

BOOK REVIEWS

Quest for the Real Samoa: The Mead/Freeman Controversy and Beyond, by Lowell D. Holmes. Bergin and Garvey Publishers, Inc. 1987. \$29.95.

Roger Sandall

Just when it seemed that Samoa had once more subsided into oceanic obscurity, two more contributions to the controversy over Derek Freeman's book *Margaret Mead and Samoa* have again brought it back into view. First there was Roy Rappoport's essay in *The American Scholar* (Summer 1986), "Desecrating the Holy Woman: Derek Freeman's Attack on Margaret Mead." Suavely skirting the embarrassments of *Coming of Age in Samoa* as a description of Samoan life, Rappoport expanded on the larger significance of what he called "mythic truth" and claimed that "texts" like *Coming of Age* are "assimilated into a mythic corpus which reaffirm and revitalize enduring *general* values at the same time that they legitimize changes in *specific* conventions—rules, usages, understandings—by which social life is assessed or even regulated." Yes, he announced with satisfaction, *Coming of Age in Samoa* is a myth. And for America the book was also "a text of liberation, a myth of enlarged human possibilities."

This approach was somewhat breathtaking to those of us whose understanding of the role of anthropology is more mundane, and we were therefore interested to see what Professor Holmes might have to say. An anthropologist who has worked in Samoa, and who is avowedly hostile to Freeman's interpretation, he is at the same time reassuringly more down-to-earth than Professor Rappoport. Not "mythic truth" but plain old everyday truth would seem to

be his forte, an impression reinforced by the photograph of him doing the fieldwork in Samoa, some 33 years ago—work on which his career has been built. His hair is short. His notebook is in his hand. "In comparison to other Polynesian peoples I found Samoans very conservative in regard to sex," he writes. And when he admits that "during the entire residence in Samoa (1954–55) it was impossible to obtain details of sexual experience from unmarried informants, though several of these people were constant companions and part of our household," it is hard to resist a surge of sympathy. Holmes had gone to Samoa to restudy Mead's own village. And here he is, admitting that his informants wouldn't even talk about this central feature of her work.

Yet nothing about Samoa is straightforward—least of all what anthropologists have to say about sexual matters. And Holmes himself is noticeably ambivalent about Mead. He writes on p. 103 that "I could not agree with Mead on the degree of sexual freedom supposedly enjoyed by young people on Ta'u." But only a few pages later Mead herself is approvingly quoted to the effect that "sex activity is regarded as play; as long as it remains informal, casual, meaningless, society smiles." At this point (if not long before) one realizes that sorting out the arguments and counterarguments presented in *Quest for the Real Samoa* will not be easy.

Perhaps the most sensational suggestion in Freeman's book, and one of the most damaging, was that Mead was duped by her informants. Holmes first expresses his confidence that anyone as keenly perceptive as Mead was unlikely to be systematically deceived. He then implies (criticizing Freeman's use of sources) that the charge that Mead was duped has been exclusively drawn"—believe it or not"—from Nicholas von Hoffman's book, *Tales From The Margaret Mead Taproom* (1976: 97) "which is nothing but a spoof on anthropology."

This is not correct. In Freeman's *Margaret Mead and Samoa* the sources for this supposition are, first, the psychologist Eleanor R. Gerber's 1975 dissertation, and second, what Freeman has personally heard from the Samoans themselves. Freeman says on page 289 of his book that "the explanation most consistently advanced by the Samoans themselves . . . is, as Gerber has reported, 'that Mead's informants must have been telling lies in order to tease her'" (my emphasis). And he concludes a two-page discussion of the question by warning that "we cannot, in the absence of detailed corroborative evidence, be sure about the truth of this Samoan claim." Furthermore, Freeman returned to the subject of the "duping issue" in pages 128-130 of the Special Volume 6, No. 2 of *Canberra Anthropology*, "Fact and Context in Ethnography: The Samoan Controversy" in October 1983. Here he answered both Holmes and six other critics whose work had appeared in the previous volume. And he included in his answer the revealing statement made by Holmes himself in 1962, that if Polynesian informants think an interrogator "would like a certain answer, they are quite willing to give it in order to make him happy."

