[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.

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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 (micro-microcuries 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 to the Russian 1955 test, then the proportion would be raised to 22 percent, and the ratio of yields, to 3.2. On the basis of this interpretation, Martell and Drevinsky's estimate is 20 to 30 times too high.

Where our approach differs from the foregoing interpretation is in the choice of (i) the appropriate reference date for the only reported high-yield explosion for the Russian test series of the autumn of 1955 and (ii) a period in which the Russian debris can be compared with contemporary Castle debris. A comparison in the same period avoids the error of the foregoing interpretation, caused by discounting the effect of the seasonal variation in Sr^{90} activity.

The relative yield from the Redwing 1956 test series may be derived in a similar manner. It has been estimated (*4*) that 78 percent of the Sr^{90} collected at Milford Haven during the autumn of 1956 could be attributed to Redwing. (For simplicity of analysis, the Redwing debris is considered in relation to a background consisting essentially of Castle debris, and the fraction due to the Russian 1955 tests is ignored.) On the basis of Libby's estimates (*3*) of the stratospheric source strengths, the ratio of yields during the autumn of 1956 is found to be

$$\frac{\text{Redwing 1956}}{\text{Castle 1954}} \approx \frac{78}{22} \times \frac{20}{6.7} \approx 11$$

This ratio is about twice that calculated by Martell and Drevinsky, who in this case have overestimated the contribution of Sr^{90} from Castle by selecting an inappropriate period of measurement.

The relevance of this type of calculation can be no greater than that of the stratospheric injection data. As Martell and Drevinsky suggest, the stratospheric component from these weapon tests is uncertain. Also, it is improbable that the selected data, from a single measuring station, would provide a truly comprehensive index of comparison for the relative global yields of these weapon tests.

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It is surprising that two interpretations of the same aspect of fallout can vary so widely. The Peirson and Stewart analysis gives a relative rate of stratospheric fallout from the 1955 Soviet tests and the 1954 Castle tests which differs from our result by a factor of 20 to 30. Part of the difference is due to a change in the time basis of comparison. The physical consequences of the Peirson-Stewart interpretation (discussed below) suggest that these authors have seriously underestimated the contribution of the 1955 Soviet tests to fallout during the first half of 1956. In every respect in which the assumptions and method of Peirson and Stewart differ from our own, they tend to reduce the contribution of the 1955 Soviet tests relative to that of the Castle tests. Since our interpretation of the Milford Haven rainfall data has been presented elsewhere (*1, 2*), we address our attention to the several points of disagreement.

In our analysis of the Milford Haven data (*2*, Fig. 2), we compared the relative intensities of stratospheric fallout for the Castle, Redwing, and 1955 Soviet tests at corresponding early times after test injection. Since we have concluded from our Ba^{140}/Sr^{90} data (*2*) that most short-lived fission products in world-wide fallout are of stratospheric origin, the initial large differences in fallout rate acquire special significance. Furthermore, during these early periods the contribution of each test source can be assessed unequivocally from Sr^{89}/Sr^{90} data within the stated uncertainties of production ratio and production date. By contrast, Peirson and Stewart attempt to resolve the concurrent contributions of the 1955 Soviet and Castle tests during the first half of 1956 and of Castle and Redwing during the autumn of 1956, thus employing not only a different time scale of comparison but a far more subjective procedure. Any uncertainty in the estimation of one component affects the other component in the opposite sense, magnifying the uncertainty in the ratio. Peirson and Stewart's assignment of all unidentified Sr^{90} to Castle is a dubious procedure, particularly for the autumn of 1956, a period for which residual stratospheric debris from the 1955 Soviet tests cannot be ruled out.

