Mead's fieldwork, when a number of chiefs of Ta'ū defied the naval government by conferring the august title of Tui Manu'a on Christopher Taliutafa Young, they told Governor Kellogg--after he forcibly quashed what they had done--that they were "dissatisfied to the death" with his interference in their affairs. The acting district governor of Manu'a at this time was Sotoa, a high chief of Lumā on the island of Ta'ū, and Governor Kellogg, holding him (Gray 1960:208) to be "primarily at fault," suspended Sotoa from office. This action by Governor Kellogg, which Sotoa considered to be unjust and which he deeply resented, was borne by him with dignity. Some six years later when the American Samoa Congressional Commission of 1929-1930 visited Manu'a (1931:217), Sotoa, when giving evidence on 2 October 1930, reiterated that it had been "unanimously agreed" by himself and the other chiefs of Ta'ū to confer the Tui Manu'a title on Christopher Taliutafa Young and roundly criticized, Governor Kellogg for his action in interfering in the affairs of Manu'a and banishing Christopher Taliutafa Young to the island of Tutuila.

Here then we have a clear instance of a major political confrontation, which was still in progress at the time of Mead's researches, and in which the high chief Sotoa most certainly suffered for his convictions, as did Christopher Taliutafa Young. Furthermore, there is certain evidence that Mead was aware of what had befallen Sotoa, whom she knew well; in *Social Organization of Manu'a* (1969:167) she refers to a dream reported to her by Sotoa, that he had had "before the political trouble resulting from the attempt to reinstate the Tui Manu'a." Once again, if Mead had investigated and reported the nature of this "political trouble," she could never have included as one of the ingredients of her depiction of Samoa as a "negative instance" the quite erroneous generalization that Samoa is a place where "no one suffers for his convictions."[24]

Thus, however much McDowell might wish to deny it, there exists the clearest evidence that the errors of Meads depiction of Samoa are indeed associated both with the problem she had been set by Boas and with the doctrine of cultural determinism of which she, like Boas, was a principal proponent.

Concluding remarks. As we have now seen McDowell has failed to make substantive points about Samoa that, in any significant way, weaken the cogency of my refutation of Mead. Rather, I have presented decisive new evidence to strengthen this refutation.

Again, even McDowell's most emphatic allegations of misquotation (as in the cases of Boas on genetics and Mead on competition) turn out, when factually analyzed, to have been misconstrued or misrepresented by McDowell herself. [25]

What then of her more general comments occurring at both the outset and conclusion of her review?

According to McDowell, my book "has almost no general or constructive relevance to contemporary anthropology." This is woefully to misunderstand the significance of refutation in the progress of a science. As Sir Karl Popper has shown, a science progresses by the elimination of error from its formulations, so that, as Charles Darwin remarked in 1879, "to kill an error is as good a service as, and sometimes even better than, the establishing of a new truth or fact" (1903:II,422).

The extreme conclusion reached by Mead in 1928, to which McDowell and many other cultural anthropologists have long given uncritical credence, is, as I have noted, scientifically preposterous, and the *formal* refutation of this conclusion is, therefore, of fundamental anthropological importance.

McDowell is entirely in error, furthermore, in asserting that my "entire argument" rests on the assumption that "in refuting Mead's negative instance" I have demolished "forever the idea that adolescence is not necessarily a period of stress." As McDowell for some reason fails to record, Mead's "negative instance" was in fact, as I have documented, used to support the more specific and extreme conclusion that biological variables are of no significance in the etiology of adolescent behavior. All that my refutation of Mead's erroneous depiction of Samoa does is to demonstrate that the case of Samoa can no longer be advanced, as it has for so long by cultural anthropologists, to justify the doctrine of cultural determinism.

There is, however, as McDowell has failed to mention, *no* logical connection between my refutation of Mead's classing of Samoa as a negative instance, and my advocacy, on general scientific grounds, of an interactionist paradigm for anthropology. As McDowell correctly notes it is certainly open to any cultural anthropologist to advance some other "negative instance" in proof of the assertion that biological variables are of no significance in the etiology of adolescent behavior and in support of the extreme doctrine of cultural determinism in which Mead and others believed in the 1930s. [26]

This, however, is a most unlikely happening for today, as Stephen Jay Gould has recently remarked, "Every scientist, indeed every intelligent person knows that human social behavior is a complex and indivisible mix of biological and social influences" (1983:6). And if this be true--and all the relevant evidence indicates it is--then the day of the "negative instance," which has proved in the case of Samoa to be entirely nugatory, as well as the day of the cultural determinism that Mead and others once championed, is indeed over.

