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Lessons for Us All

Should a reader without any personal knowledge of Samoa have doubted the reliability of Mead's work? Regarding sexual experience, she claims that it is rare for both partners to be inexperienced; as she puts it, "I knew of only one such case, where two children, a sixteen-year-old boy and fifteen-year old girl, both in boarding schools on another island, ran away together. Through inexperience they bungled badly" (1973:150). This claim implies that she had a sufficient sample of liaisons and knowledge of sexual experience to judge that the one such case that she knew of was rare. Given that the book provides no such evidence, one might have been doubtful, and consideration of the quantity of intimate data such a judgement required should have reinforced this doubt. Of course, as we now know from the field materials, she had nothing like such detailed data. In addition, her contention regarding the failure of the young couple implies that she knows that their sexual experience was unsatisfactory and that this was due to their both being inexperienced. This would be an extravagant claim even about a couple one knew intimately from our own society, never mind a couple from a different society whose language one had just begun to understand.

If this implied omniscience did not strain credulity, the

claim of knowledge that follows it should surely have alerted even the most gullible. On the very next page Mead asserts that sexual artistry is so widespread that there is "no frigidity, no impotence, except as the temporary result of severe illness, and the capacity for intercourse only once in a night is counted as senility" (1973:151). Not only does she not provide us with any data to support such a claim, but it seems hardly credible that such data could have been collected in any reliable fashion. As we now know from her field materials, she had one informant who told her that he knew of no female frigidity or male impotence and who intimated to her that a particular sexual repertoire was standard operational procedure. That this same informant told her that "girls will always tell other girls how awful first night was, and make derogatory comments on lover's skill," for whatever it is worth, is not reported by Mead in *CA*, perhaps because it is discrepant with her claim of virtually universal sexual skill.

A reader knowing nothing about Samoa might have also noticed that comparisons crucial to Mead's argument are never made. For example, she asserts that the size of the residential unit is a key factor in producing a less troubled adolescence, but she does not compare the adolescent experience of girls from larger and smaller families. Similarly, she makes no effort to compare the adolescent experience of girls who remain chaste under the watchful eyes of the pastors and their wives with that of girls she claims are left free to engage in liaisons. Surely such comparisons would be appropriate to any genuine voyage of discovery. Lacking such comparison, one should have been skeptical of the reliability of her argument.

Furthermore, there is the general problem of the paucity or absence of supporting data for her argument. In *CA* she gives no indication of how she knows about the sexual experience of her subjects; she offers not even anecdotes to convince us that there is no guilt connected with premarital sex; she provides no evidence indicative of the Samoan view of natural functions of the body and sexuality; nothing

illustrates a lack of "idealism"; there is no investigation of "philosophical perplexity"; and, of course, no effort is made to construct a metric for comparing Samoan and American adolescent experience.

These are by no means all the defects that might have been observed by those innocent of all things Samoan, but surely they are sufficient to suggest that no knowledge of Samoa was required to have been skeptical of Mead's claims. How is it, then, that Mead's work became so influential? How could anthropologists and other eminent scholars have largely ignored such blatant defects? How could generations of university professors have included *CA* as required reading for students? How could such a flawed work have served as a stepping-stone to fame?

It is perhaps useful in answering this question to begin by admitting that I am among the countless scholars who failed to attend to the faults of Mead's work that required no knowledge of Samoa. So apparently is Derek Freeman, who tells us of his original "unquestioning acceptance of Mead's writings," and tendency to "dismiss all evidence that ran counter to her findings" (1983a:xiv).

I believe that there are two basic reasons for the general failure to recognize Mead's extensive methodological faults. The first of these is ideological: We wanted Mead's findings to be correct. We believed that a more permissive sexual code would be of benefit to us all. More important, her findings were a coup for the proponents of the importance of culture vis-à-vis biology. This perspective supported solving human problems by social change, whereas the emphasis on biology insisted that our problems were rooted in human nature and therefore ineradicable. In addition, the cultural emphasis was a tool against racism insofar as it diminished the linkage between biology and behavior. The fact that the message was delivered in a skillful and charming manner and that we had inherited a vision of a South Sea Island paradise enhanced the acceptability of findings that were ideologically palatable.

