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To the Editor: We were glad to see the recent article on mortality among workers exposed to polychlorinated biphenyls.[1] At a time when fewer and fewer companies are funding occupational epidemiological studies, we commend the sponsor, General Electric, for this initiative. The completeness of case ascertainment was outstanding. In addition, this report was a model of clear writing and clear display of results.

However, two issues, sample size and exposure, raise significant concern. First, the study population was very small. Over 7000 workers contributed over 200,000 person-years of observation, more than in prior PCB mortality studies. But when attention is restricted to those workers with high exposure, moderate- to long-duration employment, and adequate person-time after a latency period, the numbers are dramatically reduced. For example, only one third of the cohort worked for longer than 5 years. (We note in passing that Table 2, the source of these data, shows 7178 workers in the upper panel and 7075 workers in the lower panel, a disparity the authors do not explain.) Similarly, less than one fourth of the cohort was classified as highly exposed, and the median period of high exposure was less than 2 years. Although data are not presented to support exact calculations, it appears that fewer than 10 cancers of any type, and more typically fewer than three, were expected in any sex-salary stratum with high exposure, more than a year of employment, and more than 20 years of latency. Could this be why the article is conspicuously silent on the issue of statistical power?

The problem of small number could have been addressed. A company as large as GE presumably had other capacitor plants and could have supported a multisite study. Alternatively, an industry-wide study would have been informative, as we have seen in the semiconductor, rubber, petrochemical, automobile, and other industries. Indeed, we wonder why restricting a cancer mortality study to only two plants should not be viewed as a willful effort to avoid a positive finding.

The second major concern lies with exposure assessment. As with many historical cohort studies, the authors created a matrix to characterize each individual's exposure. If the designated "high exposure" jobs did not actually entail high exposure, then misclassification occurred and could have introduced substantial bias toward the null. Were the exposures accurately assessed?

The article makes reference to a readily available way to validate the exposure assessment: serum PCB levels obtained during the 1970s on a sample of several hundred cohort members. Where are these measurements? Did the authors check their exposure assignments against the past serum measurements? If not, why not? If so, why was this comparison not reported?

Another difficulty with exposure in this article is the admixture of various types of PCBs. More carcinogenic forms, such as Aroclor 1254, were used in the early years, and less carcinogenic forms, such as Aroclor 1016, were used later. By combining the two rather than focusing on the early exposures, the authors may have obscured a true effect.

Overall, these concerns significantly limit the conclusions that can be drawn from the study. The authors conclude that their results "would suggest a lack of an association." This conclusion is overstated. These results do offer some evidence that PCBs are not highly potent carcinogens causing relative risks above 10 or 20, a conclusion that was already fairly well established. But they provide little reassurance that PCBs do not double or triple the risk of some cancers after significant exposure.

For this reason, we were especially concerned that the results of the study were not interpreted and presented more carefully. The authors might have noted, in their conclusion, that PCBs are serious health hazards irrespective of carcinogenicity,[2] with effects that include decreased birth weight,[3] neurodevelopmental abnormalities,[4–8] and interference with both estrogen[9] and thyroid[10] hormone function. Accordingly, even negative findings in a cancer study would not reassure us of safety. That omission in the *JOEM* article, in turn, may have contributed to overtly misleading journalistic coverage, such as the *New York Times* headline: "Study Finds Little Risks [sic] From PCB's."[11]

The authors of this study note that our knowledge of PCB health effects is "limited." On the path to a more complete understanding, the current study results represent a great leap sideways.

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The Authors Reply: Thank you for giving us the opportunity to reply to the letters by Bove et al and Frumkin and Oris commenting on our mortality study of PCB-exposed capacitor workers.[1] We disagree with the statement by Bove et al that "... limitations in the study approach tend to dilute any excesses in cancer mortality resulting from PCB exposure. . . . " These assertions are speculative and not supported by the data. Although some degree of misclassification in observational studies is unavoidable, it is usually not possible to determine whether this misclassification is differential or non-differential. Furthermore, non-differential misclassification does not always result in bias toward the null hypothesis. Neither the type nor the effect of the misclassification can be determined by Bove et al. In our article, we do, however, discuss at length the measures taken to limit misclassification, and we feel strongly that we were successful in doing so.

Bove et al assert that the healthy worker effect (HWE) results are an underestimate of the SMRs for all-causes mortality and cancer mortality. This is partially true. The HWE is most pronounced for cardiovascular deaths and thus affects all-causes mortality.[2] It has much less of an effect on cancer deaths.[3]

The presentation by Bove et al of the all-cancers SMRs and selected cancer-specific SMRs without confidence intervals (CIs) gives incomplete information and is misleading. Had the confidence intervals been reported, the lack of significance for these SMRs would have been immediately obvious to the reader. Bove et al selected the female hourly employees' all-cancers SMR of 110 (95% CI, 93 to 129), intestinal cancer (SMR = 157; 95% CI, 96 to 242), rectal cancer (SMR = 169; 95% CI, 46 to 434), melanomas (SMR = 144; 95% CI, 30 to 421), and melanomas in male hourly employees (SMR = 130; 95% CI, 42 to 303). Notably absent from this list of SMRs considered by Bove et al are the male hourly SMRs for intestinal and rectal cancer (SMR = 57; 95% CI, 25 to 112; and SMR = 87; 95% CI, 18 to 255, respectively).

Bove et al suggest that the male all-cancers SMRs of 81 (hourly employees; 95% CI, 68 to 97) and 69 (salaried employees, 95% CI, 52 to 90) are largely due to the HWE. A careful examination of Table 4 in our article suggests that the statistically significantly low all-cancers SMRs in both the hourly and salaried males result primarily from the lower than expected lung cancer SMR (for hourly workers: 42 observed/54.5 expected; SMR = 77; 95% CI, 56 to 104; and for salaried workers: 12 observed/29.6 expected; SMR = 41; 95% CI, 21 to 71).

The statement by Bove et al that these elevations were consistent with elevations found in other studies of PCB-exposed workers is not correct.[4–6] In addition to the three studies cited by Bove et al, there is the Bertazzi cohort and its update by Bertazzi et al[7] and Tironi et al.[8] The results of the Brown[4] and Sinks et al[5] studies are inconsistent with each other. The Loomis et al[6] study of utility workers, not capacitor workers, did report an elevation in melanomas in some subsets of the cohort that were presumed to have had exposure to PCBs while working outdoors. Exposure to sunlight was not adequately accounted for by Loomis et al.[6] Brown and Jones[9] and Brown[4] found an excess of liver and rectal cancers. Neither Sinks et al[5] nor Loomis et al[6] reported such increases. Sinks et al[5] reported a nonsignificant elevation in brain and nervous system cancers. Neither Brown and Jones,[9] Brown,[4] Bertazzi et al,[7] or Tironi et al[8] found an elevation in brain cancer. These inconsistencies were discussed in our article.

Bove et al state that we only included a latency-period analysis for all cancers and for intestinal cancer. This was done primarily because of space limitations. Cumulative exposure and latency tables were computed and evaluated for many other causes of death, including all of the cancers of interest. The interpretation by Bove et al that the intestinal cancer SMR increases to a significant level for women with ≥20 years of latency ignores the importance of examining the trend associated with latency and length of employment. Furthermore, it might be worth noting that for women employed for 10

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