Marshall reports on criticisms directed against the pediatric lead studies of Needleman *et al.* by consultants to EPA.

Needleman *et al.* (*1*) evaluated the neuropsychologic performance of 58 children with high dentine lead concentrations and of 100 children with low concentrations. The children with the high lead concentrations achieved a mean IQ score on the Wechsler Intelligence Scale for Children which was approximately four points below that of the children with low concentrations. Analysis of variance indicated that this difference could not be explained by any of 39 covariates, such as age, sex, race, socioeconomic status, past medical history, or child-rearing practices. Needleman *et al.* (*2*) demonstrated subsequently that the downward shift in IQ was associated with a significant reduction in the number of children in their high lead group with IQ's above 125 and with a trebling in the number having IQ's below 80. These findings have been generally corroborated in studies by Ernhart *et al.* (*3*), Lansdown *et al.* (*4*), Winneke *et al.* (*5*), and Smith *et al.* (*6*). A recent review by Rutter (*7*) concluded that those studies of the association between lead and IQ in which close heed has been paid to methodologic issues have consistently found a dose-response association between lead exposure and neuropsychologic deficit. This effect appears to have been indisputably established at blood lead concentrations above 40 micrograms per deciliter, and there is growing evidence to suggest that, even at lower concentrations, lead can cause a subtle, but irreversible, reduction in children's intelligence.

Atmospheric lead emissions have increased 2000-fold since the pre-Roman era, and even then the atmosphere may have been contaminated with lead by a factor of 3 over natural background (*8*). In 1978, the second National Health and Nutrition Examination Survey (*9*) estimated that 660,000 children age 6 months through 5 years in the United States had blood lead concentrations above 30 micrograms per deciliter, the concentration considered by the Centers for Disease Control (*10*) to represent excessive lead absorption. Taken together with the data of Needleman *et al.* (*1, 2*), these findings have profoundly disturbing implications, not only for the future development of the affected children, but for the whole fabric of modern American society. Fortunately, the number of children in the United States with acute high-dose lead poisoning has declined precipitously during the past decade as the result of implementation of the Lead-Paint Poisoning Prevention Act. Also the background rate of pediatric lead absorption has been reduced substantially, most likely as a result of the EPA-mandated phasedown in the lead content of gasoline (*11*). This latter accomplishment is, in no small measure, a consequence of Needleman's work.

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With Marshall's report, we finally have an independent published report, in an important journal, of criticism of the "classic" Needleman study of lead effects. Some of the problems identified by the Expert Committee on Pediatric Neurobehavioral Evaluations appointed by EPA have been mentioned in my public statements. Other problems, such as biases associated with missing data, were barely discernible in the published reports and are now substantiated through the committee's limited access to the data.

Since my role as adversary is mentioned, I would like to clarify a few points. Needlemen attributes criticisms of his work to industry affiliation; however, at the time of the first publication (*1*) in which I refer to problems in his work, I had no connection with the lead industry. (I now have a modest research grant from the International Lead Zinc Research Organization.)

Marshall states that I have maintained "that there is no proof of a causal relationship between high levels of lead in children's blood and low scores on IQ tests. . . ." This is misleading. There is a definite and tragic risk of intellectual deficit associated with *high* levels of exposure, and I have never stated otherwise. My assertion is that there is no convincing evidence of an association of low level lead exposure and measurable deficits in psychological development when other relevant factors (such as parent intelligence, home environment, and so forth) are controlled. Unfortunately, efforts to blame the problems of many of our disadvantaged children on lead exposure tend to detract our attention from the amelioration of the difficult conditions in which these children are reared.

My "docility" about the committee's findings results from a respect for the competence and courtesy of its members and a recognition of the validity of the major features of its review. My associates and I cooperated in supplying EPA with complete sets of our data. We also conducted reanalyses as requested. (An additional reanalysis has since been requested.) The corrected results did not support the conclusion that low level lead exposure is related to deficits in intelligence. This applies to the earlier work mentioned by Marshall, as well as the data from the 1981 report. The sample sizes, however, do not provide the statistical power necessary for a reasonably definitive statement that there is little or no effect.

Recent, well-controlled studies (*2, 3*) in the United Kingdom also failed to support the Needleman position. One study (*3*), directed by Marjorie Smith, is particularly important because it replicated Needleman's procedures with a large sample and sound methodology. After appropriate control of confounding variables, no significant association of dentine lead and intelligence was found.

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Erratum: In the Research News article "New test of variable gravitational constant" (23 Dec., p. 1316), in the third paragraph of column two on page 1317, a minus sign was left out of an exponent. The correct value for the drift of atomic clocks relative to gravitational clocks is $(0.1 \pm 0.8) \times 10^{-11}$ per year.