

Mercury Levels in Mothers

I read with great interest the excellent article by Mahaffey et al. (2004), which further describes the characteristics of the 1,709 women from the National Health and Nutrition Examination Survey (NHANES) 1999–2000 who were sampled for total and organic mercury levels in blood. It adds valuable detail to the initial report published last year (Schober et al. 2003). I would appreciate clarification on one important point: in the “Discussion,” the authors cited a new analysis which indicates that the cord blood:maternal blood ratio is not 1:1 as assumed by the National Research Council (NRC) in 2000 (Committee on the Toxicological Effects of Methylmercury 2000), but rather 1.7:1. Using the same benchmark dose lower limit and uncertainty factor used by the NRC, Mahaffey et al. (2004) calculated that blood total mercury levels > 3.5 µg/L in mothers could be associated with increased risk to the developing fetal nervous system. I am very interested in the details of this analysis and particularly in understanding why the uncertainty factor applied by the NRC to account in part for toxicokinetic variability does not compensate for uncertainty related to the cord blood:maternal blood mercury ratio. This is a critical concept because it has a dramatic impact on how many women may carry mercury levels in excess of what is believed to be safe for a fetus.

The author declares she has no competing financial interests.

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Editors note: In accordance with journal policy, Mahaffey et al. were asked whether they wanted to respond to this letter, but they chose not to do so.

U.S. PBDE Levels: Effects in Mice

I am pleased to submit this letter as a representative of the American Chemistry Council Brominated Flame Retardants Industry Panel (BFRIP). The BFRIP is composed of producers of brominated flame retardants; member companies include Albemarle Corporation, Ameribrom Inc., and Great Lakes Chemical Corporation.

In a recent study, Sjödin et al. (2004) investigated polybrominated diphenyl ethers (PBDEs) in human sera collected in the United States between 1988 and 2002. The authors concluded that such levels were increasing over time and were higher than those reported in Europe. Several points regarding these conclusions require clarification and are addressed below.

Sjödin et al. (2004) used the term “PBDEs”; however, the PBDEs analyzed in sera were only the tetra to hepta congeners. These congeners are commonly found in the commercial pentaBDE product, which is used in flexible polyurethane foam in upholstery applications. The sole U.S. manufacturer of the pentaBDE product (Great Lakes Chemical Corporation, West Lafayette, IN) will voluntarily discontinue production by the end of 2004. However, approximately 80% of the global production of PBDEs is composed of the decabromodiphenyl ether/oxide (DBDPO) commercial product, which is used primarily in electrical and electronic components (typically television cabinet backs, connectors, and wire and cable insulation) and to a minor extent in upholstery textiles. DBDPO was not included in the set of congeners analyzed by Sjödin et al. (2004). Thus, the comments with respect to time trends, if valid, apply only to tetra- to heptaPBDE congeners and not the major PBDE product in production and use, DBDPO.

Second, the results indicate that the PBDEs, and BDE-47 in particular, for the last two time intervals (1995–1999 and 2000–2002) appeared to level off. Of the six isomers analyzed, only BDE-153 appeared to increase between 1995–1999 and 2000–2002. Thus, the most recent data suggest that, in general, U.S. PBDE serum levels for the lower congeners are not continually increasing but have reached a plateau.

Third, the authors state that BDE-47 concentrations collected in similar time frames and reported by other studies in milk (83 or 130 ng/g lipid) and sera (0.63 ng/g lipid, 1988) compare “favorably” with their present sera results of 46 (1995–1999), 34 (2000–2002), and 5.4 (1985–1988) ng/g lipid. These values appear dissimilar from one another and appear to point out highly variable, rather than similar, results.

Finally, we question the validity of a comparison of U.S. to European PBDE levels. As indicated by Sjödin et al. (2004), the analyzed sera were not collected in such a way as to be representative of the general U.S. population. The same is likely true with respect to the blood and milk samples collected in Sweden; these samples are unlikely to be representative of the general European population. Thus, based on this collection process, one cannot reach reliable conclusions regarding U.S. versus European levels.

I would also like to correct information reported regarding manufacture of polybrominated biphenyls (PBBs). Sjödin et al. (2004) stated that the hexaBB product continued to be produced in Europe after the Michigan incident in the 1970s in which it was accidentally included in cattle feed. After that incident, production of only the decabromobiphenyl (decaBB) product, not the hexaBB product, continued in Europe, and that production ceased several years ago. The decaBB product did not exhibit the same toxicologic properties as the hexaBB product.

