

## Comments on "A Reevaluation of Cancer Incidence near the Three Mile Island Nuclear Plant"

This issue of the journal includes a critical review and reanalysis by Wing et al. (1) of a cancer study we conducted in the aftermath of the 1979 accident at the Three Mile Island (TMI) nuclear plant (2,3). We find the lengthy piece tendentious and unbalanced. No notice is taken of any of the innovations of the original study, such as the exposure model that took detailed account of prevailing winds and topography. As the findings from the reanalysis differ little from the original study, we will focus our comments on four brief points.

First, both our initial views and subsequent conclusions about the possibility of an accident-related cancer increase have been misrepresented. At the time we undertook the study, we were doubtful about effects of exposure, and appropriately so, given both the very low official estimates of the TMI releases and the short latency period. Nonetheless, we did think it was possible that unmonitored releases might have been greater than those estimated and thus might have produced levels of radiation exposure greater than background levels. Analysis of the off-site thermoluminescent dosimeter data available to us toward the end of our study led us to rule this out, however, and to conclude that the releases were in fact within range of official dose estimates. If the dosimeter data had yielded a different result, our interpretation of the findings would have reflected this. The conclusions we did reach have also been misrepresented. Wing et al. (1) claim we "concluded that observed associations did not reflect an accident effect." We actually said the following: "Overall, the pattern of results does not provide convincing evidence that radiation releases from the Three Mile Island nuclear facility influenced cancer risk during the limited period of follow-up" (2).

Second, contrary to what Wing et al. (1) claim, we did in fact specifically recommend follow-up of the TMI area population, both in the author's reply (4) to the commentary accompanying initial publication of the paper on cancer and radiation emissions at TMI (2) and also in official communications with the Three Mile Island Public Health Fund. Indeed, the fund accepted the recommendation for follow-up, and such studies are currently under way at the University of Pittsburgh under the direction of Evelyn Talbott.

Third, the initial assumptions of Wing et al. (1), and the context in which they interpret their results, are based on strictly

anecdotal reports of symptoms. These reports are consistent with radiation poisoning, but inconsistent with even the worst-case scenarios concerning radiation releases from TMI. The sole supporting evidence Wing et al. cite for assuming levels of radiation emissions that could lead to vomiting and hair loss comes from cytogenetic analysis by Russian scientists of 29 symptomatic individuals from TMI. These data, which we have not seen, are reported in a 1996 Russian publication, which was certainly not available to us at the time of our study. It seems odd that these data, if meaningful for the United States and the people of Three Mile Island, should appear in such an out-of-the-way place.

Fourth, the principal difference between our work and that of Wing's team is in the interpretation of results and not in the results themselves. Their replication of our original analysis produced figures identical to ours "within rounding error" [see Table 1 of Wing et al. (1)]. In addition, the analysis based on their model gave results for all cancers that were quite similar to the result found with our approach. Like us [see Tables 2 and 3 in Hatch et al. (2)], Wing et al. find positive associations of accident dose with all cancers, lung cancer, and adult leukemia. There are no new findings here, only a new interpretation—partly for the reasons mentioned above and partly, we suspect, because of a change in the zeitgeist. Due to the concerns of the time about radiation risks to children living near nuclear plants, childhood cancer was a focus of our analyses, but children have been omitted from the reanalysis published here.

In addition to these four major points, we wish to make some additional corrections or clarifications (that are by no means exhaustive).

- Wing et al. (1) mischaracterize the history of our dosimetric model. There were no court-imposed limitations on our exposure models. The only limitation involved our agreement to use an exposure model rather than upper limit dose calculations, which are not suitable for an epidemiological study in the first place.
- Wing et al. also fail to acknowledge that our use of relative rather than absolute doses is an approach designed to overcome the very uncertainties in radiation dosimetry that they cite as concerns.
- Pool (5) did not attribute the statement "... already believed that the low levels of radioactivity released by the accident were unlikely to have a measurable effect on cancer rates" to Hatch et al., as Wing and his coauthors claim, but rather to unnamed scientists.
- In spite of a possible undercount of 1975

cancers, there was no evidence that this was geographically biased rather than randomly distributed throughout the study area. Because we defined exposure to radiation in geographic terms, we saw no need to exclude the 1975 data.

- We considered but rejected a post- versus preaccident analysis such as Wing et al. have conducted because the TMI plant was operational in the preaccident period and we had also undertaken the evaluation of effects of emissions during routine operations.
- Surprisingly, Wing et al. (1) do not seem to have adjusted their standard errors to reflect the *a posteriori* nature of their hypotheses.

What leads two groups of epidemiologists to attach different meaning or give different emphasis to essentially the same data is a puzzle that is likely to remain with us for as long as subjectivity plays a role in epidemiology (6). The best we can do is to state clearly and completely the assumptions we begin with and the reasons for the conclusions we reach. After that, it is up to the reader. Indeed, we urge readers of the critique by Wing et al. (1) and our response to refer to our original publications before reaching a judgment.

Maureen Hatch

Mt. Sinai School of Medicine  
New York, New York

Mervyn Susser

Columbia School of Public Health  
New York, New York

Jan Beyea

Consulting in the Public Interest  
New York, New York

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