In this situation, given the present state of scientific knowledge, anthropology has really no rational alternative but to move toward a fully interactionist paradigm of the kind adumbrated in the final chapter of my book.

So decisive has been the advance in knowledge during recent decades that, as Ashley Montagu indicated in 1979, there is no longer any rational justification for belief in "the *tabula rasa* myth." We are indeed evolved primates, and the time has come when it is incumbent on all behavioral scientists--including cultural anthropologists--to acquaint themselves with and to take fully into account all of the phylogentically given elements in our behavior of the kind that is summarized in, say, Richard Passingham's *The Human Primate* (1982).

When this is done, within an interactionist paradigm, it then becomes possible to analyze and explain cultures in a much more scientific way than is open to doctrinaire cultural anthropologists. For when it is realized that cultures are the products of human choice, it becomes possible to relate particular cultural choices to the evolved primate nature of those who have enacted them, and in this way quite new light is cast on other phenomena of cultural differences.

In his classic book of 1937 Dobzhansky declared: "It is a demonstrable fact that human biology and human culture are parts of a single system, unique and unprecedented in the history of life" (1937:304). Since that time the truth of this declaration has become ever more apparent, and it is, in the words of Peter Corning (1983:151), "increasingly evident that the life sciences and the social sciences must converge on an Interactional Paradigm."

It is to the realization of this most important of objectives for all the human sciences that my book is a contribution, and I have no doubt at all, knowing what I do of the progression of science, that the convergence of which Corning writes will indeed eventually occur.

Derek Freeman
Australian National University

NOTES

1. In her book *My Samoan Chief* (1975:18) Fay Ala'ilima records that her Samoan husband, who was born in Western Samoa and lived from the age of twelve onward in American Samoa, completely disagrees that Samoan adolescence is not "a period of 'sturm and drang'."

2. In the preface to my book I also note that in August 1978 I offered to send Dr. Mead an early draft of my refutation of the conclusions she had reached in *Coming of Age in Samoa,* but that "I received no reply to this offer before Dr. Mead's death in November of that year." McDowell has chosen to see in this an "innuendo." She is quite mistaken. Let me for Dr. McDowell's information describe the circumstances in further detail. In August 1978, as soon as I had completed a first draft of a chapter of my book (i.e. the present chapter 16), I wrote to Dr. Mead asking if she would like to see this draft. In reply I received a letter dated New York, 14 September 1978, in which I was informed by an assistant, Amy Bard, that Dr. Mead had "been ill," and that if Dr. Mead had "an opportunity to read and comment on my manuscript" I would be notified. I heard no more from Dr. Mead's office before her death in November 1978. Early in 1979 in a letter to Amy Bard I wrote of Dr. Mead's "unfortunate death," that I considered it "a true loss" to have her "wise and challenging voice forever stilled." I had not then, nor have I now, any knowledge of the course of Dr. Mead's final illness.

3. As Barash (1982:160) has noted: "When we describe and seek to understand the natural world, we are not seeking to condone it." Again, as Barash also points out (p. 161), an understanding of "the biological factors that influence our behavior" may "even provide us with greater 'free will', by making us more aware of our own hidden tendencies, so that we may seek to resist them, if we wish."

4. McDowell asserts it to be "odd" that I should, in my book, have concentrated "almost exclusively on American anthropology." This is by no means odd, as I am concerned with the work of American anthropologists. However, as I note on page 313: "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."

5. Jenning's classic book of 1930, *The Biological Basis of Human Nature,* had the utmost relevance to the issues discussed by Boas both in the 1938 edition of his *The Mind of Primitive Man* and in the section entitled "Biological Premises" in his *General Anthropology,* also of 1938. It is mentioned in neither place.

6. McDowell, quoting from page 32 of my book, claims that I have drawn an unwarranted conclusion from a passage I cite from Stocking (1968:264). This is by no means the case. Stocking correctly refers to the whole thrust of Boas' thought as being to "separate biological and cultural heredity." This means that McDowell is wrong in asserting Stocking says "nothing" about Boas "denying biology." I must insist that he does, for in separating biological and cultural heredity, Boas (as anyone who has studied his thought will know) was denying the relevance of biological variables in large areas of human behavior where, in fact, they undoubtedly do operate.

