the earth "revolves once in 24 hours" is particularly vulnerable, partly because it is strictly untrue, but chiefly because one cannot possibly discuss the degree of accuracy of the statement or the reliability of the evidence without a detailed and explicitly statistical analysis.

3) One can, of course do a great deal of very useful and quantitative science without utilizing the procedures of the professional statistician, and certainly without bothering to remember that all evidence is (in the sense stated above) statistical. I had no slightest intention of implying otherwise.

4) Hooke states that the phrase *statistical evidence* is applied to data gathered by observation of uncontrolled events, and also to data gathered from planned experiments. Since these two together include, as far as I can see, all data, Hooke would appear to agree with me that all evidence is statistical evidence. But Hooke has a low opinion of type *a* evidence, this naturally resulting from the fact that by his type *a* he really means (as revealed by his next-to-last paragraph) badly argued conclusions from poorly observed data. Darwin used type *a* evidence and revolutionized man's thinking in the process.

5) I do not advocate turning all of science over to the statisticians, nor do I think that a small boy, when he is counting his marbles, need be reminded that the counting of electrons is a very queer and slippery business. But I do object to the snide implication that evidence which is "merely statistical" is, by virtue of that fact, silly and unreliable.

WARREN WEAVER
Alfred P. Sloan Foundation, New York, New York

Nature and Nurture

Questions about the effect of environment on intellectual potential [G. Allen, *Science* **133**, 378 (1961)] should be considered in light of Spitz's studies on "hospitalism" [R. A. Spitz, in *Psychoanalytic Study of the Child* (International Universities Press, New York, 1946), vol. 1, pp. 53–74]. Controlled studies of institutionalized infants showed a drop in developmental quotient from 124 to 72 in institution X during a given period; in institution Y there was no change, and infants at home in comparable socioeconomic areas showed no change. The significant variable was the presence of one mother or mother substitute for each infant in institution Y and of one mother-substitute for each eight infants in institution X.

Follow-up studies 2 years later

[R. A. Spitz, *ibid.* (1947), vol. 2, pp. 113–117] showed profound emotional and intellectual crippling among the surviving infants from the original study in institution X. In addition, in height and weight they were considerably below the expected levels for their age. I urge those interested in this subject to read Spitz's carefully documented and lucid report.

PETER D. KING
Reiss-Davis Clinic for Child Guidance, Los Angeles, California

Fallout

In their article "Atmospheric transport of artificial radioactivity," Martell and Drevinsky (*1*) undertake to demonstrate that the yield, for a given size of stratospheric source, from the Russian weapon test at about latitude 52°N in the autumn of 1955 was greater than that from the American equatorial tests (at 11°N), Castle in 1954 and Redwing in 1956, by factors of 60 and 10, respectively. Martell and Drevinsky calculated these factors from Sr^{90} and Sr^{89} data (*2*) from the rain-collecting station at Milford Haven, Wales, and from estimates of the stratospheric sources by Libby (*3*). We offer here an alternative interpretation of the same data and suggest that the relative yield of the high-latitude test has been overestimated.

Peirson *et al.* (*4*) calculated that not more than 13 percent of the Sr^{90} collected in rain at Milford Haven during the spring of 1956 was due to the Russian tests of 1955. The effect of the Russian tests was seen against a background of stratospheric debris from the 1954 Castle series. This percentage is derived from the values for the ratio Sr^{89}/Sr^{90} after correction for the radioactive decay to 22 November 1955, the date of the only high-yield test of this Russian series (*5*).

The amount of radioactivity injected into the stratosphere by these weapon tests was estimated by Libby (*3*) to be 20 "megatons of fission" for Castle and 1.8 megatons for the Russian 1955 series. Then, on the basis of Martell and Drevinsky's parameter (micromicrocuries of Sr^{90} per liter per megaton), the ratio of yields during the first half of 1956 is

$$\frac{\text{Russian 1955}}{\text{Castle 1954}} = \frac{13}{87} \times \frac{20}{1.8} = 1.7$$

The estimate of 13 percent for the Russian contribution is a subjective estimate of the upper limit, since a significant proportion of the *new* debris during this period could well have been of tropospheric origin. If, however, all the new debris is attributed