LETTERS

Women in Anthropology

Eliot Marshall, in his article on the Freeman-Mead controversy about Samoan morals (News and Comment, 4 Mar., p. 1042), asserts that Mead "was a figure in the women's movement as well, an especially impressive one because she had risen in a discipline that was almost exclusively male." Mead's feminist stance is unquestioned, but I take issue with the statement about her position in anthropology.

Robert H. Lowie, a student of Boas and contemporary of Mead, noted that "Women have made important contributions independently of Boas, but probably nowhere have they achieved so much work as under the stimulation of the Columbia [University] atmosphere—witness the publications of Drs. Elsie Clews Parsons, Ruth Benedict, Ruth Bunzel, Gladys Reichard, Erna Gunther, Margaret Mead, Gene Weltfish, Ruth Underhill" (*1*). Of this list, Ruth Benedict was chosen by Abram Kardiner with nine men (including Darwin, Malinowski, and Freud) in his survey of students of man, society, and culture (*2*). And Columbia was not the only locus of women in anthropology. Two women at the University of California, Berkeley, earned Ph.D.'s in the subject within a few years of Mead's (1929): A. H. Gayton (1928), among the first half-dozen from that department, and Cora DuBois in 1932. In the same period, Erna Gunther became chairman of anthropology at the University of Washington and director of the Washington State Museum, posts held for a third of a century. She went on to chair the department at the University of Alaska, Fairbanks.

I agree that women in anthropology do not, and did not, account for half the professional force, but they are likely no more underrepresented than in the physical and biological sciences, mathematics, and the other behavioral sciences.

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References

1. R. H. Lowie, *The History of Ethnological Theory* (Farrar and Rinehart, New York, 1937), p. 134.
2. A. Kardiner and E. Preble, *They Studied Man* (World, New York, 1961).

A recent characterization (Book Reviews, 25 Feb., p. 957) of Elsie Clews Parsons as an "amateur" anthropologist could mislead those unaware she was president of the American Anthropological Association at her death in 1941, a former president of the American Ethnological Society and the American Folklore Society, a former vice president of the New York Academy of Sciences, author of two dozen volumes of anthropological studies, and according to her obituary (*1*) "an unusually productive and painstaking scholar." To be sure, her own resources rather than an academic salary or grants funded her research, which included at least two dozen field trips among the Pueblo Indians in the Southwest, Zapotec Indians in Mexico, Peguchi in Ecuador, Micmacs in Nova Scotia, Kiowa and Caddo Indians in Oklahoma, and blacks in the Sea Islands, Bahamas, and West Indies. But defining her as an "amateur" as one might an unpaid athlete or dilettante does an injustice to her memory and her discipline.

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1. F. Boas, *Science* **95**, 89 (1942).

Alcoholism Studies

Now that the Dickens Independent Review Committee in Toronto has affirmed the personal and professional integrity of Mark and Linda Sobell, with regard to their early data on controlled drinking in alcoholics (News and Comment, 19 Nov., p. 771), we sincerely hope that further research on this and other treatment approaches will be forthcoming from clinical research centers in attempts to improve treatment for this difficult problem. We also hope that research into all possible methods holding some promise as effective and humane treatments will be explored and that the freedom of inquiry will not be inhibited by this unfortunate controversy. In addition, we firmly believe, in accordance with the ethical codes of the American Psychological Association and the American Psychiatric Association, that controversies such as this are most productively argued in our scientific journals rather than the lay press.

In view of the sensitivity of this issue, the implicit attack on the investigators' integrity, and the resulting storm of controversy following publication of the report by Mary L. Pendery *et al.* (9 July, p. 169), we express concern about *Science*'s editorial decision to publish a reinterpretation of an original data set, or even some new data that contradict the original data set, *without simultaneous comment from the investigators concerned*. We realize that this situation was an unusual one that was complicated by possible legal actions. However, as editors of clinical-research–oriented journals*, we believe that the course of action followed, particularly when there are strong disagreements for whatever reasons, can give a very biased, one-sided picture of this issue which makes objective evaluation difficult. By following this course of action, science has not been advanced.