7. Boas' lack of exact knowledge of genetics is apparent in his use of the terms genotype and phenotype, as in his paper of 1925 in *The Nation,* entitled "What is a Race?" In this pa-

per Boas follows the erroneous definitions of these terms that appeared in the 1914 edition of Funk and Wagnall's *New Standard Dictionary of the English Language.* They were subsequently corrected by G. II. Shull (1915:56ff.) in the *American Naturalist.*

8. There is a similar dismissive discussion of natural selection by Boas (1963, orig. 1911:97 in chapter five of his *The Mind of Primitive Man.*

9. Mead says "remembering Stevenson's rhapsodies" on p. 147 of *Blackberry Winter.*

10. I thank Dr. McDowell for having pointed out the following errata in my book: p. 24 (line 12 from bottom of page), *for* path of truth *read* path to truth; p. 89 (line 10 from bottom of page), *for* aggression *read* aggressiveness; p. 93 (line 2 from bottom of page), there should be a dash (--) between "more" and "primitive." These are all transcription errors, none of which, fortunately, alters in any significant way the sense of the excerpts being quoted.

I would also draw attention to these other errata, all of which will be corrected in future editions of my book: p. xiv (line 15 from top of page), *for* forgathered *read* foregathered; p. 121 (line 8 from bottom of page), *for* of constitution *read* or constitution; p. 176 (line 10 from bottom of page), *for* comonly *read* commonly; p. 227 (line 2 from top of page), *for* permarital *read* premarital; p. 238 (line 2 from bottom of page), *for* obseve *read* observe; p. 246 (line 9 from bottom of page), *for* bisucits *read* biscuits; p. 249 (line five from bottom of page), *for* 1938 *read* 1928. On pp. 219, 346, and 374, *for* Fenichal *read* Fenichel.

11. I would add that Richard Goodman *in an entirely independent inquiry* reaches conclusions about Mead's erroneous depiction of Samoan behavior that are virtually identical with my own.

12. In his Ph.D dissertation, "A Restudy of Manu'an Culture," Holmes (1957:15) did not hesitate to use accounts of 'Western Samoa in the mid-nineteenth century as "an early base line" for the analysis of "Manu'an culture."

13. I do not criticize Mead, as McDowell claims, "for not writing a book on Samoan his tory." I would, however, criticize her for making assertions about Samoan history (as, for example, about warfare) without ever having consulted the relevant historical manuscripts.

14. In note 33, McDowell refers to the statement of Gerber's Samoan informants in the early 1970s that "things used to be stricter than they are today." McDowell is mistaken in asserting that I just accept this statement "at its face value." I well know, from intensive firsthand experience, that things were considerably stricter in the early 1940s than in the mid-1960s, and all the relevant historical evidence confirms this.

15. I might mention in this context that when, on 17 September 1967 I interviewed Fa'alaulā, of Ta'ū, Manu'a, who was then seventy-seven years of age and who had been a close associate of Mead in 1925-1926, she claimed that she told Mead of *moetotolo,* or surreptious rape: *"E sā, sā lava ona faia se mea fa'apēnā"* (It is forbidden, most forbidden, to do a thing like that).

16. McDowell's complaint is unjustified that in referring (as on p. 234 of my book) to "the cult of virginity" in Samoa, I do not address "the issue of the nature and definition of 'cult' " and that my usage of this term is "clearly at odds with accepted anthropological practice." I am using the term "cult" in one of its accepted dictionary meanings (cf. *Random House Dictionary)* to refer to "an instance of great veneration of a person, ideal of thing; esp. as manifested by a body of admirers."

17. McDowell's claim that I have a "flagrant disregard" for my source Katchadourian (1977) is a complete non sequitur for I do not suppose, nor do I anywhere argue, that adolescent stress is either inevitable or universal.

18. The fact that McDowell's note 38 is based on a garbled newspaper report, containing totally erroneous information, makes this note entirely misconceived and irrelevant.

19. These cases are taken from reports of the proceedings of the High Court of Western Samoa that appeared during the 1920s in *The Samoa Times.* These reports are by no means complete. Further, there is a marked diminution in the cases of sexual abduction and carnal knowledge reported from 1927 onward during the political disaffection that was then rife in Western Samoa. For example, while twenty-three cases of sexual abduction were reported as having been before the High Court during the years 1924-1926, only nine such cases were so reported during the years 1927-1929. In Samoa sexual abduction, as explained on page 246 of my book, follows a *moetotolo,* or surreptitious rape, and is so referred to in court actions.

