Magico-religious Mercury Exposure

Mark Wheeler's Focus article, "Measuring Mercury" (*1*), which appeared in the August 1996 issue of EHP, contained a serious omission. Wheeler concentrated on methyl mercury and, to a lesser extent, elemental mercury in dental amalgams. He failed to mention the relatively recently described but extremely significant exposures to elemental mercury in ethnically Hispanic and Caribbean homes, consequent to its use for a variety of magico-religious and ethnomedical purposes (*2–3*).

Such domestic use and presumed exposure has been documented in a number of published papers, as well as by research sponsored by the ATSDR (*4–6*) and the EPA (*7*). In fact, an ATSDR monograph specifically alerts clinicians to this exposure pathway: "Metallic mercury has been used by Mexican-Americans and Asian populations in folk remedies for chronic stomach disorders and by Latin-American and Caribbean natives in occult practices" (*4*). This monograph was edited by Thomas Clarkson, who was interviewed by Wheeler, and who has long been aware of elemental mercury's domestic use. Similarly, the EPA's Kathryn Mahaffey, also interviewed, has been aware of domestic mercury exposure for some years, and the EPA issued a risk assessment document on cultural uses of mercury in 1993 (*7*).

These mercury exposures are especially significant from an environmental health perspective because, in many cases, they are certain to be orders of magnitude greater than (methyl) mercury exposures from eating fish or from the leaching of mercury in amalgam fillings. Additionally, the mercury vapor released from mercury intentionally sprinkled on floors affects all occupants of contaminated homes, from the fetus to the elderly.

Andrew Rowland, cited in "The Issue of Amalgams" (*1*), has been aware of domestic mercury exposure for several years. Rowland makes a call for more research on health effects of amalgam–mercury exposure. I make a similar call for research on magico-religious mercury exposure. If the environmental health research community continues to ignore magico-religious mercury exposure, its health effects will never be ascertained.

Arnold P. Wendroff
Mercury Poisoning Project
Brooklyn, New York

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MMA:DMA Ratios Reversed

I would like to bring to your attention an apparent typo in a recent response written by Mushak and Crocetti in Environmental Health Perspectives (*1*). In describing a publication by Warner et al. (*2*), they note that "the corresponding MMA:DMA ratios for exposed and control subjects were 0.32 and 0.5 ..." (p. 1017, first column). These values should be reversed.

As reported by Warner et al. (*2*) and correctly cited by Mushak and Crocetti in their original commentary (*3*), urinary arsenic concentrations were 190 mg MMA/l and 390 mg DMA/l for the exposed group, and 14 mg MMA/l and 44 mg DMA/l for the control group. Therefore, the actual MMA:DMA ratios should be 0.49 (190/390) for the exposed group and 0.32 (14/44) for the control group. These correct ratios are consistent with our hypothesis that MMA:DMA ratios tend to be higher in exposed populations and that methylation may be less efficient as dose increases. For clarification, this error should be noted.

Tracey Slayton
Gradient Corporation
Cambridge, Massachusetts

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3. Mushak P, Crocetti AF. Risk and revisionism in arsenic cancer risk assessment. Environ Health Perspect 103:684–689 (1995).

Response

Dr. Slayton calls attention to a minor typo in our response letter to Slayton et al. in the October 1996 EHP (*1*). She notes that the two ratios of MMA:DMA were in reverse order relative to the antecedent corresponding. The fact of the typo is correct as the wording appeared and you may wish to note a correction. The consequence of the typo, however, is nil for any of our interpretations in the response and the original commentary and therefore requires no editorial amplification by EHP.

As we noted in the commentary (*2*) and in the response to Slayton et al. (*1*), the Nevada MMA:DMA ratios of methylated arsenic compared to a control group are relatively insignificant as to change despite the high water arsenic exposures. This was and remains the main point. The amount of change is equally modest whether one is comparing 0.32 to 0.50 or 0.50 to 0.32. The context makes it clear what was intended. Getting these values reversed has no impact on anything we said or have interpreted in either article. Dr. Slayton seems to think the fact of the reversal of ratios would likewise compel us to reverse our conclusions and magnify the arguments of Slayton et al. That is not the case. We knew what was intended in both the commentary (*2*) and the letter (*1*).

Paul Mushak
PB Associates
Durham, North Carolina

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Reply to Comments on "A Reevaluation of Cancer Incidence Near the Three Mile Island"

In their letter (*1*), Hatch et al. appear to be confused about the purpose of Table 1 of our paper (*2*); the table merely shows that the differences between our results and theirs are not due to statistical methods. Our Tables 2 and 3 show that underascertainment of incident cancers, data management errors, and failure to adjust for baseline variation in cancer rates led Hatch et al. (*3*) to underestimate associations between estimated radiation doses from