The second reason for the failure to note the many deficiencies in Mead's work derives from a pervasive problem of cultural anthropology as a discipline. From its inception, its practice has often been profoundly unscientific and positively cavalier in its willingness to accept generalizations without empirical substantiation. It tends to make little use of logic and mathematics in expressing relationships and seldom employs reasonably well-defined concepts. It therefore often produces propositions that are untestable. Relationships and concepts tend to be so ill-defined that they provide too much "wiggle room"—opportunity to claim that whatever test has been offered to falsify a claim has missed the intended meaning.

There were certainly social scientists, even in the 1920s, who were aware of the requirements of science and who demanded that their students adhere to them. Indeed, Mead herself was quite aware of these standards, as she indicated in a letter to Boas written from the field January 5, 1926. After almost two months of work in Manu'a, she asks Boas for advice as to how to proceed:

> Ideally, no reader should have to trust my word for anything, except of course in as much as he trusted my honesty and averagely intelligent observation. I ought to be able to marshall an array of facts from which another would be able to draw independant conclusions. And I don't see how in the world I can do that. Only two possibilities occurr to me and both seem inadequate. First I could present my material in a semi-statistical fashion. It would be fairly misleading at that because I can't see how any sort of statistical technique would be of value. But I could say "fifty adolescents between such and such ages were observed. Of these ten had step-mothers and five of the ten didn't love their step-mothers, two were indifferent and three were devoted. Fifteen had had some sex experience, five of the fifteen before puberty etc."[1] All of which would be quite valueless, because whether fifty is a fair sample or not could be determined only on the basis of my personal judgment. And saying you don't love your step-mother, or that you rebel against your grandfather but mind your older sister, or any of the

> thousand little details on the observation of which will depend my final conclusions as to submission and rebellion within the family circle, are all meaningless when they are treated as isolated facts.

The paragraph ends on a plaintive note to the effect that she doubts "whether the Ogburns [William Fielding Ogburn, a noted sociologist then at Columbia known for his emphasis on statistical rigor] of science will take any other sort of result as valid." As we now know from the overwhelmingly favorable reception of Mead's work, either there were not very many such "Ogburns of science" or they were so pleased with her findings that they were willing to overlook the work's scientific failings.

Though there are many anthropologists of today as ignorant of statistics as Mead was in the 1920s, her statement that the fairness of a sample of fifty depends upon her "personal judgment" is an egregious error. Of course, a "fair sample" means one that is representative of some population. To choose such a sample, one employs a process of randomization. In fact, Mead made no effort to choose a random sample even of the female adolescent population of Manu'a or of her three villages; as we have seen, some of her statements claim that she sampled all of the adolescent girls of the three villages, but in fact she did not. Furthermore, the question of the adequacy of a sample of fifty depends not on anyone's whim but on a well-established statistical principle. Large samples obviously provide more accurate estimates of a population characteristic than do small ones; the size of the sample required can be calculated for any prescribed degree of accuracy (see Blalock 1972:214–15). Whether this technique was widely known in the 1920s I cannot say.

The second possibility considered by Mead in her letter to Boas for meeting the canons of science was to

> use case histories, like this. "[X] is a girl of 12 or 13. . . . She is just on the verge of puberty. Her father is a young man with no title and a general reputation for shiftlessness. . . . She regards her playmates as so many obstacles to be beaten over the head. She has no interest in boys whatsoever, except as extra antagonists.". . . But to fill such case histories with all the minutiae which make them significant to me when they are passing before my eyes is next to impossible. And the smaller the details become, the more dangerous they become if they are to be taken just as so many separate facts which can be added up to prove a point. For instance, how many other little girls carry babies all the time, and how many other mothers go visiting. Facts which possess significance in one case but which are mere bagatelles of externality in another would have to be included in each case history or they would not be comparable.

