LETTERS

NSF Appointments

Colin Norman's article about management changes at the National Science Foundation (NSF) (News and Comment, 24 Dec., p. 1286) raises questions about the role of the National Science Board (NSB) in the selection of NSF deputy and assistant directors.

The enabling statute explicitly provides that the President appoints the NSF management team. The NSB is given the opportunity to propose candidates to the President. Once appointed and confirmed by the Senate, the deputy and the presidentially appointed assistant directors serve at the pleasure of the President, and by delegation at the pleasure of the director. Thus the director has a duty to the President to supervise the management team and do his or her best to strengthen it.

The NSB is responsible for policy, new program approval, and approval of major grants; the director is responsible for administration. The director and the Board share a responsibility to ensure the highest level of scientific competence, personal integrity, and managerial ability in the individuals recommended to the President for appointment to these positions.

It is in this spirit that the director and the Board are working together to find the most capable leadership the NSF can attract. Ed Knapp is a member of the Board's search committee, which will look for the most capable people. As has been true in the past, the Board's search process is made without inquiry concerning political affiliation. We would expect the final list of recommendations for each position to be forwarded to the President over the signatures of both the Board chairman and the director.

The division of authority between the Board and the director is the result of a compromise between the Congress and President Truman when the NSF was created more than 30 years ago. The original proposal, vetoed by Truman, was a presidentially appointed Board which had full power to select (and remove) the management team. Truman's veto reflected his conviction—shared by other Presidents—that an agency dispensing public funds must be run by an administration accountable to the President. But the President agreed that, in the interest of scientific objectivity, excellent technical judgment, and a focus on the long-term goal of national scientific leadership, the agency should have an elbow's length, if not arm's length, insulation from day-to-day political pressures.

Thus the enabling act provides for the Board's role in the search for new management and the director's role in supervising it and making changes when in his judgment changes are needed. The scientific community can best assure the effectiveness of this arrangement by suggesting well-qualified people to serve, and most especially by being willing to accept the call of public service when asked.

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Extraterrestrial Intelligence: A Skeptical View of Radio Searches

In "Extraterrestrial intelligence: An international petition" (Letters, 29 Oct., p. 426) there appear two assertions that I question. The first is that "The radio search . . . assumes nothing about other civilizations that has not transpired in ours." The second states, "The results [of the proposed radio search]—whether positive or negative—would have profound implications for our view of our universe and ourselves."

If the radio search indeed assumed nothing about other civilizations that has not transpired in ours, it would have to assume that no extraterrestrial civilization intentionally broadcasts radio messages to other civilizations for any significant length of time, because we have not done so. On the basis of this assumption, a search must be conducted for radio leakage from transmitters comparable to ours, not for intentional messages. Furthermore, the leakage sought would be from those transmitters which were sufficiently numerous that their output could be considered omnidirectional. The probability of receiving an unintentional signal from a single powerful unidirectional transmitter is very small.

The 10-year radio search program, as recently described by Wolfe *et al.* (*1*), is not designed to look for such leakage. According to Sullivan *et al.* (*2*) [see also (*1*)], the most powerful, effectively omnidirectional sources of radio leakage from Earth are ultrahigh frequency (UHF) television stations and BMEWS-type defensive radar. Sullivan *et al.* (*2*, p. 384) assert the Arecibo antenna could detect the former out to a distance of 1.8 light-years and the latter out to about 18 light-years. [Similar numbers appear in (*1*).] Thus the proposed experiment could not detect television emission from any civilization around any star, and radar from only a very few nearby stars. Furthermore, both UHF and radar transmission are frequencies less than 1 gigahertz (GHz) (*2*), while the proposed search will concentrate on frequencies between 1 and 10 GHz (*1*, p. 399). An Arecibo transmitter could indeed be detected by an Arecibo receiver over a distance "of many thousands of light years," but this is possible only because such a transmitter is essentially unidirectional, so that a high probability of reception would require intentional beaming, which is ruled out by assumption. Hence the proposed radio search could detect extraterrestrial civilizations (which, by assumption, have radio equipment like ours but no better) only if they were extraordinarily close, and then only if the search frequency were changed. Thus the proposed search is really based, not on the stated assumption, but on the assumption that extraterrestrial civilizations would have been intentionally broadcasting signals for primitive civilizations to receive for centuries or longer at average power levels far above the total radio signal output of Earth. Such civilizations would necessarily be very much in advance of ours, and assuming their existence would depend "on a major extrapolation from the circumstances on Earth, here and now," an extrapolation which the extraterrestrial intelligence petition explicitly claims the radio search does not make. In fact, any search for extraterrestrial civilizations conducted today at reasonable expense must assume that the searched-for civilizations are very much in advance of our civilization. I have argued (*3*) that the space probes of such civilizations would have long ago reached our solar system. Since they are not here, they do not exist.

Because the proposed radio search cannot conclusively rule out the presence of civilizations comparable to ours around a significant number of stars, a negative result is most unlikely to have "profound implications for our view of our universe. . . ." Even if the radio search were drastically upgraded to a capability of detecting Earth-level radio leakage from anywhere in the galaxy, a negative result would probably not convince supporters of the Search for Extraterrestrial Intelligence (SETI) that extraterrestrial civilizations do not exist. They could simply argue, in the face of a negative result, that advanced extraterrestrial civilizations generally abandon inefficient radio transmitters like ours after a short time. In fact, Sagan has already argued that such abandonment is likely (*4*). If scientific theories are char-

acterized by their falsifiability, then the proposed radio search is not a scientific experiment at all because it cannot falsify the hypothesis being tested, namely that extraterrestrial civilizations exist. SETI will become a science only when its proponents tell us what observations will convince them that it is reasonable to assume we are alone.