Perhaps this last riposte was just too much for Holmes. At all events, although both of these volumes contain material of vital interest for an understanding of the Mead/Freeman controversy, neither of the principals is ever directly mentioned in Holmes' book. Nor do they even appear (except in a single obscure reference to a manuscript) in his bibliography. So on the "duping issue" one is obliged to report, first, that Holmes misrepresents the most publicly notorious of the allegations associated with Freeman's book, substituting a secondary and minor footnote reference for the principal source given in the text. (The Nicholas von Hoffman book is indeed referred to by Freeman, but only as an inci-

dental source at the back of his book, reporting, *inter alia*, "views commonplace among Samoans.") Holmes then compounds this delinquency by either suppressing or ignoring all reference to an important volume containing Freeman's replies to his critics in 1983—the first of these critics being Holmes himself, to whom the very title is addressed: "Inductivism and the test of truth: a rejoinder to Lowell D. Holmes and others."

This refusal to acknowledge even the existence of Freeman's rejoinder produces a very odd, anachronistic effect. Things all seem in the wrong order, or as if the evidence at a trial were being wantonly presented out of sequence. (Of course this is only if you *know* the other evidence exists. Most readers don't.) Holmes reprints most of his 1983 criticisms of Freeman, unamended, but without telling us where they originally appeared, and without printing Freeman's 1983 replies. For example, on the question of levels of violent crime in Samoa Holmes had questioned some of the figures Freeman cited. In places like Samoa, governmental classification, collection, and sheer record-keeping are often somewhat haphazard. To the nonspecialist, therefore, a degree of skepticism regarding arguments from such statistics would seem appropriate. Besides, claimed Holmes, "Rape and other violent crimes tend to be urban phenomena in the Pacific, and R. G. Crocombe has pointed out that 'within Western Samoa, nearly 70% of the reported crime is said to be committed among the 18% of the population which lives in the capital.'"

How was that again? Is "*said to be*"? Naturally, anyone as alert as Derek Freeman had already caught this hearsay in his 1983 rejoinder, where he wrote: "An analysis of samples of rape behavior (32 cases) and of criminal aggression (61 cases) drawn from police records in Western Samoa for the years up to 1967 shows that only 18.75% of

these rapes were committed in Apia and environments . . . (as also) 18% of the cases of criminal aggression." So why is Crocombe's hearsay repeated, yet again, in this 1987 book? Why is there absolutely no mention of the immediate response, made directly to Holmes himself, in 1983?

The best construction one can place on the treatment of such matters in this book is that as a contribution to the Mead/Freeman controversy it is a singularly lazy, slack, and irresponsible publication. In a way this is all rather sad. Holmes himself appears to be a moderately competent and unpretentious ethnographer of the old school. He was never a Mead protégé—indeed, the careful descriptive work which marked his original restudy of her village led him to disagree with her findings on a number of important points, and made his relationship with the holy woman "stormy" for several years (she wrote a "terrible review" of his first book about Samoa in 1958). Furthermore, irony of ironies, it was these findings and their revelations which both attracted Freeman's attention and aroused his determination to undertake the research which, in 1983, culminated in *Margaret Mead and Samoa: the Making and Unmaking of an Anthropological Myth*.

These comments should not be taken as an unqualified endorsement of that book—great though my admiration is for Freeman's achievement. In my personal view he is overimpressed by Karl Popper; he has an unfortunate habit of needlessly beating his opponents about the head with charges of "inductivism"; in his more polemical deliveries he is too given to categorical formulations (must cultural determinism be quite so often rendered as *absolute* cultural determinism?); and his combative style and manner is not to everybody's taste. But in unmaking the Meadian myth he had set himself a task which only a man of dauntless spirit and iron will could have even contemplated.

