Letters

Margaret Mead and Cultural Anthropology

In the article "A controversy on Samoa comes of age" (News and Comment, 4 Mar., p. 1042), Eliot Marshall reviews questions that have been raised by the advance publicity given to Derek Freeman's book (*1*), in which Freeman severely criticizes the work of Margaret Mead in Samoa and, I gather, what he considers to be the theoretical stance of cultural anthropology in the United States. Marshall suggests that cultural anthropology in America now faces the necessity of having to confront its current standards and practices relating to ethnographic research and also its theoretical emphasis on the cultural determination of human behavior.

The impression is conveyed that standards of ethnography have not changed since the 1920's and that Margaret Mead's work is typical of how ethnographic research is conducted. Neither impression could be farther from the truth. In more than 40 years as a student and teacher of cultural anthropology, I cannot recall Mead's *Coming of Age in Samoa* or *Social Organization of Manu'a* ever being cited as models of how ethnographic research ought to be done. When she did her work in Samoa, moreover, the discipline was yet to feel the full impact of Bronislaw Malinowski's work in the Trobriand Islands, which was just beginning to be published. A decade later, his work had transformed anthropologists' conception of what good ethnographic research required: such things as learning the local language, participating as much as possible in local events, learning how the social system actually works in practice, and using semantic analysis as a major tool in getting at local concepts. By the 1940's good ethnography also included as standard procedure such additional things as taking censuses and collecting genealogies as basic reference data for the entire community under study, mapping landholdings, working out the seasonal round of activities, and counting just about everything one can count. The ethnographies we hold up as models for students today are exemplified by the work of the late E. E. Evans-Pritchard, Raymond Firth, Frederica DeLaguna, Elizabeth Colson, Douglas Oliver, Leopold Pospisil, Harold Conklin, Roger Keesing, Robert M. Netting, and Brent Berlin. These are all famous among anthropologists for the high quality of their ethnographic work, the kind and mass of data collected, and the analysis of ethnographic data. There are many younger anthropologists who are also doing outstanding ethnography. By its very nature, however, such work does not get read by nonscientists or by students in freshman-level social science courses.

In his article, Marshall reports that there is a "gentleman's understanding" in cultural anthropology "that one should not study a culture already being studied by a peer." There is no such gentleman's understanding. It is understood that one does not get in someone else's way physically while he or she is actually doing field research; but restudies, follow-up studies, and independent contemporary studies have always been regarded by anthropologists as essential to the conduct of the discipline. That Samoa was studied by at least several anthropologists before and after Margaret Mead was there is not peculiar in this regard. Almost every society that Margaret Mead studied has been subsequently studied by others as well. It is noteworthy, moreover, that the Human Relations Area Files, in selecting societies for its "blue-ribbon" sample for cross-cultural research, has made it a policy to give preference to societies for which there is good information from at least two independent sources.

It also misrepresents cultural anthropology to imply that as a discipline it has too little regard for the role of biology, as against culture, in explaining human behavior. Clearly, biology has everything to do with the fact that all human groups possess cultures and languages. Biology has very little to do, however, with explaining why people in China speak Chinese and eat with chopsticks while people in France speak French and eat with forks. Differences of this kind, which it is the task of the cultural anthropologists and linguists to describe, are within the range of possibilities for which our common biological heritage allows. Their explanation requires resort to history and ecology and to our knowledge of the conditions in which social and psychological processes, of which cultures and languages are artifacts, have been at work.

Whatever may be the popular repercussions of Derek Freeman's book, then, Margaret Mead does not and never has exemplified for anthropologists the highest standards of ethnographic field research. Neither do the views she has expressed in regard to the role of culture versus biology in human behavior accurately reflect where anthropology stands today. Her contributions to science lie elsewhere. She raised important questions about things both scientists and the lay public were taking for granted about human behavior, namely, our own customary views regarding differences in the behavior of immigrants of different ethnic background, regarding the inherent nature of men and women, and regarding the behavior of adolescents. She raised these questions at a time when experiments in conditioning behavior by psychologists and the findings of clinical psychology and psychoanalysis were demonstrating a great deal about the degree to which behavior, even genetically programmed behavior, is affected by or is a product of experience. It was a time when it was appropriate to call attention to the enormous role of custom and tradition in structuring experience. That her own empirical research in connection with these questions was of questionable quality and that at times she overstated her case are minor matters compared with the role she played in raising these questions and stimulating others to examine them. Some of the great figures in the history of science have been, themselves, poor field or laboratory researchers but have inspired the kinds of questions that have helped to move the research enterprise along.

