

The Attack on Mead

Margaret Mead and Samoa. The Making and Unmaking of an Anthropological Myth. DEREK FREEMAN. Harvard University Press, Cambridge, Mass., 1983. xviii, 380 pp., illus., + plates. \$20.

Subtitled "The Making and Unmaking of an Anthropological Myth"; described by Harvard University Press on its jacket blurb as correcting "a towering scientific error"; praised on the dust jacket and in prepublication publicity by an ethnologist, a biologist, a psychoanalytic historian, a physical anthropologist, and a science writer (all men of stature) as masterly, a potential turning point, promising a major impact, extraordinary, salutary, and so forth; heavily advertised ("Read what the experts are saying about the book that's stirring controversy in *Time*, *Newsweek*, and on radio and TV")—this book has been, prior to publication, a media event in the United States and Great Britain. Ordinarily all this would be extraneous to a review of a book, but in this case it is not. For those who believe, as I do, that this response is out of all proportion to the quality of the book, the response seems to be based on and to exploit its rhetoric, its tone, and its own mythopoeic intent, and the book and the response are inseparable.

In 1925–1926, when she was 23 years old, Margaret Mead spent nine months doing her apprentice anthropological fieldwork, mostly on the island of Ta'u in the Manu'a Island group in the eastern portion ("American Samoa") of the Samoan archipelago. She reported the results of her study in two books (one, *Coming of Age in Samoa*, for a general readership and the other, *Social Organization of Manu'a*, for a professional one) as well as in a number of papers and articles (1).

For the general public the didactic, committed, and ideological writings of Margaret Mead contributed to the support of liberal values from the 1920's until her death in 1978. According to Theodore Schwartz (2) "Anthropology's claim to having transformed human cultural consciousness rests to an extraordinary extent on her work. While most anthropologists have spoken to and read one another, she has spoken to and been read by innumerable students and a vast public." Within the profession the re-

sponse to her work was a sort of ambiguous admiration for "one of anthropology's most creative and brilliant personalities," in the characterization of one of her severest methodological opponents, Marvin Harris (3). She was a model of commitment to fieldwork and to the people of the anthropological laboratory, a model for the essential humanistic component of anthropology. But whatever her importance to a many-headed, vigorously developing present-day anthropology may prove to have been in some future retrospect, neither her theory nor her method nor her data are at the center of current discussion—which is not to belittle her enormous contribution to the organization, vitality, and morale of the profession during her lifetime.

As for her Samoan work, professionals working in Samoa soon recognized that the portrait she drew was problematic. As Bradd Shore notes in what is the most sophisticated and important recent anthropological study of Samoa (4), while some observers of Samoans have emphasized polite passivity, as Mead did, or aggressiveness, as did the social psychologist Edwin Lemert (5), others have noted "the strongly contradictory tendencies of Samoan personality," including, in the words of F. and M. Keesing (6) as quoted by Shore, "security," "conformity," and "group responsibility" and the symmetrical balancing of social structures on the one hand . . . [and] 'divisiveness,' 'deviousness,' 'turbulence,' and the potential of 'violence' on the other." This is only paradoxical, as Shore demonstrates, if one believes that what seem at some moments and in some contexts to be prevalent forms of behavior characterizing a community are, or should be, without structure. Mead assumed something like this in her search for an essential style, the "ethos," characterizing and pervading Samoan culture, and it made her vulnerable to the overgeneralizing of a partial vision and, in turn, to this present latter-day polemic.

Derek Freeman, an Australian anthropologist, first went to Western Samoa, then a territory of New Zealand's, in 1940 and, finding it to be different from Ta'u in American Samoa as described by Mead, decided he would have to engage in "systematic testing of Mead's depiction of Samoan culture." He presents

the results in this book "based on investigations . . . over some forty years, including six years spent in Samoa and even longer in the research libraries of Australia, New Zealand, England, and the United States." He concludes that "many of the assertions appearing in Mead's depiction of Samoa are fundamentally in error, and some of them preposterously false."