Mead goes on to say that she has a lot of detailed data. "But how to use it? If I simply write conclusions and use my cases as illustrative material will it be acceptable? Would it be more acceptable if I could devise some method of testing the similarity of attitudes among the girls, in a quantitative way. . . . I wouldn't feel any wiser after collecting information in that style but maybe the results would be strengthened."

What Mead is claiming is that she is quite confident of her conclusions even though she has not done any formal sampling, and therefore the only reason to do such sampling would be to convince others. In addition, she claims a kind of insight based upon context and judgment that can never be justified by a presentation of observations. Her colleagues, then, must either except her judgment or not. Of course, even if one concedes that an observer may know more than she can demonstrate, one might still contend that a concession might more readily be made if one were presented with the observations that are available. Mead is asking to be excused from collecting and presenting a fair sample and supporting her conclusions with case histories. What she wants is permission to present data simply as "illustrative material" for the representativeness of which one will simply have to take her word.

Mead need not have worried, for Boas turned out not to be one of the Ogburns of science. On February 15, 1926, he wrote her as follows:

> I am very decidedly of the opinion that a statistical treatment of such an intricate behavior as the one that you are studying, will not have very much meaning and that the characterization of a selected number of cases must necessarily be the material with which you have to operate. Statistical work will require the tearing out of its natural setting, some particular aspects of behavior which, without that setting may have no meaning whatever. A complete elimination of the subjective use of the investigator is of course quite impossible in a matter of this kind but undoubtedly you will try to overcome this so far as that is at all possible. I rather imagine that you might like to give a somewhat summarized description of the behavior of the whole group or rather of the conditions under which the behavior develops as you have indicated in your letter to the Research Council and then set off the individual against the background.

Boas adds that Mead might follow the "methods that is used by medical men in their analysis of individual cases on which is built up the general picture of the pathological courses that they want to describe." To be fair to Boas, he does not quite give Mead carte blanche to present only illustrative material, but he is totally unconcerned with the question of the representativeness of her samples, and of course he offers no criticism of her comment on sample size. His acceptance of the context argument as the basis for not having to justify by empirical example calls to mind the twice-told tale of the "Belfish Bushmen." As Hans Reichenbach told the tale in his course on the philosophy of science, an anthropologist just returned from field work among the Bushmen was asked by a physicist colleague what was the most striking thing about them. He replied that their one extremely salient characteristic would be very difficult to convey to someone who had not experienced Bushman life. The physicist, a man of many parts, replied that he appreciated the difficulty but urged the anthropologist to make an effort nevertheless. "Well," the anthropologist replied, "the characteristic that is most pronounced is how belfish they are." When the physicist asked for clarification, the anthropologist repeated that it would be very difficult to explain the term to one who had not

experienced Bushman life. "Perhaps," the physicist politely said, "it would help if you would provide me with some examples of things the Bushmen do that are belfish." "Yes," said the anthropologist, "that is easily done; for example, one day I desperately needed an ax to cut down some trees, and when I asked a Bushman who was making no use of his ax if I might borrow it, he absolutely refused. On another occasion I saw a Bushman who had much more game than he could consume refuse to share any of it with another Bushman who had none." "Well," said the physicist, "I don't want to leap to conclusions, but 'belfish' seems to mean something very like our 'selfish'!" (It should, however, be noted that real anthropologists have claimed that real Bushmen are exceedingly generous; recently the limits of this generosity have received greater emphasis.) "By no means," said the anthropologist. "Without having experienced Bushman life it is impossible to understand how profoundly different the two terms are." "Yes, indeed," said the physicist, "I can see how that might be so; perhaps, however, you could provide further help by giving an example in which a Bushman does something that you would call 'belfish' but we would not call 'selfish.' " "That I cannot do," said the anthropologist. "The difference can only be known to those who have experienced Bushman life!" With the blessing of Boas, the "father of American anthropology," Mead provided a charter for nonempirical justification: "If you weren't there with me, you simply can't really understand. Therefore you may take my word for things or not, as you like."