Finally, Sjödin et al. (2004) stated that “PBDEs cause neurodevelopmental effects in mice . . .,” citing Eriksson et al. (2001, 2002) and Viberg et al. (2002). Taylor et al. (2002) were unable to reproduce these effects in rats, whereas Viberg et al. (2004) reported similar results in rats and mice. Perhaps these diverging results are related to the small sample size and statistical design used by Eriksson et al. (2001, 2002) and Viberg et al. (2004) that grossly inflates the type 1 (i.e., false positive) error rate. Eriksson et al. and Viberg et al. both used mouse pups as the experimental unit, whereas the litter is the more appropriate measure [U.S. Environmental Protection Agency (EPA) 2004; Organisation for Economic Co-operation and Development (OECD) 2003]. Litter effects are substantial, and using more than one pup from a few litters, as reported by Eriksson et al. (2001, 2002) and Viberg et al. (2004), will confound treatment effects with litter effects (Holson and Pearce 1992). Holson and Pearce also stated that “within-litter variance would likely become substantially lower with age than that between litters.” This would further increase the already sizeable effects of litter and may account for the conclusions of Eriksson et al. (2001, 2002) and Viberg et al. (2004) that neurodevelopmental effects increase with age.

The author is the manager of the American Chemistry Council’s (ACC) Brominated Flame Retardants Industry Panel (BFRIP). ACC is a trade association representing the leading companies in the business of chemistry. BFRIP is composed of the manufacturers of brominated flame retardants. The members of BFRIP are

Albemarle Corporation; Ameribrom, Inc.; Bromine Compounds, Ltd.; and the Great Lakes Chemical Corporation.

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PBDEs: Sjödin's Response

I appreciate Cleet's response to our paper concerning time trends of polybrominated diphenyl ethers (PBDEs) and related compounds in the U.S. population (Sjödin et al. 2004), and I appreciate the opportunity to address his comments.

Pentabromodiphenyl ether (pentaBDE), along with the lower brominated congeners, was the topic of our investigation (Sjödin et al. 2004). Cleet's statement emphasizing that the current production of PBDEs is solely in the form of decabromodiphenyl ether belittles the fact that, in 2001, 95% of the 7,500 metric tons of pentaBDE was produced and consumed in the United States [Bromine Science and Environmental Forum (BSEF) 2003]. The industry's with-

drawal of pentaBDE and octabromodiphenyl ether (octaBDE) from the market by the end of 2004 will decrease environmental output. However, continued monitoring of environmental and human levels is needed to measure exposures originating from pentaBDE and octaBDE manufactured before 2005 and to study potential exposure to decaBDE, which will continue to be manufactured.

Cleet's second remark proposes the possibility that current human PBDE levels have reached a plateau. Because of the variability in our data (Sjödin et al. (2004) and the regionalized sampling, we believe such a conclusion may be premature. As Cleet mentions later in his letter, these studies may not be representative of U.S. and European populations. We did not claim that the sampled pools are representative. To further confirm and track our preliminary observations of human exposure to PBDEs, broader representative studies have been proposed.

Cleet's third issue concerns comparability of our data on BDE-47 with earlier studies. We referenced several publications regarding the similarity of our measured levels to earlier findings. In a 1988 Illinois study, human levels of BDE-47 were reported to be 0.63 ng/g lipid, with a range of < 0.4–24 ng/g lipid (Sjödin et al. 2001). These Illinois levels can be contrasted to the data from serum pools collected in the southeastern United States, where we found a range of < 1–6 ng/g lipid for the same year [see Figure 1 in our paper (Sjödin et al. 2004)]. We also compared our BDE-47 levels to those in other studies: for example, 33 ng/g lipid in breast adipose tissue (range 7–200 ng/g) collected in the late 1990s (She et al. 2002); 83 ng/g lipid in a milk pool ($n = 19$) collected in 1997 in New York (Betts 2002); 130 ng/g lipid in a milk pool collected in 2000 in Austin, Texas, and Denver, Colorado (Päpke et al. 2001); and 41 ng/g lipid in milk collected in 2001 in Texas (Schechter et al. 2003). These authors reported concentrations in the same range as our study [e.g., Figure 1 in our paper (Sjödin et al. 2004)].

I appreciate Cleet's clarification concerning production stoppage of hexabromobiphenyl (hexaBB) in Europe. Also, Cleet's speculation about the differences in outcomes in animal studies is potentially useful. Although we did not study toxic effects of PBDEs, we asserted the cited studies to be examples of potential concern. We selected the work of Eriksson and colleagues in this regard, demonstrating observed effects in four publications: Eriksson et al. (2001, 2002), Viberg et al. (2002), and Sand et al. (2004).

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The Human Population: Accepting Earth's Limitations

I thank Fowler and Hobbs for their letter (2004) and their research (2003). The view that a complexity of factors impacts human population growth certainly makes sense, and they have correctly pointed out that scientifically organized efforts to deal with human problems must take account of manifold interconnected events. Although it is necessary to recognize and acknowledge the complexities inherent in cultural life and the natural world, it is equally important that a dizzying array of variables not blind us to certain scientific facts of biophysical reality. Humankind is bound by such predominant facts because the workings of the world exist independently of human wishes and beliefs.

With this in mind, I thank Hopfenberg for his article (2003) in which he provided an elegant model that accounts for the salient factors governing the dynamics of global human population numbers. According to his findings, the size of the human population is determined primarily by food availability.