20. Successful personation by a rapist is indeed an extremely rare event. The only case I have come across in my researches on rape is reported on pages 148-149 of *Crime in New Zealand* (Department of Justice, Wellington, New Zealand, 1968).

21. McDowell, in note 37, asserts that I go to "ridiculous lengths" in stating that Mead returned from the field "with tales running directly counter to all other ethnographic accounts of Samoa," yet she gives no examples to falsify my statement. Let me then repeat that, to the best of my knowledge, the tales that Mead brought back to New York in 1926 about aggression, warfare, competition, premarital sexuality, adultery, rape, and not a few other aspects of Samoan existence, indeed did run counter to all other then existing ethnographic accounts of Samoa.

22. I also reject McDowell's argument that because Mead's comment--that a Samoan who "feels strongly" is maladjusted--was taken from Mead's *Sex and Temperament in Thee Primitive Societies* (1935) it is very much out of context. As I know from my conversation with Mead in 1964 and from other sources, her views about Samoa did not change over the years, for after June 1926 she did no further substantial research on Samoa. Thus her reference of 1935 is a repetition of the view she had expressed in *Coming of Age in Samoa* in 1928.

Again, the summary I give (pp. 93-94) of Mead's depiction of adolescence in Samoa, to which McDowell has objected, is, in my judgment, both reasonable and fair. The crucial issue here is Mead's assertion that in Samoa adolescence is "the age of maximum ease." Further, the fact to which McDowell directs attention, namely that Mead justifies her assertion by claiming that an adolescent is near some supposed "center of pressure," is not worthy of mention because it is an unwarranted and false supposition.

23. For example, in *Coming of Age in Samoa* (1961, orig. 1928:11), Mead described the "question" that "sent" her to Samoa as: "Are the disturbances which vex our adolescents due to the nature of adolescence itself or to the civilization?" In her paper "Cultural Contexts of Puberty and Adolescence" (1959:60) Mead wrote: "The problem which he [Boas] sent me to Samoa to study concerned the extent to which the well-known vicissitudes of adolescents in our society were dependent upon the physical changes through which they were passing or upon other nature of the culture in which they grew up." And, in her paper "Retrospects and Prospects" (1962:122) she records that Boas persuaded her to "undertake the study of the relative strength of biological puberty and cultural pattern."

24. Again, if Mead had inquired at the High Court when she was in Pago Pago, as she was during both September and October 1925 and again in May and June 1926, she could readily have established that rape did indeed occur among Samoans and might even have discovered the case of Selesa (which I have described) and thus corrected her inaccurate account of *moetotolo.* While Mead spells the name Sotoa correctly in the acknowledgements of *Coming of Age in Samoa,* it is spelled incorrectly as Soatoa on page 167 of *Social Organization of Manu'a* (1969, orig. 1930).

25. I apologize to the readers of *Pacific Studies* for the great length of this rejoinder and can only plead that this has been necessitated by the many points raised by Dr. McDowell. Even so, I have dealt only with the issues to which Dr. McDowell has given special emphasis and have considered not a few of her points to be altogether too trivial to warrant serious discussion. Although this rejoinder has been written under great pressure against a deadline. I have striven for accuracy at all times. I would also add that in writing it I have been conscious of the fact that since 1981 I have held the positions of Foundation Professor of Anthropology and Consultant on Samoan Studies at the University of Samoa.

26. McDowell cites a letter from the *New York Times* of 6 February 1983, in which Murphy, Alland, and Skinner state concerning Mead's conclusion about adolescence in Samoa: "whatever may be the Samoan facts, subsequent research in other parts of the world has sustained her essential theoretical stance." This is untrue. No other cultural anthropologist. to the best of my knowledge, has reached the same extreme conclusion about adolescent behavior that was reached by Mead in 1928.

REFERENCES

Allen, G. E.

1972 Introduction to the Reprint Edition of *The Mechanism of Mendelian Heredity* (orig. 1915) by T. H. Morgan, A. H. Sturtevant, H. J. Muller, and C. B. Bridges. New York.

1979 (orig. 1975) *Life Science in the Twentieth Century.* Cambridge, England.

American Samoa: Hearings before the Commission Appointed by the President of the United States. 1931. U.S. Government Printing Office. Washington, D.C.