What the field materials and *CA* actually indicate is that on a number of matters in which observational interpretation might well depend on context, Mead's generalizations rest on virtually no observations whatsoever. As we have seen, Mead conducted no inquiry among her adolescent girls regarding guilt or fear of divine retribution for violating sexual mores; she seems to have relied on the comments of one white schoolteacher. One girl told her that premarital sex was bad, and she chose not to report this; perhaps she believed that

such a comment was atypical, but perhaps she was wrong. One might conceivably believe that premarital sex was "bad" and still not feel guilty; perhaps the remark meant only that one feared for one's reputation if found out. The point is that Mead gives us no reason for having confidence in her judgment on this important point.

On somewhat less subtle matters such as Mead's contentions about the absence of frigidity and impotence, as we have seen, she had but one informant. Surely this would not have been claimed had she been required to present supporting observations. Of course, the experience of sexuality and the attitudes surrounding it are indeed subtle matters. We may demonstrably know more about them than we can clearly explain, or we may think we do and be mistaken. It might therefore be thought that Mead's meager empirical support for her contentions is connected with the subtlety of her subject matter. This, however, is certainly not the case. Mead's quite prosaic monograph *The Social Organization of Manu'a* is plagued by the same practice of almost never telling how things are known. For example, in an account of so-called fine mats (*'ie tōga*), she asserts a number of alleged equivalences; for example, "a fine mat is a two guinea payment; four large tapas a mere two pounds." "One fine mat is the virtual equivalent of any of the following: one large pig, four large tapas each four to five yards in length, ten small tapas, forty floor mats, ten bed mats, a roll of sennit three feet long and eight to ten inches in diameter" (1969:74). Clearly, we are dealing with no subtle matters here, yet not a clue is given as to how this is known. Was it told her? Did she get these equivalences by asking for equivalences? Did she observe any such transactions? I was unable to find any evidence for these assertions in the field materials. Since the perceived quality of fine mats varies greatly depending on a number of technical features such as the fineness of the weaving, decoration, size, and so forth, as well as its history, Mead's equivalences seem greatly oversimplified at the very least. Except for rather low-quality fine mats sold in the market, I have never observed

their being used in market exchange; there are occasions that require the presentation of one or more fine mats, and, according to my observations, no one substitutes currency or other valuables. Unfortunately, we do not know if Mead observed anything different.

A number of objections to my critique immediately spring to mind. In the first place, it seems to impose a rather positivistic mold on all anthropological inquiry. To this objection I say that I have no such intention; however, if one is not going to do science then one must avoid the impression that it is being done. One may not claim that a theory—for example, that there is a biological basis for adolescent stress—has been tested and disproved by a counterexample. If, in contrast, one is to do a creditable humanistic inquiry, then it ought to be filled with the sensitive observations that will help readers to see why one thinks as one does. Occasionally Mead does meet this test, and doubtless this is the genuine strength in her work.

A second set of objections is that this was Mead's first fieldwork, that she was only twenty-four at the time, and that today there is a much higher standard of fieldwork and argument. These particular objections were widely stated in the numerous reviews and commentaries on Freeman's critique, and one notes at the outset that they are frequently combined with a stout defense of Mead's work. It reminds one a bit of the folktale about the lady who borrowed someone's pot and failed to return it; when called upon to give it back she said, "In the first place, I never borrowed it, and besides the handle was broken!" Be that as it may, Mead's age and inexperience are irrelevant to my inquiry because I am not concerned with an appraisal of Mead; my interest is an appraisal of the work. I am perfectly content to let others judge whether her later work was done with greater skill. The question of the time at which the work was done is of concern to me, however, because it raises the question whether my critique has any relevance to anthropology as it is currently practiced. Here, in my view, lies the genuine wider significance