The realization that these two points of view differ—that there is complexity and simplicity in the world we inhabit—does not necessarily mean that one is correct and the other incorrect. To the contrary, it could be that each point of view is valid based on the scope of observation.

It may be somehow not quite right to agree with the entire idea of Hobbs and Fowler (2004) that “human population size is beyond human capacity to list, comprehend, and synthesize” without noticing that the same can be said regarding any observable phenomenon. Reality is likely just as complex as Hobbs and Fowler described; but it is also clear from the research of Hopfenberg (2003) and Hopfenberg and Pimentel (2001) that the dynamics of human population growth is no longer preternatural but knowable, and that the population dynamics of *Homo sapiens* is not essentially different from the population dynamics of other species in both the complexity and the simplicity of the governing elements.

A comprehensive and objective approach to human problems and human potentiality must acknowledge that humankind is a part of the biophysical world, not apart from it. Although Hobbs and Fowler (2004) are correct to note the control human culture exercises in “value systems, economics, politics and religion” in taking account of what is real, human and environmental health could be increasingly at risk because humanity denies scientific facts over which living beings may not have control.

In light of the different sets of data presented by Fowler and Hobbs (2003) and by Hopfenberg (2003), perhaps it is a misnomer for Hobbs and Fowler (2004) to uniformly describe the many, complicated ways humanity is changing the natural world as an “unprecedented success.” Are particulate and solid-waste pollution or the conversion of biomass into human mass with resulting biodiversity loss examples of success? Perhaps the economic success of the prevailing culture is not sustainable and cannot be maintained much longer. Unbridled economic globalization, unrestricted increases in human consumption of resources, and growing absolute human population numbers are negatively affecting Earth by degrading its fitness as a habitat for humans and other species.

A point in human history may have been reached when the scale and rate of

growth of economic expansion, the consumption of natural resources, and the increasing human population can be seen as patently unsustainable. Understanding the causes of and limits to humanity’s impact in the world is a necessary step toward changing human production, consumption, and population trends. Regardless of how long a culture prizes growth and chooses to leave it unchecked, surely it is not too late to accept limits to growth of the human economy, human consumption, and human numbers worldwide by altering human behavior accordingly.

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Conflicts of Interests: Declarations for All

Concerning your editorial, “Embracing Scrutiny,” in the October issue of *EHP* [*Environ Health Perspect* 112:A788 (2004)], the need for full disclosure of all potential conflicts of interest by all coauthors contributing to a publication in *EHP* is commendable and obviously needed. Might I take this one step further and suggest that all reviewers of *EHP* manuscripts be required to sign a form listing all of their potential conflicts of interest.

The author declares he has no competing financial interests.

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Editor’s note: In our Instructions to Authors (<http://ehp.niehs.nih.gov/docs/admin/edpolicy.html>), we do require editors and reviewers to disclose competing financial interests but are not currently requiring a signed form. We are considering taking that next step.

CORRECTIONS

In the letter by Storm and Mazor [*Environ Health Perspect* 112:A862–A864 (2004)], the title of Table 1 was incorrect; also, P1 and P2 were not exposed but were unexposed children tested in another study. The corrected table is presented below.

Table 1. Child participants in VCS studies.

ID	Exposed		Unexposed		
	Age	DD or ADD	ID	Age	DD or ADD
E9 ^a	8	–	C9 ^a	9	–
E10 ^{a,b}	6	X	C10 ^a	8	–
E14 ^{a,b}	12	X	C14 ^a	12	–
E17 ^a	6	–	C17 ^a	5,7	–
			P1 ^b	8	X
			P2 ^b	10	X

^aChildren shown in Figure 1A (NYSDOH, unpublished data; Schreiber et al. 2002). ^bChildren shown in Figure 1B: E10 and E14 were from Schreiber et al. (2002); P1 and P2 were unexposed children examined in an NYSDOH study (NYSDOH 2004).

At the time the October 2004 Forum article “Farm Chore Checkup” [*Environ Health Perspect* 112:A804 (2004)] went to press, Anne Gadomski’s assessment of the North American Guidelines for Children’s Agricultural Tasks was scheduled for publication in the October 2004 issue of the *American Journal of Public Health (AJPH)*. However, publication of the assessment in *AJPH* was delayed; a new publication date has not been set. *EHP* regrets the error.

Wasserman et al. detected errors in their article “Water Arsenic Exposure and Children’s Intellectual Function in Araihaazar, Bangladesh” [*Environ Health Perspect* 112:1329–1333 (2004)]. In the first paragraph of “Results” (p. 1331), the values should be reversed to read “On average, mothers and fathers reported 2.9 and 3.7 years of education, respectively.” In the second paragraph of “Results” (“Exposure characteristics”), the mean water As concentration should be 117.8 µg/L, not 117.8 µg/dL.

Also, on page 1332 in “As metabolism,” the authors would like to clarify that Chowdury et al. (2003) reported that only the first reaction of the arsenic metabolic pathway—the formation of MMA—is less active in children than in adults.