Mead had written in *Coming of Age in Samoa*, generalizing, as she later regretted having done, from the inhabitants of Ta'u to "Samoans," that for them growing up is easy; the society as a whole is casual; no one suffers for his or her convictions, or fights to the death for special ends; love and hate, jealousy and revenge, sorrow and bereavement, are all "matters of weeks"; and people learn not to care too deeply about any one relationship. During these 40 years Freeman has searched for counter-evidence. He documents violence, tension, constriction. Samoa, as everyone who has worked at length there has written, is not the Arcadia depicted by Mead. (In fact, however, Ta'u and Manu'a in general seem to have undergone a much less stressful political, religious, and economic modernization than Western Samoa, where Freeman worked, and, as the later studies of Lowell Holmes [7] in Ta'u suggest, may have been, at least in the early decades of the century, less turbulent than other parts of Samoa.) But if not Arcadia, Mead perceived accurately something about Samoa (and the rest of Polynesia) that Freeman, in his attempted demythologizing, will not allow her.

More of this later, but first why did he bother? It is, at least according to his public writing and statements, because he had come to think that Mead is representative of, and in part responsible for, something portentous, the "Boasian paradigm" (referring to Franz Boas, Mead's teacher at Columbia and a founder of American cultural anthropology), which involves "the explanation of human behavior in purely cultural terms" and which "allowed the independence of cultural anthropology, at an enduringly crippling intellectual cost." The cost, according to Freeman, was the exclusion of "nature" from any kind of consideration whatsoever." Those who, reading Freeman, accept this portrayal can only be grateful to him for clearing such an obviously error-ridden ground. But the portrayal is muddled, distorted, and tendentious.

In the first part of the book Freeman reviews the intellectual climate in which the anthropology of Boas took form. He

portrays it, as it was in part, as a reaction against the powerful, simplistic, racial, racist, and eugenic interpretations and programs of the day, urged by partisans of "nature" in the nature-nurture debate. This was certainly an important part of the position of Boas and his students. But there were, in addition, many ideas in the air (and, even, in the work of Durkheim and other sociologists, on the ground) deriving from the powerful critiques of Enlightenment thought on the nature of man, society, history, progress, epistemology, and scientific method, collectively characterized by Isaiah Berlin as the "Counter-Enlightenment" (8). These critiques entailed nascent conceptions of autonomous social and historical processes. As the historian of anthropology George Stocking has written in the central work on the period with which Freeman is concerned (9), the idea of culture, as formulated and introduced by Boas, which became "a crucial part of the modern social scientific 'paradigm' for the study of mankind, . . . involved the rejection of simplistic models of biological or racial determinism, the rejection of ethnocentric standards of cultural evaluation, and a new appreciation of the role of unconscious social processes in the determination of human behavior. It implied a conception of man not as a *rational* so much as a *rationalizing* being."

But Freeman, by taking and exaggerating one element in this conception, is able to provide a plot. To wit, the extreme claims of the nature school about how to explain human behavior (particularly its variation in different kinds of communities) were countered by the extreme claims for culture and nurture of the Boasians; Mead's claims about the lack of what was taken to be a universal bit of nature (the "adolescent crisis") in Samoa (a lack she presented as an element in a generalized Arcadian Samoan ethos) were taken as the first and crucial empirical test of the overwhelming importance of culture; somehow under the influence of her ideological convictions (and because she was lied to by her Samoan informants, for whom she was a willing dupe) she wildly distorted the Samoan reality; therefore, the case for the great importance of culture in human behavior has been spuriously established—thus the "towering scientific error." (This implies that the whole profession of cultural anthropology, like the acolytes of Trofim Lysenko, was either blinded by this brilliant critical experiment or perhaps—Freeman implies this but doesn't quite dare to say it—equally duped by their informants or by their

methods throughout the following 55 years.)

The thesis of "nature," Freeman tells us, had been countered with the antithesis of "nurture." Now, he admits, "the nature-nurture controversy of the 1920s has receded into history." Nevertheless, Freeman says, proposing the redeeming synthesis (the dialectical terms are his), "We have reached a point at which the discipline of anthropology, if it is not to become isolated in a conceptual cul de sac, must abandon the paradigm fashioned by Kroeber and others of Boas' students, and must give full cognizance to biology, as well as to culture, in the explanation of human behavior and institutions."

We now have assertions that need further comment—on Mead in Samoa, on the "Boasian paradigm," and on Freeman's synthesis.