of the Mead-Freeman dispute. That Mead's seriously flawed work, which is filled with internal contradictions and grandiose claims to knowledge that she could not possibly have had and is so weakly supported by data, could have survived and formed the foundation for an illustrious career raises substantial doubt regarding improved standards of research. Though it would be inappropriate to cite current research that has all the scientific failings of Mead's work, I have many such examples in mind. I can also bring to mind some excellent works that are not lacking in scientific rigor and others that frankly present themselves as more humanistic inquiries rather than making unwarranted claims. Finally, I do not gainsay the achievement in anthropology of some greater measure of understanding, both scientific and humanistic, than existed in the days of Mead. I do, however, insist that her scientific stance and her unscientific practice are far too prevalent even in these times. That a person of such conspicuous talent could have produced such a flawed work and that it was so widely accepted and praised by so many should serve as an object lesson to us all.

Partially confirming the currency of my critique is the fact that virtually none of the numerous reviews of the Mead-Freeman controversy point to the profoundly unscientific nature of her work. This failure is doubtless to some extent attributable to attempts at damage control or "spin doctoring," but some of the comments indicate a rejection of scientific practice on the basis of a naive understanding of the nature of such practice. One example comes from a review by one of our best Samoan ethnographers, Bradd Shore, whose 1982 monograph on the Samoan village of Sala'ilua is filled with valuable insights on Samoan culture and personality. Shore (1983) perceptively notes that a number of Freeman's criticisms of Mead suffer from what Schwartz (1983:919) has called a "maximally contrasting" bias—a tendency to say that things are precisely the opposite of what one's opponent has said that creates an equally misleading impression. Shore helpfully points out a number of other instances of this bias.

Unfortunately from my perspective, he regards Freeman's misleading and oversimplified rejection of Mead's claims as an exemplification of "Popperian" science (1983:943):

> What is wrong, in the end, with the kind of absolute, formal refutation that is the hallmark of Popperian science and that informs Freeman's book is that it pretends the "facts" of human existence operate like some bloodless, mindless machine according to the strictest principles of Aristotelian non-contradiction. And yet human life is riddled with contradiction, with colliding impulses and incompatible values. Simply bringing to bear on a statement evidence that is contradictory is not in the human sciences sufficient to disprove that statement.

Shore seems unaware that there is nothing illogical about people having "impulses" and/or values that lead them sometimes to do one thing and sometimes another, about wishing to do something and feeling that one ought not to, about being torn between conflicting impulses and/or values. It is only in applying the word "contradictory" to such instances that one might be led astray. Logically, "contradictory" means "mutually incompatible," but there is nothing incompatible about "contradictory" impulses and values. It is therefore not the fault of logic that Freeman sometimes oversimplifies the human experience. Shore's "bloodless, mindless machine" is set forth as though it defined the proper range of the application of logic: clearly, there is no more justification for mutually exclusive arguments regarding humans than there is for such arguments regarding "machines." Logically contradictory evidence refutes an argument. Constructing arguments that are capable of refutation is the hallmark of science. I cannot think of a polite term for arguments not admitting of refutation; perhaps the phrase "not even wrong" is all the condemnation that is required. Shore's antiscientific bias is capped by his statement that "Freeman seems to have redesigned human nature to satisfy the requirements of positivistic methodology." There is nothing in scientific methodology that demands that our impulses and values be configured so as never to push us in

opposite directions. One might as well claim that science denies, a priori, the opposition of physical forces on the grounds that they are "contradictory."

In fairness to Freeman it must be said that one of Shore's criticisms of his work is plainly unfounded and the result of a misunderstanding. Shore asserts that he "cannot concur with the blanket assertion that Samoans' 'engaging affability,' their smiling or politeness, is always 'in reality, a defensive cover for their true feelings,' a mask for aggression, or depression, or hostility" (1983:939). After providing an example of a difficult social situation in which a chief is being criticized, Freeman says, "As these examples indicate, it is usual, especially in demanding social situations, for Samoans to display an affable demeanor which is, in reality, a defensive cover for their true feelings—to be as they themselves put it, 'smooth on top but whirling beneath' [*e manino i luga' ae vivili i lalo*]" (1983a:217). Clearly, Shore's "always in reality" is not what Freeman said or meant. Furthermore, the Samoan saying plainly indicates that Freeman is perfectly aware—as are Samoans—of the ordinary complexity of human experience.

