available for less than 20% of chemicals in use today, and epidemiologic evidence is available for less than 1% (*1*). Animal studies for teratogenesis have been performed on less than 10% of the chemicals currently in commercial use (*2*). The question of how to manage that uncertainty is a political, social, and economic question, not a scientific one.

Abelson refers to the critics' favorite example, Bendectin, to illustrate the perniciousness of the litigation industry. While Bendectin is certainly not the second coming of thalidomide as was once claimed, at the time litigation began in 1977, there was a paucity of studies, animal or human, of its teratogenicity (*3*, pp. 677–678; *4*, p. 341). Contrary to Abelson's statement, statistically significant epidemiology studies do exist (*5*), although there is reason to believe that these studies do not identify true causal relationships. Even with the numerous studies that have been completed in the late 1970s and 1980s, there is residual doubt about the safety of Bendectin. The studies simply are not powerful enough to rule out increased risks of up to 100% for some classes of birth defects.

Bendectin was removed from the market in 1983, ostensibly because of the costs of litigation. But sales of Bendectin had declined precipitously after litigation began in 1977. Might some women have sensibly decided that the uncertainty about safety was more important than relief for a transient condition that resolves itself and in the vast majority of cases causes no lasting harm?

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No conscientious scientist would deny the difficulties posed by the process of risk assessment, but it is unfair to indict the whole process by lurid examples of how it can be distorted. Abelson himself seems guilty of distortion. He asserts, for example, that the human health risks of PCBs and dioxin have been greatly overstated and quotes assertions that effects in laboratory animals have been produced only after massive doses and that there is no convincing evidence that PCBs cause human illness at low doses (*1*). This may be true for cancer, but not for neurobehavioral toxicity. PCB levels in maternal diets are correlated with lowered IQ in offspring (*2*). As with lead and methylmercury, the margin of safety, if one exists, is disturbingly narrow in the U.S. population (*3*). Neurobehavioral disorders are hardly "phantom risks."

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I, along with Abelson, believe that there is a need for balance, and on some of the specific issues I might agree with him. But his editorial does a disservice to his cause. He is wrong from the beginning to charge that the public gets only a "one-sided portrayal of risks" and that environmental organizations are "well heeled" or "self-serving." Surely it is the chemical industry that is well heeled and self-serving in its public relations campaigns on these matters.

To say that hazards may be uncertain is hardly an argument. If we wait for a body count before acting against hazards, isn't that too late? In the cases of DDT, thalidomide, and HIV (human immunodeficiency virus)–tainted blood, we had reassurances that proved to be erroneous. Perhaps it is better to err on the side of prudence.

Yes, one can go overboard with caution, and judicious balance is required. Abelson's editorial does not offer that.

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Carcinogenicity Certification

In their letter of 13 August, "Determining carcinogenicity" (p. 814), H. Vainio, B. Armstrong, and L. Tomatis of the International Agency for Research on Cancer (IARC) refer to my letter of 4 June (p. 1408). Except for some oblique references to some of my comments, they appear reluctant to accept my challenge to discuss their method and purposes.

I used the word "certification" in my letter purposely and believe it to be apt. Whether the IARC management likes it or

not, their evaluations are used, almost verbatim, by government health regulators worldwide. A regulator ignoring an agent classified by the IARC as a "2A carcinogen" ("an agent probably causing cancer in the human") does so at his or her peril. Vainio *et al.* insist they are scientists merely making objective reviews and that "[r]eaders may . . . accept or reject them"; many a manufacturer only wishes this were the case. This advice may well apply to the casual reader who has the good fortune not to be a responsible government regulator.

It is my view that either these evaluations should be done according to the logical procedures of the toxicologist that take into account dose, route of exposure, and mechanisms of action or they should be objective evaluations of the scientific literature without arbitrary indices of potential hazard to humans. I would favor the latter approach and a return to the original format of the IARC monograph series.

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Excluded Gender?

Was it with a taste for irony that 1 week after reporting the resolution of a gender-bias lawsuit at the University of California, Berkeley, mathematics department (ScienceScope, 19 Mar., p. 1683), *Science* published a book review titled "Mathematical malaises" (26 Mar., p. 1928) restating the principle of the excluded middle?

A man of Seville is shaved by the Barber of Seville if and only if the man does not shave himself. Does the Barber shave himself?

The review says, "The statement can be neither true nor false. The conclusion: the barber cannot exist." Excluded middle or excluded gender? Clearly, the Barber is a woman!

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Volcanology in Europe

Paolo Gasparini's Perspective "Research on volcanic hazards in Europe" (18 June, p. 1759) provides an excellent updated summary of recent achievements of European research in the field of volcanic hazard mitigation. One should note, however, that the "laboratory volcanoes" project, initiated in 1992, is entirely financed by the Commission For the European Communi-