Boas, Kroeber, Mead, and others were reacting not against "biology" *per se*, but first against a violently and increasingly nasty racism that proposed that the differences in behavior of different groups were a function of their genes. (Though Freeman's arguments might seem to open again the possibility of this kind of explanation, he does not deal with this in his discussion or synthesis.) The Boasians were also opposed to those extreme claims for human biology in general that seemed to limit the possibilities of education and social reform, for which they had considerable hopes. Thus in regard to "scientific" writing about adolescence in the 1910's and 1920's Mead wrote (in *Coming of Age in Samoa*) that the anthropologist who was learning "that neither race nor common humanity can be held responsible for many of the forms which even such basic human emotions as love and fear and anger take under social conditions, . . . [had] heard attitudes which seemed to him dependent upon social environment—such as rebellion against authority, philosophical perplexities, the flowering of idealism, conflict and struggle—asccribed to a period of physical development. And on the basis of his knowledge of the determinism of culture, of the plasticity of human beings, he doubted. Were these difficulties due to being adolescent or to being adolescent in America?" Note "many of the forms," and "basic human emotions," and "forms [taken] under social conditions." "Cultural determinism" was here a matter of the shaping of aspects of something biological, however underspecified. And this is even clearer in Mead's later reference (10) to the theme of her Samoan study as having been whether the prob-

lems ascribed to adolescent "*Sturm und Drang* and *Weltschmerz*," were cultural or inherent in "the adolescent stage of psychobiological development with all its discrepancies, uneven growth, and new impulses."

(I must interpose something here. Freeman disputes Mead's picture of the peacefulness of adolescence in Samoa, thus countering her "critical" argument for plasticity. But he later quotes, for other purposes, a work on the biology of adolescence by Herant Katchadourian [11] that notes that "research on ordinary adolescents has generally failed to substantiate claims of the inevitability and universality of adolescent stress." The historical moment, socioeconomic class, family values, or culture if you will is, in addition to individual variation, strongly at work here. Mead was right, even if her Samoan work were wrong.)

Freeman himself quotes (for different purposes) Boas's statement that "culture is not an expression of innate mental qualities [but] . . . a result of varied external conditions acting upon general human characteristics." Boas is clearly rejecting the idea of specific racial characteristics, but not "biology," which is what "general human characteristics" meant to him. For Boas worked, like Freeman, within a model in which all that was not "cultural" or "exogenetic" (in Freeman's use of Boas's term "exogene") was "biological" or "genetic." Alfred Kroeber, who argued for the study of culture as something that (like the subject matter of biology, chemistry, sociology, mathematics, or linguistics) should be treated at its own level of phenomenology, in its own terms, with the hope of clarifying regularities and discerning laws, is Freeman's example of the paradigm at its most benighted and dangerous. Kroeber was in such statements proposing the search for a science. Freeman confuses this (and this is an important part of the muddle) with the different question of the nature of the proper explanation of human behavior, of concrete events. For Kroeber, in regard to such explanation, the emphasis on culture was a *method* of investigation. As he wrote later (12), "Insofar as . . . social or acquired traits can be determined and discounted, the innate and truly racial ones will be isolated, and can then be examined, weighed, and compared."

Mead, Boas, and Kroeber knew that the "exogenetic" factors they chose to define as "culture" were, *pace* Freeman, only a component in human behavior and events. Which is not to say that they were not emphasizing culture, plas-

ticity, possibility, and the hope for social and pedagogic rather than "biological" ameliorative programs and what seemed to them a biologically based conservatism and pessimism. Which is to say that they had a position on the source and necessity of evil.

It was evil that Mead denied in her summary of Samoa, although not in her accounts. In fact in some publications (for example 13) she did write of the "opposite tendency, the rebellion of individuals within [component social] units against . . . subordination to [hierarchical orders], and their use of a place in a component unit to foment trouble and rivalry with other units." But her emphasis was on what seemed, to Americans weary and fearful of competition and violence, pastoral values. Freeman emphasizes the opposite. It should be noted that in his selective collection of evidence against Mead's picture of Samoa his own methodology (as represented in this study at least) is highly vulnerable by present standards. He simply does not contextualize his materials adequately so that they can be evaluated as data. Neither did the early Mead, who wrote later about the importance of locating informants and data in the social context, foreshadowing the important present-day concern with interpretation, with hermeneutics. Freeman's method is, in fact, a naïve positivism, as though social behaviors could stand as "facts" in themselves, without qualification of their meaning within the psychological and social worlds of the involved actors. (He occasionally comes up with simply silly things like a table on the percentage of virgins based on information from interviews, in a community whose official overt public morality is Calvinistic Protestantism.)