My describing Shore as a gifted ethnographer despite all this may seem a vindication of antiscience, but I do not believe this to be the case. All of us develop understandings of behavior without knowing how we have come to them; some of these we cannot articulate or may articulate in ways inconsistent with our practice. Nevertheless some of these understandings may be demonstrably correct; of course, some may also be demonstrably incorrect. For example, any ordinary native English speaker can tell with considerable accuracy when someone is telling a joke. To tell how one knows is of course much more difficult, and perhaps few of us could provide a precise rule for making such a judgment. It is therefore not at all necessary to practice science to be good at understanding human behavior. The difficulty in the practice that Shore advocates is that it does not require the careful testing of insights by means of observations. Therefore

neither he nor the reader should feel confidence in his insights until they have been tested. I know that his assertions are often reliable because I have tried some of them out; occasionally, too, I have noticed that an unconfirmed suggestion of his is mistaken. When such a gifted observer cannot articulate precisely how he knows something, he may of course still tell us what he thinks he knows and present us, so far as he is able, with the observations that confirm his insight. If there are no such observations, he may of course still be right, but neither he nor the rest of us can tell.

While plainly recognizing that Mead's emphasis on "obedience, gentleness and cooperation" (1983:936) is one-sided, Shore attributes this to "the notion of cultural configuration—being worked out in its early phases by Benedict," which may have led her to simplify her observations "so as to conform to a single dominant theme." But such an explanation of her error overlooks the fact that this simplification neatly fits her argument; the opposite emphasis would hardly be consistent with a more tranquil adolescence. The same is also true of her misleading emphasis on sexual freedom. In both cases we know that she knew better, and had she wished she could easily have made the opposite case. It may be better for the reputation of Mead and anthropology to defend Mead's misleading statements on the grounds of commitment to a high-flown concept, but the convenience of her misleading statements seems to have a simpler explanation. Those who still believe that Mead's misleading generalizations are the result of looking for dominant themes might note that one of these generalizations is that "neither poverty nor great disasters threaten the people to make them hold their lives dearly and tremble for continued existence" (1973:198). Mead experienced both a destructive hurricane and a heavy storm on January 12, 1926, that resulted in enormous damage to both homes and crops. Such storms are by no means infrequent. Surely her generalization here cannot be attributed to looking for patterns, but it is consistent with the requirement of a halcyon Samoa. The storm is, of course, not

reported in *CA* or elsewhere.

A similar antiscientific perspective is manifest in the review of the Mead-Freeman controversy provided by Annette B. Weiner (1983:909). Weiner has a number of perceptive things to say about Samoan culture. In particular, she points out how Freeman fails to take account of Samoan sensitivities regarding sexuality, religion, and history and therefore asks us to accept Samoan accounts at face value that ought reasonably to be interpreted in the light of these sensitivities. But she condemns Freeman for holding the view that "there is one Samoan reality, and there are no contradictions to be found in understanding an individual's perceptions, negotiations, and interactions" (913). She cannot mean that mutually exclusive things are true; perhaps she means by "contradiction" something like what Shore means.

Weiner later says that "the notion that there is a single 'true version' of history, genealogy, or oral traditions comes from a Western positivist position in which the scientist believes in the existence of ultimate truth." It is difficult to imagine a scientist who would use the term "ultimate truth," but, be that as it may, Weiner's intention seems again to be to remind us that we must be careful in accepting what Samoans say about such matters as their history. Noting the comments of the anthropologist Malama Meleisea, Weiner says that "different versions of genealogies, oral traditions, histories, and events are told and accepted and the 'truth' depends on situations such as one's relationship to others and the political relationship of one's residential village to other villages" (1983:914). Certainly it is correct that Samoans like all other peoples construct accounts of events often with an eye to their social use. Some of these accounts, to speak plainly, are patently untrue and even self-contradictory. That Samoans twist the truth like the rest of us in no way indicates that they are lacking a sense of *the* truth. If there is any need to demonstrate this, I might note that Samoans are as quick to utter the epithet "liar" (*pepelo*) as any people I have known and are not unaware of efforts to construct accounts of history

to serve some political end.