Margaret Mead also had the capacity to stimulate the lay public to look at things differently and to accept the idea that common assumptions about human nature needed to be questioned. It is for this reason, not their scientific accuracy, that her books have been widely used in introductory social science courses. They turned students on.

The lay public is now learning what professional anthropologists have long known about the quality of her early

ethnographic fieldwork, done in her youth. It is inevitably an occasion for public excitement because, through the inspiring role she played, Margaret Mead had become a national institution by the end of her career. But there is no need, therefore, for scientists to conclude that there is a crisis in anthropology. The crisis is in the public's view of a public idol.

WARD GOODENOUGH
Department of Anthropology, University of Hawaii at Manoa, Honolulu 96822

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Deep-Sea Drilling

In Richard A. Kerr's Briefing of 15 April (News and Comment, p. 287) and Colin Norman's article "Accelerating research at Texas A & M" (22 Apr., p. 392), the proposed role of Texas A & M as science operator of the future Advanced Ocean Drilling Program (AODP) is noted. Although the interested community is working hard to make AODP a reality, we must emphasize that the final decision on the program rests with the NSF and Congress. Only after a formal proposal has passed peer review and been approved by the National Science Board, and only if fiscal year 1984 funds are appropriated by Congress, will Texas A & M actually be able to move ahead with the scientific program.

D. JAMES BAKER, JR.
G. ROSS HEATH
Joint Oceanographic Institutions, Inc., 2100 Pennsylvania Avenue, NW, Washington, D.C. 20037

Endosymbiosis and Autogeny

Roger Lewin's article (Research News, 4 Feb., p. 478) about gene transfer between the genomes of organelles and the nucleus is introduced in the framework of the endosymbiotic hypothesis for the origin of eukaryotic cells. The possibility that such transfers occur is probably good news for partisans of that hypothesis but is not a compelling argument for or against it. It is more logical to see in those transfers evidence for a similarity between the three genomes, similarity stressed by the possession of split genes, which is much more in line with the alternative autogenous hypothesis (*1*).

Furthermore, the cited work of Farrelly and Butow (*2*) demonstrating incorporation in a yeast nuclear chromosome of what is probably a "petite" mitochondrial genome emphasizes the plasmid character of mitochondrial DNA central to several autogenous models (*3*).

It seems that endosymbiosis is so unquestioned that the alternative autogenous theories are being mentioned less and less often. This would be acceptable if new evidence were leading us to disregard the autogenous theories. Nothing of the sort has yet happened, however, and those who specialize in this particular phylogenetic problem are still divided (*4*). As Dixon (*5*) recently wrote: "scientists themselves, whatever their attachment to stern objectivity, are swayed on occasion by fashions triggered by forces quite separate from normal scientific intercourse and its internal logic."

V. DEMOULIN
Department of Botany, University of Liège (Sart Tilman), B-4000 Liège, Belgium

References

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4. C. W. Burky, *Science* **215**, 495 (1982); A. B. Fulton, *Cell* **28**, 673 (1982).
5. B. Dixon, *Sciences (N.Y. Acad. Sci.)* **23**, 44 (1983).

Texas Telescope

I appreciate the spirit of Colin Norman's friendly comments (News and Comment, 22 Apr., p. 390) regarding the University of Texas (UT) McDonald Observatory plans to build a 300-inch telescope but would like to clarify some of his points. We are currently seeking (but do not yet have) about $17 million from the private sector in order to build the telescope (not just the primary mirror). We had indeed hoped for (although it now seems jeopardized) a $5-million special kickoff appropriation from the legislature. It would markedly speed up the project, but the project does not depend on this appropriation. We do hope the UT regents may soon approve the use of university construction funds for at least part of the project, but we do not have any formal commitment to that effect.

HARLAN J. SMITH
McDonald Observatory, University of Texas, Austin 78712-1083