The matters Freeman calls attention to are, in his words, "the darker side" of Samoan life—murder, suicide, passion, rape, fear, aspects of sexual control. And, as for carefree adolescence, there are "high rates of delinquency," including sexual offenses, adolescent suicide, and so on, and thus "Mead . . . was at error in her depiction of the nature of adolescence in Samoa, just as she was . . . in her portrayal of other crucial aspects of Samoan life."

Although he has a (very) few phrases here and there about such matters as endocrines, the limbic system, and ultra-paradoxical states of brain activity, these are really only used as figures of speech and as rhetorical devices. What is emphasized in opposition to Mead's "over-emphasis on culture" is this darker side of Samoan life. "Samoan character . . .

has two marked sides to it, with an outer affability and respectfulness masking an inner susceptibility to choler and violence." Throughout most of the rhetoric of the book it is clear that such qualities as affability and respectfulness are seen as closer to society and "culture" (representing a socialized mask and the necessary controlling function of social forms and institutions), whereas such qualities as choler and violence are on the side of biology and universal incorrigible humankind. This is a Hobbesian view of the relation of man and society and has not passed much beyond the early opposition of the nature and nurture debate. This is the core of Freeman's passionate attack on Mead, who concentrated, as he tells us, "exclusively on the domain of the cultural, and so neglect[ed] much more deeply motivated aspects of Samoan behavior."

I will return once more to biology and anthropology, but first something more may be said about Mead's work in Samoa in the face of this attack. I have noted that Manu'a had special features that may, in spite of Freeman's arguments to the contrary, have made it in the 1920's (and later) different from other parts of Samoa, but surely Mead did distort the Samoan reality in the directions and for the motivations Freeman emphasizes and puts to polemic use. Nevertheless his emphasis on what she neglected or played down, on her "preposterously false" claims, neglects evidence that she did perceive an important dimension of Samoan culture, that she presented not a false picture but an insightful partial picture, which represents not only an aspect of Samoan reality but of Polynesian reality in general. Two important later students of Samoa, Lowell Holmes and Bradd Shore, both credited her with this. Holmes, who worked later in Ta'u itself, found the people to be, in fact, "gentle, cooperative, low-key, and submissive" (14). Shore suggests how such characteristics are correlated with one dimension of Samoan social structure and "dark" characteristics with another. (Freeman characteristically notes with approval the aspects of the work of Holmes, Shore, and other later workers in Samoa that counter Mead's report and then ascribes their findings congruent with hers to their having been taken in by Mead's reputation, and he has tried to discredit them in quoted public statements in various *ad hominem* ways.) It must also be noted that many careful studies of other parts of Polynesia, which differ from Samoa in social forms and post-contact (with Europe and America) histories and political

situations, show the casual, nonviolent, nonstriving, present-oriented, sexually relatively casual ethos that Mead over-generalized and misplaced in her description of Samoa to be in fact a dominant and valued cultural style, albeit a style often conflicted, defended and built out of recalcitrant human (both biological and social) materials (15). Thus Mead, whatever her paradigmatic predisposition, was able to see and to allow some Samoans to tell and show her something about Polynesian culture that Freeman could not. Freeman's blind spots and areas of clearer vision are different from Mead's. The problem, as graduate students know, is one of context, and the nature of the observer as part of that context.

This book seems to me to be a prisoner of the same ideological conflicts about the moral relation of man to society that generated the nature-nurture opposition in the first place. Its rhetoric, as well as much of its subject matter, is of battle, of confrontation, of the enlightened against the benighted. This rhetoric has been responsible for the small tumult of its reception in the popular media.

