LETTERS

Potassium Iodide Distribution

The News and Comment briefing "Potassium iodide and nuclear accidents" (19 Mar., p. 1485) by Constance Holden misrepresents my congressional testimony. At the hearing, I made it absolutely clear that I was quoting from a report issued by the American Thyroid Association on 18 September 1981. This organization represents the leading experts in the field of thyroid disease. Their report said that "evidence from subjects exposed to relatively large amounts of diagnostic [iodine-131] in Sweden and carefully followed suggested no increase in thyroid tumor incidence in populations exposed to about 100 rads (adults) or 159 rads (persons under 20 years of age). For these reasons, projected thyroidal doses from radioiodine as high as 500 rads have recently been proposed as a realistic threshold for the institution of blocking counter-measures in the event of a reactor accident releasing radioiodines into the environment. . . . The projected absorbed dose of 10–30 rads recommended by NCRP [National Council on Radiation Protection and Measurements] 55 as the threshold for the institution of iodine blockade in the event of a reactor accident is overly conservative. Based upon available data, it would seem unlikely that clinically significant thyroid disease would result from individual thyroid exposure of less than 100 rads." Frank von Hippel and Sydney Wolfe challenged this position and stated that I had not published in this field. Their challenge is irrelevant since I was simply quoting from the experts. I was not reporting from my experience, as the briefing states.

The briefing also states: "If the lineup at the hearing is any indication, it would appear that the main opponents of general distribution of KI are also the strongest nuclear power enthusiasts." The American Thyroid Association's report concluded that data are not now available to define more precisely the relative risks of radioiodine exposure and of short-term iodide therapy and recommended the appointment of a national task force of appropriate specialists to consider the problem. One can hardly characterize the members of this association as "the strongest nuclear power enthusiasts." It simply consists of the most knowledgeable thyroidologists in the country.

Representative Edward Markey (D–Mass.), who called the hearing and who was the only member of the Committee on Interior and Insular Affairs present, favors distribution of KI, and all but one of those invited to testify reflected this point of view. Holden's statement that " . . . the preponderant scientific opinion—judging from recent hearings in the House—is that KI should be made available to people who live near reactors" leaves the impression that this reflects preponderant scientific opinion rather than the viewpoint of a single congressman and the witnesses he called who reinforce that viewpoint.

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Models of Human Evolution

C. Owen Lovejoy's article, "The origin of man" (23 Jan. 1981, p. 341), offers a series of interesting suggestions regarding possible sequences of change in human evolution and the natural selection pressures which may have brought them about. However, a general scientific readership that is not acquainted with details of the state of inquiry in the study of human evolution should be aware that a number of the assertions incorporated in Lovejoy's argument are in fact uncertain, and several are currently under investigation.

1) In a discussion of human evolution one should be concerned with demographic patterns without agriculture. Although reliable data on human birth spacing are scarce, those cases that have been reported are inconsistent with Lovejoy's estimate of 2.5 years and imply values for spacing that are as high or higher than those estimated for apes (*1*).

2) It is not clear that, before the spread of agriculture and firearms, the great apes were precariously poised on the brink of extinction (*2*). Presumably the ultra-*K* selective reproductive strategy of apes is derived, rather than primitive, and it would be important to assess the adaptive qualities of the ape strategy.

3) There are serious difficulties with the suggestion that among incipient hominid populations some nonbipedal males improved their reproductive success by gathering nonmeat foods in order to provision their mates and offspring. The transport of sufficient nuts, berries, and insects poses problems even if one assumes a simple bark tray was used as a carrying device. The difficulties seem particularly acute if, as implied, the incipient hominids were not yet bipedal. The feasibility, energy potentials, and energy costs of such provisioning in savanna environments can and should be measured.

4) Lovejoy is dealing mainly with evolutionary events that occurred just before 4 million years ago. He specifically discounts the possibility that meat was involved in hominid feeding systems of those times. While this may be correct, readers should be aware that a shift into an adaptive pattern involving food transport and provisioning would be greatly facilitated if highly portable, high-quality food such as meat were a component of diet. I know of no a priori reason why meat, whether secured by hunting or scavenging, could not have been important and think that Lovejoy's model would have been strengthened by having had this possibility left open for further investigation.

It would certainly have been relevant to have drawn readers' attention to the fact that, by 2 million years ago at the next stage of human evolution, there is strong archeological evidence for hominid consumption of meat from the carcasses of much larger animals than are eaten by any living ape. Equally, there is archeological evidence from the same time range (which many researchers would regard as consistent with Lovejoy's central hypothesis) that food began to be transported at an early stage in evolution (*3*). While this archeological evidence relates to the time range following that which is the focus of concern, it is germane to an assessment of Lovejoy's arguments and is compatible with it.

Archeological indications from 2 million years ago are fully consistent with the most important part of Lovejoy's overall scenario—namely, his suggestion that pair-bonding and provisioning were shifts that occurred early on in human evolution. The crucial next step is to separate out testable components and implications from Lovejoy's scenario and to test them.

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References

1. N. Howell, in *The Demographic Evolution of Human Populations*, R. H. Ward and K. M. Weiss, Eds. (Academic Press, New York, 1976), pp. 25–40; *Demography of the Dobe !Kung* (Academic Press, New York, 1979); J. V. Neel and K. M. Weiss, *Am. J. Phys. Anthropol.* **42**, 25 (1975).