Correspondence

Radiation Exposure and Cancer: A Simpler View of Three Mile Island

The article by Wing et al. (*1*) introduced the idea of calculating the relative intensity of radioactive fallout between sectors of a circle drawn around the accident at Three Mile Island. The partition derived from wind and weather data allowed a comparison of ratios of radiation exposure between sectors with cancer incidence ratios and bypassed much of the uncertainty involved in calculating actual doses. The results indicated that higher fallout was correlated with higher cancer incidence. Considering that the average levels of radioactivity in each sector were low, this could be interpreted as evidence for a proportional carcinogenic response to low doses of ionizing radiation. There is, however, a simpler way to interpret the same set of data.

Wing et al. (*1*) reported that some of the people exposed to the fallout from the accident showed signs of acute radiation damage. They also reported that ground measurements of radioactivity were not adequate to show the details of distribution of the fallout or the resulting doses to individuals. We are free to infer that the fallout was patchy, that some of the residents were exposed to high doses of ionizing radiation, and that the data of Wing et al. (*1*) show a correlation of the frequency of high exposure "hits" on people in each sector with the sector's share of the region's radioactive burden. In this view, increases in cancer incidence were simply correlated with the number of people hit by high doses of ionizing radiation. This interpretation is consistent with the conclusions drawn by Raabe et al. (*2*) from data on radium dial painters, the data set that offers the most precise readings of both individual radiation doses and carcinogenic effects in humans. It is also consistent with the patchy distribution of radioactive fallout from explosions often observed at the Nevada Proving Grounds.

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Consequences of the 1979 Three Mile Island Accident Continued: Further Comment

As principal investigator of the Three Mile Island (TMI) Accident Study funded by the TMI Public Health Fund, I appreciate the space for some observations further to our reply (*1*) to Wing's reanalysis (*2*) of our study (*3,4*). Two far from careful or evenhanded news items (*5,6*), and your own editorial (*7*) call for further comment.

My principal colleagues in this study (Jan Beyea, Maureen Hatch, Sylvan Wallenstein) and I are committed to rigor in science as well as to the public health and environmental movements. We applaud your desire to air controversy. But we forsee and fear the ultimate discredit that poor science, together with advocacy parading as science, can bring to epidemiology and the movements of which we are a part. The essential point is that what you label controversy is not controversial in any real sense.

Instead, we have a situation manufactured from misconceptions, misinterpretations, mistaken logic, and simple error. Our results and those of Wing et al. (*2*) differ in no important respect. Our conclusions do differ: we saw no convincing evidence that cancer incidence was a consequence of the nuclear accident; they claim there is such evidence. We urge you and those of your readers interested in the issue to study our original papers and reports before judging. Your own (*5*) and other news reports [for instance, *The Lancet* (*4*)] ignored our published response to the brouhaha. In that light, we need to expand on some points and make some new ones.

At the heart of the matter, it seems to us, is Wing's assertion that our original interpretation is based on circular reasoning. He makes this charge, he says, because we did not believe in the hypothesis under test. The first of two objections to this charge is that it is untrue. We had no such simplistic belief. At the outset, in the light of the uncertainties about the dose of radiation from the 1979 nuclear accident, and also of a reported cluster of deaths that conceivably pointed to acceleration of cancers already initiated, we accepted the possibility of an effect.

At the same time, given the short postaccident observation period and the putatively low dose, we were doubtful that any but the most radiosensitive cancers could be detected. We did not seek, but were sought out, to investigate on behalf of the TMI Public Health Fund. Our acceptance of the considerable undertaking involved was realistic, with no great expectation of startling results. Public duty at a time when fear and unrest beset the affected communities was a strong motive.

This mistaken allegation about our beliefs is much the lesser of our two objections. The greater objection is to Wing's claim that circular reasoning led to failure to prove an *a priori* hypothesis we allegedly did not believe. To test an *a priori* hypothesis, which we did, is of course a procedure specifically designed to preclude circularity. More disturbing is the religious cast of mind this charge displays. To make a prior belief a criterion for judging evidence is the very antithesis of any scientific or logical method, from the inductivist Francis Bacon early in the 17th century to the hypothetico-deductive Karl Popper in the 20th. Whatever we do, we must surely aim for a maximum of rigor and objectivity. One is obliged to attempt disproof no less than verification. In striving toward elusive truth, *a priori* belief is beside the scientific point. Passionate belief, which characterized Wing, may well be a handicap.

An *a priori* hypothesis subject to test, by contrast, is a considerable asset regardless of belief. Wing's position amounts to a charge that we are either incapable of understanding our data—on that score let our records speak—or that we obfuscate or lie. In your news report (*5*), indeed, Wing suggests the latter: he is quoted as ascribing our conclusions to author bias. (In this respect, should there not have been mention of Wing's role, with regard to the accident, as witness for the plaintiff in personal injury litigation against the TMI utilities?)

Wing's more specific case against our report rests mainly on two particular issues, namely, his use of relative rather than absolute dose and the adjustments he made for baseline conditions. Let us take these in turn.

Wing makes much of his use of relative dose as "an alternative logical approach" and seems to reproach us on this score [see Wing et al. (*2*), page 53, second and third columns]. Although they later note that we in fact used relative dose, in remarks to the press (*5*) we are again reproved. So we must make clear that all our major analyses and results in fact derived precisely from the use of relative dose.

