the case detected this year is almost sure to become next year's fatality.

Finally, the HEW document is severely criticized for (allegedly) overestimating the magnitude of the population at risk. The use of figures derived from the National Occupational Hazard Survey of 1974 is particularly condemned on the basis that the survey data reflected potential exposures at any level rather than actual exposures at levels sufficiently elevated to increase risk. Clearly population-at-risk figures are among the most difficult to ascertain in making the projections under discussion: in some cases populations at risk could be overestimated, in other cases underestimated. However, the error associated with this potential overestimate may not be so significant as industrial critics suggest because turnovers among the cohort of workers who will contract the occupational cancers of the next two decades have not been taken into account. Furthermore, the HEW projections did not include any estimates of the industry-related tumors likely to occur among the many occupational groups in which excess cancer incidence has been reported but for which a specific etiologic agent has not yet been identified. Nor did they include any projections of excess risk from exposures to chemicals, such as epichlorohydrin, shown to be carcinogenic in laboratory animals but not yet studied in human populations. For these reasons, any present estimate of future occupational cancer risk could be conservative.

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Hypothesis Testing

The authors of the important article "Cognitive development and social policy" (23 June 1978, p. 1357) quote, as have so many others, a conclusion of Record, McKeowan, and Edwards (*1*) pertaining to the intelligence of twins. The latter authors erred, however, in concluding *from their data* that the intellectual deficit of twins is due to a deficit in the postnatal environment. Since the conclusion in question represents a common misuse and misinterpretation of hypothesis testing in statistics, setting the matter straight is of wider interest than as it pertains to factors in cognitive development. The mean level of intelligence of the surviving member of a twin pair when the other member died very early in development turned out to be about halfway between two expected values: the mean for single-birth individuals and the lower mean for normal twins. Record *et al.* compared the mean in their data with the single-birth mean and were unable to reject the null hypothesis. Thereupon they accepted that hypothesis. If they had compared the mean of their experimental group with the expected value for normal twins, they would also have been unable to reject the new null hypothesis. Obviously both null hypotheses cannot be correct. The solution is also obvious: withhold judgment until more data can be gathered.

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We thank Humphreys for his attention to our article on cognitive performance in Warsaw schoolchildren. He makes a point entirely peripheral to the results of our study in Warsaw. However, the result of Record *et al.* (*1*) that he chooses to criticize is an important issue of substance because it sustains the view that family microenvironment is a determinant of IQ. We therefore reviewed their original article.

First, Humphreys is mistaken when he states that the IQ of single survivors of twin pairs is "about halfway between two expected values: the mean for single-birth individuals and the lower mean for normal twins." The actual data are as follows:

Category	Number	Standardized mean IQ
A. Single survivors of twins	148	98.8
B. Surviving twin pairs	1,924	95.2
C. Singletons	41,195*	99.5

*Estimated from (*1*), table 2.

Single survivors of twin pairs score only 0.7 IQ point less than do singletons, and 3.6 points more than surviving twin pairs, which is by no means halfway between the 4.3 points separating singletons and surviving twin pairs.

Second, by our calculations Humphreys is also mistaken in his computation of the statistical significance of these results. As a preliminary step, we examined the statistical power of each comparison. We assumed a standard deviation of 15 throughout. In the comparison of categories A and B, the power to detect a significant result (for a two-tailed test, given the observed difference of 3.6 IQ points) is 83 percent.

In the comparison of categories A and C, given the difference of 0.7 IQ point, the power is less than 25 percent. Nonetheless, the two comparisons have similar power when (with the same numbers, standard deviation, and significance level) they are applied to observed difference of the same size. Thus the test of the two hypotheses, that singleton survivors of twins do not differ from singletons while pairs of surviving twins do so differ, is fair with regard to power.

Record *et al.* made no mention of significance tests, and it is incorrect for Humphreys to say they "were unable to reject the null hypothesis. Thereupon they accepted that hypothesis." We can agree with Humphreys that there is no statistically significant difference between category A (single survivors of twins) and category C (singletons), using the *t*-test. Contrary to Humphreys, however, we *do* find a statistically significant difference between category A (single survivors) and category B (twin pairs) at the 5 percent level. Therefore it seems to us that any need for more data to support this result does not stem from the statistical reason that Humphreys gives, but from the logical reason that a unique and important result should be tested by replication.

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Women and *Science* Editorials

Publicly, most scientific organizations are committed to integrating women into professions which, until now, have been dominated by men. Women often claim