The Peirson and Stewart analysis results in assignment to the Castle tests of 87 percent of the Sr^{90} fallout in the first half of 1956 and 22 percent of that in the autumn of 1956. Applying these percentages to the Milford Haven rainfall data (*3*) gives values for Castle components of 5.6 $\mu\mu$c/lit. for the period 14 February to 9 July 1956 and of only 0.52 $\mu\mu c$/lit. for the period 2 August to 1 December 1956. Seasonal variation of this magnitude, with levels varying by a factor of nearly 11 during 1956, is a surprising result for Castle debris, which had been injected into the high equatorial stratosphere more than 2 years earlier. This remarkable variation is all the greater when only the spring peak and the fall minimum periods are compared, and also when the Castle component in rainfall in the autumn of 1956 is corrected for residual debris from the 1955 Soviet tests. By contrast, Stewart (*3*) has shown that levels of Sr^{90} in rains at Ohakea (latitude 40°12′S) varied seasonally by less than a factor of 2 during 1956, when Castle was unquestionably the only significant source of fallout in the Southern Hemisphere. Observed seasonal variations in ozone levels in tropospheric air in the Northern Hemisphere are similarly small. We suggest that the remarkable seasonal variation in Sr^{90} fallout from Castle which results from the Peirson-Stewart analysis is not real and is due to underestimation of the contribution of the 1955 Soviet tests during the first half of 1956.

Peirson's method (*4*) of estimating the Soviet-test component in fallout of Sr^{90} during the first half of 1956 differs from our own in two important respects, each of which results in an underestimation on Peirson's part, or an overestimation on ours, of the Soviet test contribution. First, Peirson arbitrarily assigns a substantial fraction of the Sr^{89}, and thus some of the Sr^{90}, to tropospheric sources. On the basis of the Ba^{140}/Sr^{90} data for New England rains (*2*) we have concluded that substantially all Sr^{89} in world-wide fallout is stratospheric in origin. The Milford Haven Sr^{89}/Sr^{90} data for the first half of 1956 are consistent with assignment of all Sr^{89} to the 1955 Soviet tests. Second, Peirson takes the high-yield 23 November shot alone as the source of stratospheric fallout from the 1955 Soviet tests. Other shots in that series took place on 4 August, 24 September, and 10 November. Although the yield and the cloud heights for these events have not been made public, they cannot be excluded from consideration. Surface shots of 100 kilotons and air shots of even lower yield would inject debris into the lower stratosphere at latitudes of Soviet testing (*5*). In our own analysis we assume stratospheric origin of the Sr^{89} and a Sr^{89}/Sr^{90} activity-production ratio of 170. For the Milford Haven data for the first half of 1956, this leads to an assignment to the stratosphere of 25 percent of the Sr^{90} fallout for the 23 November 1955 shot alone, or nearly 100 percent of the fallout for the 4 August 1955 shot alone. We take the mid-point of the 1955 Soviet test series as the production date for which it can be assumed, with a factor of uncertainty of 2, that 50 percent of the debris was of 1955 Soviet test origin.

On this basis, we suggest that Peirson and Stewart have underestimated the 1955 Soviet contribution by a factor of between 2 and 8, the Soviet-Castle ratio being thus affected by a much larger factor. The physical consequences of their interpretation indicate that the higher factors must apply. The remaining difference can be explained on the basis of differences in rate of deposition of Sr^{90} fallout from Castle for the two quite different periods considered.

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References and Notes

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5. For example, see W. W. Kellogg, *U.S. Atomic Energy Comm. Rept. No. AECU 3403* (14 June 1956).

On Reading Original Papers

The light-hearted editorial, "Electricity and personal magnetism," in your issue of 3 March [*Science* **133**, 611 (1961)] makes amusing reading, but it exhibits the lack of understanding that is at the root of C. P. Snow's "Two cultures." While I cannot claim to have read all 2.5 million words of the "Great Books of the Western World," or even the 642 pages of Faraday's *Experimental Researches in Electricity* (and am in no way connected with the publishers or endorsers), I am sure that your editorial view of what constitutes good reading about science is an extremely limited one.

As I understand it, the writer of the editorial proposes that a reader be told what parts of a scientific work are "really great," what terms are to be considered "right," and where a scientist of the caliber of Galileo, Newton, Faraday, or Darwin was "wrong." Apparently he feels that it is a waste of time to "make one's way" through lengthy, outdated material in the classic works of science when the confirmed results can be condensed to half a page