And the full measure of the reformative work still to be done is well illustrated by the essay in *The American Scholar* mentioned earlier. Here, Professor Rappoport, President of the American Anthropological Association, announces that the propounding of ideas for social change, imaginatively constructed out of anthropological "visions" of primitive societies, is a large part of what modern anthropologists should be doing. It is, we are told, the "meaning" which *we* can derive from other social arrangements (and not an accurate *description* of those arrangements in themselves) which justifies the anthropological endeavor. These meanings are, as it were, intellectual resources to be culturally reworked into "myths"—and we should all be delighted that this is so. For Freeman to mistake Mead's book as having a primarily *descriptive* purpose was a naive, if pardonable, error. What she provided in *Coming of Age in Samoa*, we are now to understand, was, instead, something altogether more important—a vision.

The consequence of Professor Rappoport's essay is to legitimize ethnographic literary fantasizing as "social science." This development seems almost too opportune to be accidental. After all, anthropologists have nearly run out of primitive societies to study. But no matter. One may sit comfortably at home, and by drawing on the rich store of materials brought back in ethnography's last one hundred years, subjectively construct "visionary" schemes—new and exciting social orders of one's own invention. This being the case, the formal merging of anthropology and creative writing might be an appropriate administrative rationalization of the academic scene.

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Empty Promise: The Growing Case Against Star Wars by the Union of Concerned Scientists. Beacon Press, Boston, 1986, 238 pp. \$7.95 pbk.

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This book is a sequel to *The Fallacy of Star Wars* by the Union of Concerned Scientists (UCS), and it presents a new approach to criticism of the Strategic Defense Initiative. The attempt to prove the impossibility of ballistic missile defense, on the basis of rigorous scientific arguments, has been virtually abandoned. This is all to the good, for as the Fletcher Panel on Defensive Technologies and the Eastport Study Group have observed, such arguments simply do not stand up. Instead, the authors of *Empty Promise: The Growing Case Against Star Wars* build their case on technological forecasting and strategic analysis in a collection of ten essays.

The authors find the current progress in SDI technology slow and unpromising. They conclude that defensive systems can be easily foiled by cost-effective countermeasures and are vulnerable to a preemptive attack by a determined adversary. In the authors' judgment SDI also raises unique and intractable questions about battle management and software reliability. Strategically, they conclude that the development of Soviet countermeasures will put an end to the arms control process. The ensuing instability and growing reliance on conventional defense would also strain relations with our European allies.

Forecasting the relative pace of technological progress in defensive measures and offensive countermeasures is clearly the appropriate starting point for any critical analysis of SDI. So too is defining the impact of defensive systems on our strate-

gic objectives of crisis stability and escalation control. The authors of *Empty Promise* should be commended for identifying the key questions. Unfortunately, the usefulness of the UCS analysis is handicapped by oversights and omissions in technology assessment, a failure to examine all policy alternatives, and mutually contradictory statements by the various authors. A limited sampling of arguments from the various chapters vividly illustrates these shortcomings.

A broader coverage of the new technologies would have prevented serious omissions. John Tirman (executive director of the Winston Foundation for World Peace, Boston) downplays progress in SDI research as politically motivated demonstrations of old technology. He fails to discuss the two most important advances, the development of free electron lasers and adaptive optics. While the ultimate practicality of the free electron laser has not been proven, lasers four times as bright as earlier UCS calculations had demanded appear feasible. Yet neither Tirman's introductory chapter nor the other four chapters that cover technological issues deal with the subject. Quite baffling too is the authors' silence about advances in adaptive optics, the technology that enables the distortion-free propagation of a laser beam over very long distances. This highly selective approach is in sharp contrast to the comprehensive though somewhat dated descriptions of the state of technology that can be found in the recently published report of the American Physical Society (APS) on directed energy weapons.