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References

1. J. H. Wolfe, R. E. Edelson, J. Billingham, R. B. Crow, S. Gulkis, E. T. Olson, B. M. Oliver, A. M. Peterson, C. L. Seeger, J. C. Tarter, in *Life in the Universe*, J. Billingham, Ed. (MIT Press, Cambridge, Mass., 1981).
2. W. T. Sullivan III, S. Brown, C. Wetherill, *Science* **199**, 377 (1978).
3. F. J. Tipler, *Q. J. R. Astron. Soc.* **21**, 267 (1980); *ibid.*, **22**, 133 and 279 (1981); *Mercury* (January-February 1982), p. 5; *Phys. Today* **35**, 26 (1982); *New Sci.* **96**, 33 (1982).
4. C. Sagan, *Science* **202**, 374 (1978).

Calcium Intake and Hypertension

On the basis of our experience with dietary histories and food-frequency reports, particularly with respect to dietary calcium (*1, 2*), we wish to raise some objections to the report by McCarron *et al.* (16 July, p. 267) suggesting a lower calcium intake as a factor in hypertension.

We are concerned about the small sample size (< 50 each) and the apparent mixture of males and females and blacks and whites. Although males have a higher caloric intake, the male consumption of milk products is often less (*3*). Blacks consume less milk and other dairy products, not necessarily because of lactose intolerance. Moreover, black women tend to be fatter than white women, a function of their lower socioeconomic status. We are also concerned about the extent to which the 46 hypertensives had engaged in dietary restriction, as a result of medical advice or on their own initiative, since a sodium-restricted or calorie-restricted diet generally limits the intake of fluid milk and cheese.

But calcium intake is notoriously difficult to ascertain in short-term dietary data, and when extradietary sources of calcium are not considered, as in drinking water, beer, and antacids (*1*). Calcium intakes are also correlated with caloric intakes, and a lower (reported) caloric intake on the part of the hypertensives could also explain the apparent difference. Even under optimum conditions short-term food-intake data may provide inaccurate indications of individual intakes, even for the same week and the same season (*4*).

A final problem arises from the fact that the calcium intakes reported for both normotensives and hypertensives fall well above values for most of the world, except where yellow maize is the staple. Yet the very countries where calcium intakes are lowest tend to be countries where blood pressures are also low and where hypertension is uncommon.

Under these circumstances we find it difficult to accept the proposition that an apparent lowering of calcium intake by 200 milligrams per day is causally related to hypertension.

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References

1. S. M. Garn, *The Earlier Gain and Later Loss of Cortical Bone* (Thomas, Springfield, Ill., 1970).
2. _____ and M. A. Solomon, *Ecol. Food Nutr.* **10**, 195 (1981).
3. R. L. Rizek and E. M. Jackson, *Current Food Consumption Practices and Nutrient Sources in the American Diet* (Consumer Nutrition Center–Human Nutrition, Science and Education Administration, Department of Agriculture, Hyattsville, Md., 1980).
4. S. M. Garn, F. A. Larkin, P. E. Cole, *Ecol. Food Nutr.* **5**, 245 (1976); *Am. J. Clin. Nutr.* **31**, 1114 (1978).

Many of the expressed concerns of Garn and Larkin were addressed in our report. Our study compared a population of normotensives and hypertensives and, as defined in the report, our subjects were groups matched for age, sex, and race; this, in itself, would control for the effects of these variables. As we noted, average weight was not different for the two groups. In contrast to the stated concerns of Garn and Larkin, both of our populations had fewer males and only several black subjects in each group. As mentioned in our report, our subjects did not know previously that they were hypertensive and were excluded if they gave a history of diet modification. Since both groups came from the same close geographical region, extradietary sources of calcium, such as water, would not have explained the overall differences. As shown in figure 1 of our report, caloric intake was virtually identical for the two study populations and could not, therefore, explain the dietary differences.

We concur that dietary recalls are inaccurate for estimating lifetime individual intakes of nutrients but, as noted in our reference 14, recalls are valid for cross-sectional population studies such as ours. We agree that the hypertensive's Ca^{2+} intake is well above that in many countries where hypertension may be less prevalent. However, Garn and Larkin do not acknowledge that dietary Ca^{2+} requirements vary depending on level of physical activity, protein intake, sodium balance, alcohol exposure, and phosphate intake (*1*), all of which make the obligate Ca^{2+} intake required to stay in balance higher in more developed societies, where hypertension is more prevalent.

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Reference

1. L. H. Allen, *Am. J. Clin. Nutr.* **35**, 783 (1982).

Heptachlor Exposure in Hawaii

R. Jeffrey Smith's article about heptachlor exposure (News and Comment, 9 July, p. 137) points out the concern of potential carcinogenic effect in the infants of Hawaii. Also of concern is the potential teratogenic effect for the 15,000 infants known to have been exposed prenatally. Although no human studies have been done, researchers have shown that organochlorine pesticides are a subtle teratogen in mice, resulting in behavioral differences in the animals exposed (*1*).

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References

1. G. M. Al-Hachim, *Psychopharmacologia* **21**, 370 (1971); _____ and A. Al-Baker, *Br. J. Pharmacol.* **49**, 311 (1973); G. M. Al-Hachim and G. B. Fink, *Psychopharmacologia* **12**, 424 (1968).

Erratum: In the article "The 1982 Nobel Prize in Physiology or Medicine" (19 Nov., p. 765), the last full sentence in column 2 on page 765 should have read, "In the 1930's, Raphael Kurzrok and Charles Leib at Columbia University discovered that human seminal plasma contracted uterine smooth muscle." In the first sentence of the last paragraph in column 1 on page 768, "Calloway" should have been "Kellaway."