Amir, M.

1971 *Patterns in Forcible Rape.* Chicago and London.

Barash, D. P.

1982 *Sociobiology and Behavior.* London.

Barnouw, V.

1979 *Culture and Personality,* 3rd ed. Homewood, Ill.

Benedict, R.

1945 (orig. 1934) *Patterns of Culture.* London.

Boas, F.

1916 "Eugenics." *The Scientific Monthly* 3:471-78.

1925 "What is a Race?" *The Nation* 120:89-91.
1928 *Anthropology and Modern Life.* New York.
1963 (orig. 1911)
The Mind of Primitive Man. New York.
1939 "Genetics and Environmental Factors in Anthropology." *The Teaching Biologist* 9:17-20, and 45.
1940 (orig. 1935)
"The Tempo of Growth of Fraternities." In *Race, Language and Culture.* New York.
1940 (orig. 1932)
"The Aims of Anthropological Research." In *Race, Language and Culture.* New York.

Boas, F. ed.
1938 *General Anthropology.* New York.

Bronowski, J.
1965 (orig. 1956)
Science and Human Values. New York.

Calkins. F.
1975 *My Samoan Chief.* New York.

Campbell, D. T.
1961 The Mutual Methodological Relevance of Anthropology and Psychology. In F. L. K. Hsu, ed. *Psychological Anthropology.* Homewood, Ill.

Cook, O. F.
1907 "Aspects of Kinetic Evolution." *Proceedings of the Washington Academy of Sciences* 8:197-403.

Corning, P.
1983 *The Synergism Hypothesis: A Theory of Progressive Evolution.* New York.

Cox, P. A.
1983 "Margaret Mead and Samoa." *American Scientist* 71:407.

Darwin, C.
1903 *More Letters of Charles Darwin.* F. Darwin and A. C. Seward, eds. 2 vols. London.

Department of Justice, New Zealand
1968 *Crime in New Zealand.* Wellington, N.Z.

Dobzhansky, T.
1957 (orig. 1937)
Genetics and the Origial of Species. New York.

Duff, R. Interview in the Christchurch *Press,* 23 November 1937.

Dunn, L. C.
1927 "The Theory of the Gene--a Review." *Journal of Heredity* 18:22-24.

Festinger, L., H. W. Riecken, and S. Schachter
1964 (orig. 1956)
When Prophecy Fails. New York.

Fisher, R. A.
1930 *The Genetical Theory of Natural Selection.* Oxford.

Freeman, D.
1972 "*Social Organization of Manu'a* (1930 and 1969) by Margaret Mead: Some Errata." *Journal of the Polynesian Society.* 81:70-78.
1983 *Margaret Mead and Samoa: The Making and Unmaking of an Anthropological Myth.* Cambridge, Mass.
forthcoming "The Burthen of a Mystery." *Oceania.*

Gerber, E. R.
1975 "The Cultural Patterning of Emotions in Samoa." Ph.D diss. University of California, San Diego.

Gladwin, T.
1961 "Oceania." In *Psychological Anthropology.* F. L. K. Hsu, ed. Homewood, Ill.

Goodman, R. A.
1983 *Mead's Coming of Age in Samoa: A Dissenting View.* Oakland, California.

Gould, S. J.
1983 "Genes on the Brain." *The New York Review of Books* 30 (11):5-9

Gray, J. A. C.
1966 *Amerika Samoa.* Annapolis.

Herskovits, M. J.
1943 "Franz Boas as a Physical Anthropologist." American Anthropologist, N.S. Memoir 61:39-51.

Holmes, L. D.
1957 "A Restudy of Manu'an Culture: A Problem of Methodology." Ph.D. diss. Northwestern University.

Huxley, J.
1949 *Heredity East and West.* New York.

Jennings, H. S.
1927 "Review of *The Theory of the Gene* by T. H. Morgan." *The Nation* 124:184.
1930 *The Biological Basis of Human Nature.* New York.

Judd, A. F.
1926 Expanded notes, ethnology, etc. American Samoa, February 15-April 2, Islands of Tutuila, Ofu and Ta'ū. MSS. Bernice P. Bishop Museum Library, Honolulu.

Katchadourian, H.
1977 *The Biology of Adolescence.* San Francisco.

Kluckhohn, G., and O. Prufer
1959 "Influences during the Formative Period." In *The Anthropology of Franz Boas.* W. Goldschmidt, cd. *American Anthropologist* N.S. Memoir 89:4-28.