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In the absence of criticism of experiments and replication of results, the integrity of science would be destroyed. The overwhelming majority of scientists understand this, and most cooperate with those who challenge the validity of their work. The behavior of the Sobells with respect to the research report by

Pendery *et al.*, was unprecedented in my experience of more than 20 years as editor.

The Sobells, in writing, threatened us with legal action while we were in the initial phase of considering the paper. Shortly after, we received a letter from their attorney. Under such circumstances, prudence dictates that contact between the principals cease and that one deal with the matter through attorneys.

The report that we published in our 9 July issue was very carefully edited. It was extensively reviewed, including evaluation by an expert statistician. Painstaking efforts were made to ensure an absence of comment about the integrity of the Sobells. We required that assertions made about patients' histories be documented by court records, police records, hospital records, or affidavits. The final draft was checked repeatedly, sentence by sentence, to ensure that supporting evidence was available. In crucial instances, two or more independent documents corroborated statements made.

For years the Sobell paper of 1972 went virtually unchallenged. Their work received a large play in the media. Attempts by Mary Pendery to examine the basic data and to follow up on patients' subsequent histories were impeded by repeated legal action by the Sobells. The avenue of a technical comment has been and remains open to the Sobells. They have not so far availed themselves of it.—PHILIP H. ABELSON

Millisecond Pulsar

In M. Mitchell Waldrop's excellent article about the Millisecond Pulsar (Research News, 18 Feb., p. 831), there are two minor errors. First, the spectrum of 4C21.53 *falls* rapidly with frequency, as does the spectrum of pulsars. Second, while the ratio of period (P) to period derivative ($\dot{P}$) gives a time scale of billions of years, I do not think that its age is much greater than 10^6 years. The pulsar is very near the galactic plane. Since most pulsars move at 100 kilometers per second, this indicates an age near 10^6 years. Also, the original period was probably not much less than $P_o \sim 1.5$ milliseconds; if so, the age is not $P/\dot{P}$, but $P/2\dot{P} \times (1 - (P_o/P)^2)$.

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Nuclear Power in Space

While we do not dispute any statements of fact in William J. Broad's short article "Fallout from nuclear power in space" (News and Comment, 7 Jan., p. 38), we believe that an unnecessarily frightening impression may have been received by *Science* readers. For example, Broad's conclusion that "The contamination was not unprecedented but it was quite large"—referring to the plutonium-238 from the reentered and burned SNAP-9A power supply—does not follow from data presented in table 3 of the paper to which he refers (*1*). The global plutonium deposited by 1970 was made up of ^{239}Pu and ^{240}Pu (325 ± 36 kilocuries), ^{238}Pu from weapons (7.7 ± 0.9 kilocuries), and ^{238}Pu from SNAP-9A (13.9 ± 2.2 kilocuries). The other two alpha-emitting isotopes (masses 236 and 242) were virtually too low in concentration to be measured.

Indeed, the total ^{238}Pu on the ground before the SNAP-9A incident was a little more than 2 percent of the total plutonium, and the ". . . threefold increase of plutonium-238 contamination . . ." mentioned at the end of Broad's fourth paragraph increased the fraction to 4 percent. In this context it does not seem reasonable to refer to the additional plutonium as "large," nor does questioning the possible health effects of the accident appear practical in light of the small overall risk attributed to the total environmental ^{238}Pu (*2*).

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1. E. P. Hardy, P. W. Krey, H. L. Volchok, *Nature (London)* **241**, 444 (1973).
2. United Nations Scientific Committee on the Effects of Atomic Radiation, *Ionizing Radiation: Sources and Biological Effects. UNSCEAR 1982 Report to the General Assembly* (United Nations, New York, 1982).

Erratum: In the report "Eruption of El Chichón volcano, Chiapas, Mexico, 28 March to 7 April 1982" by J. M. Hoffer *et al.* (24 Dec., p. 1307), the millimeter readings in figure 2 (p. 1308) were in error by a magnitude of one; the 100, 200, 300, 400, and 500 mm contours should have been 10, 20, 30, 40, and 50 mm.

Erratum: The report "Topography, albedo-temperature feedback, and climate sensitivity" by G. E. Birchfield and J. Wertman (21 Jan., p. 284) should have included the following acknowledgment as note 11: "This work was partially supported by grant 8111138 from the Climate Dynamics Section of the National Science Foundation."