Both affability and choler among the Samoans are problems of biology *and* of culture, among other things (the assumption that "biology" and "culture" are analytically exhaustive is itself suspect, but that is another problem), and neither is necessarily more "external" than the other. (Think of the socially produced murderousness of the sometimes internally gentle soldier.) In nonideological ways some anthropologists are, in fact, concerned with biocultural transactions—problems of relations to the ecosystem, of nutrition, of individual development, of stress, of emotion, of biological constraints on cognition, among others. Some, intrigued by such problems, are interested in problems of universals and of the nature of the structures underlying and limiting variation. Others, however, follow Kroeber's *method* in the search for the possibilities of creative and adaptive innovation revealed in various historical and ecologically varied communities as the contribution to the understanding of the nature and components of humankind that they are best trained to undertake. And some will continue to feel free to try to make of culture and society an autonomous discipline and to pursue the implications of that strategy.

Freeman still could well deepen our understanding of Samoa and enhance the possibilities of a productive synthesis of biological and cultural perspectives through an ethnography based on his

years of work there. It would be by a compelling positive demonstration that he would contribute to our understanding. He has only served to cloud it in this volume.

ROBERT I. LEVY

Department of Anthropology,
University of California at San Diego
La Jolla 92093

References and Notes

1. M. Mead, *Coming of Age in Samoa* (Morrow, New York, 1928); *Social Organization of Manu'a* (Bishop Museum, Honolulu, 1930); "The Samoans," chapter 9 of *Cooperation and Competition among Primitive Peoples* (Beacon Press, Boston, 1937); see also (10).
2. T. Schwartz, introduction to *Socialization as Cultural Communication*, T. Schwartz, Ed. (Univ. of California Press, Berkeley, 1976).
3. M. Harris, *The Rise of Anthropological Theory* (Crowell, New York, 1968).
4. B. Shore, *Sala'ilua, A Samoan Mystery* (Columbia Univ. Press, New York, 1982).
5. E. M. Lemert, "Forms and pathology of drinking in three Polynesian societies," in *Human Deviance: Social Problems and Social Control* (Prentice-Hall, Englewood Cliffs, N.J., ed. 2, 1972).
6. F. Keesing and M. Keesing, *Elite Communication in Samoa* (Stanford Univ. Press, Palo Alto, Calif., 1956).
7. L. Holmes, "A Restudy of Manu'a Culture: A Problem in Methodology" (Ph.D. dissertation, Northwestern University, Evanston, Ill., 1957); *Ta'u: Stability and Change in a Samoan Village* (Polynesian Society, Wellington, New Zealand, 1958).
8. Isaiah Berlin's writings on the Counter-Enlightenment include *Against the Current* (H. Hardy, Ed.; Viking, New York, 1980) and *Vico and Herder* (Viking, New York, 1976).
9. G. Stocking, *Race, Culture and Evolution* (Free Press, New York, 1968).
10. M. Mead, *Blackberry Winter* (Morrow, New York, 1972).
11. H. Katchadourian, *The Biology of Adolescence* (Freeman, San Francisco, 1977).
12. A. Kroeber, *Anthropology* (Harcourt, Brace, New York, 1923; rev. ed. 1948).
13. M. Mead, *Cooperation and Competition among Primitive Peoples* (Beacon Press, Boston, 1937).
14. L. Holmes, quoted in *Science* 219, 1044 (1983).
15. There is a large literature on Polynesian cultural and psychological characteristics. For an early review see R. I. Levy, "Personality Studies in Polynesia and Micronesia: Stability and Change," Working Paper No. 8, Social Science Research Institute, University of Hawaii (Honolulu, 1969; reprinted 1972). For a study of related problems in Tahiti, see R. I. Levy, *Tahitians: Mind and Experience in the Society Islands* (Univ. of Chicago Press, Chicago, 1973) and "Tahitian gentleness and redundant controls," in *Learning Non-Aggression*, A. Montagu, Ed. (Oxford Univ. Press, New York, 1978).

Five Decades of Physical Anthropology

A History of American Physical Anthropology, 1930–1980. Papers from a symposium, Detroit, April 1981. FRANK SPENCER, Ed. Academic Press, New York, 1982. xvi, 496 pp., illus. \$44.50.