Insofar as one is considering propositions that are in principle verifiable, it makes no sense to speak of multiple truths, though it has become fashionable in contemporary academia to do so. Of course people may have different experiences, depending, for example, on their social status or a host of other factors. If one wishes to call these different experiences different "truths" one may do so, but these different truths are not logically contradictory. Observers may even see—in a literal sense—the same event differently depending on their physical positions, but there is nothing logically contradictory in this either. Freeman may say that he has refuted Mead on some point when in fact he has not. This is not the fault of positivist science or logic. Often both Mead's statements and Freeman's have enough ambiguity in them that it is impossible to know just what they mean; in such instances it is of course impossible to speak of verification or of contradiction.

The ambiguous notion of multiple truths has become a charter for the idea that "anything goes," that you have your truth and I have my mine, and ultimately for a rejection of empirical verification. In its most egregious form this approach holds truth to be merely a label employed by competing interest groups to sanctify beliefs useful to furthering their ends. From this perspective, evidence for propositions is of little interest; competing claims are seen simply as furthering competing ends, and the demand that propositions be stated in a manner capable of verification is vilified as ethnocentric positivism and doubtless a tool of some ruling class. In such a climate it is not surprising that the profoundly unscientific nature of *CA* has hardly entered the debate.

No doubt a more scientific climate in anthropology in the 1920s and afterwards would have reduced the probability of Mead's having engaged in such an unscientific inquiry and made it unlikely that her work would have been so widely accepted. But the acceptance of anthropologists like myself, who knew full well the requirements of ordinary scientific

practice, points to the power of ideology in our response to ideas. Unfortunately, I was not alone in this mistake. In a splendid modern book on biology and behavior that addresses the very heredity-environment issue at the core of Mead's work, the biological anthropologist Melvin Konner has this to say: "Mead was one of the greatest of all social scientists, and if she had become the first such scientist to win the Nobel Prize in Medicine and Physiology (she could have been cited for instance, for her contributions to pediatrics and psychiatry, as well as for her almost single-handed formulation of our present, flexible concept of human nature), the choice would have done credit to the Swedish Academy." (1982:107).

In view of Konner's demonstrated practice of thoughtful science, there can be no question that he knows how it ought to be practiced. To be fair to Konner, it must be said that he does not specifically attend to *CA*, and the work of hers that he directly considers is *Male and Female* (1949). Furthermore, he implicitly acknowledges her inattention to quantification and "exactitude"; writing in praise of the work of the anthropologists Beatrice and John Whiting, he says of the former: "She is one of the most quantitatively oriented of anthropologists, and may be said to have built an edifice of exactitude on the foundation that was laid by Margaret Mead" (1982:112). What Konner likes about Mead's work is her emphasis on a flexible concept of human nature, in the case of *Male and Female* the flexible nature of what were often called, rather ambiguously, sexual role models. Today the word "gender" would likely replace "sex" here, signifying the very flexibility that Mead championed. But *Male and Female* does draw on Mead's Samoan findings, and even if her later work in New Guinea was less subject to criticism, it seems a flimsy basis for deciding on a view of human nature, especially if it is a view one favors! A Nobel Prize in a field of science seems a bit extravagant for championing a cause without scientifically acceptable evidence. One can, however, learn a good deal about males and females, the flexibility of human nature, and the complexity of interaction between heredity

and environment from Konner's thoughtful work. I have cited his essentially unqualified praise of Mead only to warn us of the power of ideology to anesthetize our critical faculties.[2]