Tirman devotes considerable effort to detailing the alleged X-ray laser scandal, purportedly uncovered by former *Science* magazine defense reporter R. Jeffrey Smith. Smith reported in November 1985 that the results of a key X-ray laser experiment conducted in March were thrown into question

by the discovery that monitoring equipment had been miscalibrated. Lawrence Livermore National Laboratory confirmed the experimental difficulty on November 12. Nonetheless, Edward Teller lobbied successfully for more funds, and Livermore proceeded with an additional test in December. Tirman weaves a sensational tale of scandal and deception out of these difficulties. He construes Smith's report as suggesting that Edward Teller used the faulty data to make exaggerated claims about a breakthrough in X-ray laser technology and thus secure an additional \$100 million for further tests. A more accurate account of the incident would have mentioned a subsequent article in which Smith quotes independent scientists reviewing the experiment on behalf of the General Accounting Office as saying that "the X-ray lasing has been demonstrated." Congressional program critics now admit that going ahead with further tests is "a legitimate scientific judgment call."

The chapters on battle management and software reliability follow the same pattern of fragmentary analysis. It is generally agreed that to insure survivability, the battle management system should have a decentralized architecture. Robert Zirkle (Ph.D. candidate, MIT) states without any justification that a decentralized system would be only 20% efficient. Extensive calculations carried out by Charles Seitz of the California Institute of Technology as part of the Eastport Group study on software feasibility indicate 80% efficiency. Taking issue with or at least mentioning these calculations would have enhanced the value of Zirkle's contribution. Greg Nelson and David Redell (both with the Digital Equipment Corporation) in their review of software reliability perpetuate the myth that defensive weapons are somehow unique in requiring large and complex software. In fact according to *Air Force Magazine* the

software needed for the advanced tactical fighter would require seven million lines of code, essentially the same as SDI.

The defense of space-based SDI components is a subtle undertaking that involves the tandem action of several, mutually reinforcing strategies. The most important approach, and one *not* discussed by Tirman and Peter Didisheim (legislative assistant to Rep. George E. Brown, Jr., CA) in their section on satellite survivability, is the use of decoy satellites augmented by electronic countermeasures. Such decoys would present a multitude of targets and confuse incoming anti-satellite (ASAT) weapons. This strategy, when combined with satellite maneuverability, may tip the balance in favor of the defense, even if the Soviets deploy sophisticated maneuvering ASATs. Whether the offense or the defense profits more from decoys is a complex question, requiring a systems analysis more comprehensive than the sketchy arguments given by Tirman and Didisheim.

The authors not only strike a tone of pessimism about developments in defense technologies, but also paint a brighter picture than warranted for the efficacy of offensive countermeasures. After a lengthy analysis Richard Garwin (IBM and Columbia University) settles on the fast-burn booster as the key Soviet countermeasure, and documents its feasibility with a reference to a preliminary 1983 report by the McDonnell-Douglas Corporation to the Fletcher Panel. But McDonnell-Douglas has also voiced concerns that the high acceleration of this device may cause uncontrollable oscillations in the guidance system, leading to loss of accuracy and possible destruction of the booster. Garwin cannot be unaware of these concerns, and should have explained his sanguineness. Even if a fast-burn heavy ICBM could be built, it could not deploy its payload within the atmosphere, immediately after booster burn-

out. It would still have to coast to an altitude of 150-180 km (as indicated in the APS Report) because the atmosphere at lower altitudes would interfere with the decoys. The APS Report flatly contradicts Garwin's estimate of 90 km as the optimal deployment altitude. Even with a fast-burn booster the offense is in the words of the APS Report "not home-free."

Jonathan Dean's chapter on the European reaction to the SDI is perhaps the weakest in the book, providing no references or footnotes. In the absence of footnotes it is very difficult to assess independently the various European positions in their proper context. Dean (former diplomat and arms control negotiator, adviser to UCS) portrays, for example, former French defense minister Charles Hernu as ambivalent towards SDI. Yet Hernu is quoted in *Nature* as saying "SDI is star peace not star wars," and "France must have its place in this star peace." When these and many other omissions consistently serve to strengthen the case against SDI, one is tempted to ask whether they reflect lack of awareness or the authors' value system.

The second shortcoming of the book is its failure to examine the full set of policy alternatives while pursuing a certain line of strategic analysis. One sorely misses, for example, any reference to Alvin Weinberg's proposal on defense-protected build-down when reading about SDI and arms control.