Relative dose is not an exact or complete description of what we did. In a major and labor-intensive effort, one of us (J. Beyea) carried out detailed topographic and meteorologic mapping of the area to model estimates of the direction and concentration of radiation emissions from the accident, from routine operations of the plant, and from background radiation. To our knowledge, ours was a unique approach to deriving exposure measures in the face of uncertain actual dose. We also divided the local area at risk into 69 census-derived

small tracts. We then compared cancer incidence and mortality rates in the tracts most heavily exposed with those less exposed, having taken account of both background radiation and routine plant emissions.

What Wing et al. (*2*) themselves did about relative dose is not clear to us. In their paper, no description was apparent, nor did we recognize any consideration of background radiation or routine emissions, both strong features of our overall analysis. We assume that they made use of our estimates of radiation distribution from the accident.

In our analysis, we judged observations after the accident to be the critical test in making adjustments for baseline values. We were cautious in adjusting for demographic and other such variables from the situation existing before the accident because of uncertainties in these data. No information was to be had about subsequent migration, and the target population could only be that exposed to the accident and remaining in the district thereafter.

In any case, in the matter of cancers as an outcome, our study sought effects of the accident strictly in one direction. On this ground, there would seem to be reason to adjust for the baseline, but only after a positive effect was observed, and this we did. An apparent effect could always be a consequence merely of the previous distributions of cancer existing in the affected areas. Nonetheless, the data were in the main presented stratified by area for postulated exposure level and by time period. (We see no point in the fuss Wing makes about cancer incidence data from 1975—the first of 5 preaccident years—that we concluded were undercounted. In the absence of detectable geographic bias our decision to include them, and Wing et al's decision to exclude them and adjust their results, are equally justifiable.)

There is neither mystery nor obfuscation in our presentation of the data. We are not sure we can say the same for Wing et al. They charge that we were constrained in our analyses in respect of emissions estimated by the judge's antecedent order. Certainly, we had no direct access to the records of the TMI Utility, but as far as we know, what was available was published. Of course, in using our models Wing et al. (*2*) operate under exactly the same constraint. We do not see that they find anything of note not reported by us and, indeed, they report rather fewer results than we do and in a less acessible manner.

Contrary to yet another allegation, our recommendation was firm [to the TMI Public Health Fund and also in print (*4,8*)] that a follow-up was needed, both to allow larger numbers of cases to accumulate in the aftermath of the accident and to collect individual level data on possible exposure and confounding.

In sum, then, Wing et al. (*2*) make assertions about what they take to be proven effects while we are cautious in accepting them as proven. It is a stretch to rate this difference, which your journal has given such prominence, as a controversy. Can it be said, in truth, that by going into contention Wing et al. have advanced the cause of the community or the environment? As we see it, they have done no more than muddy the waters.

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Response: Science, Public Health, and Objectivity: Research into the Accident at Three Mile Island

Although controversies over scientific findings are common, the topic of health effects of ionizing radiation has generated an exceptional amount of heat. Despite a century of research since Roentgen's discovery of X rays, fundamental disagreements exist over biophysical mechanisms, dose–response assumptions, analytical strategies, interspecies extrapolations, and the representativeness of studies of select human populations (*1–7*). In the United States, the last decade has seen revelations about human radiation experimentation (*8*) and a shift in responsibility for radiation health effects research from the Department of Energy to the Department of Health and Human Services, stimulated by concerns over secrecy and conflict of interest (*9,10*). These disagreements have been amplified by public and scientific debates over military, energy, and medical applications of nuclear technology (*11*).

As one of the best known technological failures of the nuclear era, the 1979 accident at the Three Mile Island nuclear power plant has generated its share of controversy, most recently in the pages of *Environmental Health Perspectives* (*12–16*). In his letter, Susser raises a number of important issues related to the context and logic of research on health effects from the 1979 nuclear accident at Three Mile Island (TMI) (*17*). We would like to follow his lead by giving some background regarding our involvement in the study of cancer incidence in the 10-mile area around TMI and also respond to some of his specific points regarding the logic and methods of the original study and our reanalysis.

Susser notes that he and his colleagues did not seek the opportunity to study cancer incidence around TMI, but were asked to investigate the accident "on behalf of the TMI Public Health Fund" (*17*). The Fund, financed by the nuclear industry as a result of a legal settlement, was governed by the U.S. District Court for the Middle District of Pennsylvania, which imposed requirements regarding the conduct of research and the review and approval of reports by attorneys for the industry (*18*). We do not suggest that this led Susser and colleagues to alter findings or purposefully construct research to support the industry. However, to the extent that all research is influenced by assumptions and beliefs from the framing of questions to the interpretation of evidence, the context of negotiation with industry representatives is important to understanding the research product.

Like Susser, we did not seek out funding for our reanalysis and, like the original research, our work was conducted in a context that is important to understanding the product. We were asked to review Susser and Hatch's data on cancer incidence by attorneys for approximately 2,000 plaintiffs in a class action suit that was before the same court that administered the TMI Public Health Fund. Civil suits may be a poor way to address public health problems; however, in our society, civil action has played an important role by bringing health issues (including asbestos, tobacco, air and water pollution) to public attention, and has provided some recourse to members of the public seeking protection from powerful industries.