Sophisticated practitioners of the "natural" sciences rather than the "unnatural" ones might argue that ideological predilection could hardly diminish *their* capacity to judge things as they are. And so as a final example of the blinding power of such predilections I have a current example from the writings of the eminent Martin Gardiner, whose powers of analysis and clear explication many of us have long admired. Writing in the *Skeptical Inquirer* on what he calls "The Great Samoan Hoax," he uncritically accepts everything that Freeman has to say. He speaks of "irrefutable evidence . . . supporting the claim that young Mead was indeed the gullible victim of a playful hoax" (1983:131). As we have seen, it is not even irrefutable that Fa'apua'a and her friend even tried to hoax Mead, that such a hoax, if it did take place, fooled her, or that it would have mattered if it had. The question is not whether some young unmarried persons had sex but whether it was in some sense permissible. He claims that Mead thought Samoans' sex lives were unrestrained, but, as we have seen from analysis of the text, this is simply not true. In his zeal to make the hoax charge stick, Gardiner provides an account of what Fa'apua'a said that includes things she never said, judging by the published texts; he has her saying without quotes "yes, adolescents had complete sexual freedom, moving stress-free from childhood to adultery." As we have seen, this is not what Fa'apua'a claims in the text that Freeman has published. She claims only that she and her friend Fofoa joked about their own sexual exploits. Freeman maintains that she made similar claims about the adolescent girls but has published no text of these remarks or how they were elicited. No one has claimed that Fa'apua'a said anything about life being "stress-free from childhood to adultery." That would hardly be fit content for joking remarks! Furthermore, Gardiner distorts Mead's argument by claiming that she "was convinced that Samoan adolescents never suffered the anxi-

eties and torments of Western teenagers" because their sex lives were unrestrained. A greater degree of sexual acceptance and permissiveness was but one of the factors that Mead considered as contributing to a less stressful adolescence, and the word "unrestrained" is inconsistent with her understanding. Of course, Gardiner could not have known that there was not a single word in all of Mead's field materials indicating that Fa'apua'a was any kind of informant. Nor was he knowledgeable enough about Samoa to recognize that Mead could not possibly have believed Fa'apua'a's alleged account because she would then have had to maintain that the *tāupou* was not chaste.

As Gardiner makes clear, he is glad to see Mead undone partly because, as he has shown in previous works, she supported beliefs in "occult" phenomena such as the notion that "the earth is being observed by extraterrestrials in flying saucers" (1983:131). But, more important, he strongly supports Freeman's call for a more "interactionist" anthropology with greater attention to biological factors. Gardiner links this biological emphasis with establishing universal human needs and with a less relativistic perspective in evaluating cultures. Whatever merit there may be in these views of Gardiner, they have certainly dimmed his usual inquiring, skeptical perspective. Ideology can make dupes of us all.

Notes, Chapter 8, A Lesson to Us All

1. It is curious that this hypothetical example has a considerably lower rate of sexual experience than her actual sample and a much higher rate of experience prior to puberty.

2. Following the critique of Freeman, Konner notes in his *Why the Reckless Survive, and Other Secrets of Human Nature*, a sequel to *The Tangled Wing*, that Mead's "characterization of Samoan life hasn't weathered the decades very well" (1990:156–157).

9

Conclusions

Freeman contends that Mead depicts adolescent heterosexual experience as under little adult constraint and as commonplace. Clearly, there are lines enough in *CA* and other works of Mead to justify such a claim. However, a close reading of *CA* and examination of the field materials demonstrate that Mead was well aware of a number of limitations on the sexual life of Samoan adolescent girls. These acknowledged limitations were connected with age, rank, and residence in a pastor's home. Furthermore, there is considerable evidence in the field notes and even some in *CA* indicating specific restrictions on female adolescents.

As for the actual experience of heterosexual relations, Freeman has pointed out that slightly less than half of Mead's adolescent sample had had any such experience, and approximately the same figure may be derived from the field materials. Because the evidence regarding sexual experience for any particular person is so thin, it is difficult to judge its reliability. It was obtained in haphazard fashion from the subjects themselves and from other adolescents, children, adult females, and adult males, including *matais*. In a number of instances there is no indication in the field materials of how the information was obtained. Surely no great reliance should be placed on the accuracy of these findings. (Perhaps, as the