Konner, M.
1928 *The Tangled Wing: Biological Restraints on the Human Spirit.* New York.

Kroeber, A. L.

1916 "Inheritance by Magic." *American Anthropologist* 18:19-40.

1943 "Franz Boas: The Man." *American Anthropologist* N.S. Memoir 61:5-26.

1956 "The Place of Boas in Anthropology." *American Anthropologist* 58:151-59.

Lawick-Goodall, J. van

1971 *In the Shadow of Man.* London.

Leach, E.

1983 "The Shangri-la that Never Was." *New Society* (24 March 1983): 477-78.

Lowe, R. B.

1967 *Problems in Paradise.* New York.

Macdonald, J. M.

1975 *Rape: Offenders and Their Victims.* Springfield, Ill.

McDowell, N.

1980 "The Oceanic Ethnography of Margaret Mead." *American Anthropologist* 82:278-302.

Marsack, C.

1964 *Samoan Medley.* London.

Mayr, E.

1982 *The Growth of Biological Knowledge.* Cambridge, Mass.

Mead, M.

1928 "The Role of the Individual in Samoan Culture." *Journal of the Royal Anthropological Institute* 58:481-95.

1937 "The Samoans." In M. Mead, ed. *Cooperation and Competition among Primitive Peoples.* New York.

1959 "Cultural Contexts of Puberty and Adolescence." *Bulletin of the Philadelphia Association for Psychoanalysis.* 9:59-79.

1961 (orig. 1928)
Coming of Age in Samoa. New York.

1962 (orig. 1950)
Male and Female. Harmondsworth.

1962 "Retrospects and Prospects." In *Anthropology and Human Behavior,* T. Gladwin and W. C. Sturtevant, eds. Washington, D.C.

1969 (orig. 1930)
Social Organization of Manu'a. Honolulu.

1972 *Blackberry Winter.* New York.

1977 *Letters from the Field 1925-1975.* New York.

Mead, M. ed.

1937 *Cooperation and Competition among Primitive Peoples.* New York and London.

1962 *An Anthropologist at Work.* New York.

Morgan, T. H.
1919 *The Physical Bask of Heredity.* Philadelphia and London.
1929 "Lamarckism." *Encyclopaedia Britannica,* 14th. ed. 13:607-10.
1964 (orig. 1926)
The Theory of the Gene. New York.

Morgan, T. H., A. H. Sturtevant, H. J. Muller, and C. B. Bridges
1972 (orig. 1915)
The Mechanism of Mendelian Heredity. New York and London.

Noble, A. M., and W. Evans
1921 *Codification of the Regulations and Orders for the Government of American Samoa.* San Francisco.

Passingham, R.
1982 *The Human Primate.* Oxford and San Francisco.

Radin, P.
1939 "The Mind of Primitive Man." *The New Republic* 98:300-303.

Raum, O. F.
1967 (orig. 1940)
Chaga Childhood. London.

Russell, B.
1958 (orig. 1928)
Marriage and Morals. London.

Schneider, D. M.
1980 (orig. 1968)
American Kinship: A Cultural Account, 2nd ed. Chicago.

Schoeffel, P.
1979 "Daughters of Sina: A Study of Gender, Status and Power." Ph.D diss. Australian National University.

Shore, B.
1982a "Sexuality and Gender in Samoa: Conceptions and Missed Conceptions." In S. Ortner and H. Whitehead, eds. *Sexual Meanings.* Cambridge, England.
1982b *Sala'ilua: A Samoan Mystery.* New York.
1983 *In Barnard Bulletin,* 92 (9), April 13.

Shull, G. H.
1915 "Genetic Definitions in the New Standard Dictionary." *American Naturalist* 49:52-59.

Stocking, G. W., Jr.
1968 *Race, Culture and Evolution.* New York.

Vidich, A. J.
1966 "Introduction." In *The Method and Theory of Ethnology* by Paul Radin. New York and London.

Wendt, A.
1983 "Review of *Margaret Mead and Samoa* by D. Freeman." *Pacific Islands monthly* 54:10-12, 14, and 69.

Williams, J. J.
1936 "Boas and American Ethnologists." *Thought* 11:194-209.

Winston, E.
1934 "The Alleged Lack of Mental Diseases among Primitive Groups." *American Anthropologist* 36:234-38.