In 1930, the American Association of Physical Anthropologists (AAPA) was founded by Aleš Hrdlička and 83 other scientists, most of whom were anatomists. They adopted the *American Journal of Physical Anthropology (AJPA)*, which Hrdlička had started in 1918, as the official publication of the new society. Fifty years later these events were celebrated in Charlottesville, Virginia. And a major portion of the 1981 meeting of the AAPA in Detroit, Michigan, was devoted to reminiscences, ruminations, records, and revisionist historical sketches on a wide array of subfields in physical anthropology.

Twenty-four of the shorter papers were published as a jubilee issue of the *AJPA* (56, 327–557 [1981]). Most of them, including ten that focus on dead (but by no means extinguished) stars, were written and delightfully read by senior members of the profession.

A History of American Physical Anthropology, 1930–1980 consists of the generally longer, less personal accounts, mostly by younger scientists. Few of them are able historians. However, chapter 1, "The roots of the race concept in American physical anthropology" by Brace, merits a wide readership and high marks for erudition. Brace squarely confronts racist influences on

the two chief founders of institutional physical anthropology in the United States—Hrdlička, based at the American Museum of Natural History, and E. A. Hooton, with whom most of the second generation of physical anthropologists studied at Harvard.

Brace argues that Hrdlička was indirectly influenced by the American craniologist Samuel George Morton via his mentor and idol in Paris, the brainy Paul Broca. Hooton admired Sir Arthur Keith, who was tainted by Haeckelian racism. Fortunately, some of Hooton's intellectual progeny left the white sheets on the bed. Sherwood L. Washburn, a Hooton student who in his turn trained many anthropologists of my generation, brainishly resisted the quantitative racism and elitist eugenics of his teacher and stays ever ready to battle others of that ilk.

Brace's closing comments are upbeat. He reiterates the modern view that we should abandon the concept of race altogether and instead record the gene frequencies and traits of populations that are identified simply by their geographic localities. This genotypic and phenotypic information is to be interpreted in terms of historical and proximate selective forces.

If we classify the chapters according to the triune model of physical anthropology, half (11 through 20) are on aspects of human variation; a fourth (3 through 7) are on primatological subfields; and three (8 through 10) cover paleoanthropology.

Human variation is the least newsworthy realm of physical anthropology. Yet, unlike that in paleoanthropology and, to a lesser extent, primatology, basic research in human variation has many applications for human health and forensic sciences. Its practitioners have ruled the AAPA benignly and served as editors of the *AJPA* for many years, during which time membership has grown to include more than 1100 persons, including numerous students and foreign scientists.

The chapter by Weiss and Chakraborty on genes, population, and disease is one of the best in the volume. It contains a balanced historical review of changing ideas on the relative importance of selection and drift, adaptive and nonadaptive traits, classical and balance theories of genetic polymorphism, the genetic effects of radiation, and other long-standing controversial issues of population genetics. The authors justly praise Frank Livingstone's landmark hypothesis linking subsistence practices in Africa with the maintenance of the malaria-based human genetic polymorphism, sickle-cell trait. This work paved the way for other studies on cultural factors in the ecological genetics of certain human diseases. But despite a "wealth of progress" (p. 394) definitive solutions have not been forthcoming because of the complexity of most diseases and the cultural and genetic processes that govern their expression.

Physical anthropologists have contributed notably to studies on human ecology (reviewed by Little), growth and development, physiology, and adaptability (reviewed by Beall and by Little), paleopathology (reviewed by Ubelaker), and forensics (reviewed by Thompson). These subfields appear to have healthy futures. Additional promising subjects for study by physical anthropologists are nutrition (including paleonutrition), biological aging (Beall, p. 456), and historical demography (Harrison, p. 469). All of these areas would benefit from greater interdisciplinary emphases instead of the more common multidisciplinary approaches. This means that students will have to declare their research topics early and include a variety of archeological, cultural anthropological, basic natural scientific, and medical subjects in their programs. Postdoctoral studies and joint M.D.-Ph.D. degrees probably will become increasingly common among human variationists.

Human skeletal biology is moribund because of its long history of abuse by racial typologists and its largely descriptive nature (Armélagos *et al.*; Lovejoy *et al.*). Armélagos *et al.* argue that functional approaches and perspectives,