Garwin claims that proliferating fast-burn boosters with single warheads would be a cost effective countermeasure, for it would require a corresponding proliferation of expensive laser battle stations. This calculation may be true for boost-phase interception, but does not necessarily apply to later stages of the defense. Detailed technical objections aside, the argument assumes a *purely defensive* American response, ignoring other alternatives. Given that the

Soviets have to allocate two warheads to destroy one U.S. missile, we need build only one single-warhead missile for every two built by the other side, an indisputably cost effective option for the United States. In fact, nowhere in the book does the UCS succeed in devising an offensive strategy that can simultaneously defeat SDI and destroy our land-based missiles. When asked about it, Garwin dismisses the vulnerability of our land-based missiles by recommending the policy of launch-under-attack (i.e., we launch automatically as soon as we receive indications of a Soviet attack), a strategy Senator Sam Nunn has described as very destabilizing.

The UCS has always insisted on perfect defense, claiming that the consequences of limited defenses have not received the scrutiny they deserve. Thus Peter Clausen (senior analyst, UCS) undertakes a critical review of limited defense options. His central assumption, that a first-generation SDI system will not include boost-phase defenses, has been superseded by events. Early deployment proposals by the Secretary of Defense include boost-phase interception. Clausen suggests that limited defenses may encourage, rather than deter, a first strike by helping to "shield an aggressor against retaliation following an attack on the adversary's nuclear forces." This analysis is convincing only because it is vague. What level of protection constitutes limited defense? Boost phase weapons bring SDI effectiveness to the 90% range. The Soviet Union would have to launch its entire arsenal in a "self-disarming" first strike to hit 15% of our land-based missiles, a consequence that has escaped Clausen's scrutiny.

The major weakness of the book, however, is its mutually contradictory opinions and premises. The book after all is a group effort by the UCS, which advocates a well-defined set of policies. Thus we would

expect the book to present a coherent and unified position.

Tirman states that "virtually every expert sees the Soviets eventually emulating SDI." On the other hand, Garwin's calculation of the cost effectiveness of proliferating fast-burn single-warhead ICBMs (forgetting for the moment their questionable feasibility) is predicated on a *purely offensive* Soviet response to SDI. Proliferation ceases to be a cost effective countermeasure by anybody's calculation if one considers the additional cost of a Soviet SDI. According to the book, the Soviets will both build and not build strategic defenses of their own.

Garwin concedes that SDI will push the Soviets to abandon MIRVed missiles in favor of single-warhead ICBMs. But he thinks "it is pure sophistry to suggest that the threat to U.S. security is greater from ten warheads on a single (MIRVed) SS-18 than it is from ten of the same warheads on ten small single-warhead fast burn boosters." In a later chapter, however, Tirman observes that "the lesson is unambiguous: arms control—the elimination of MIRVs in this case—would have greatly enhanced U.S. security." Once again a contradiction. The authors tell us abandoning MIRVs will both enhance and not enhance U.S. security. These shortcomings are compounded by a polemical style that should prove distracting even to those skeptical of SDI.

Jonathan Tucker's chapter on attitudes towards SDI in the science and engineering community is also replete with gratuitous remarks. Tucker (Ph.D. candidate, MIT)

insinuates that recipients of SDI research funds are rewarded for their salesmanship, not the scientific merit of their research proposals. He is sympathetic to the Pledge of Non-Participation, a research boycott petition circulating on some campuses. But serious critics of SDI have criticized the petition for its crass generalizations. For example, the Pledge characterizes all SDI research as being "of dubious scientific validity." Jack Ruina of MIT, an SDI skeptic, regards the petition drive as an assault on academic freedom. Tucker gloats over the "growing attitude of noncooperation that will inevitably hurt the program." In fact, Tucker's sympathetic attitude towards the research boycott belies the ostensible support of the UCS for a prudent level of research in defensive technologies. The APS Report points the directions for an intensive research program in directed energy weapons. Signing boycott petitions, or virtually eliminating the SDI budget as Tucker suggests, is certainly not the way to answer the many questions raised in the APS Report.

As the subtitle indicates, the book promises to make the growing case against Star Wars. Alas, it is an empty promise.

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