Letters

Cost of International Congresses

Recently I received the first circular of the 28th International Geological Congress, to be held in Washington, D.C., in 1989. Preregistration costs $250 (U.S.), and the cost of the technical excursions (probably the most informative and useful activity at geological congresses) ranges from between $300 and $2000. This means that the minimum cost of attending the congress and one excursion is $550, which is equivalent to approximately 1 month of my salary. If one takes into account the cost of air travel to and from Washington (approximately $500) and a 10-day stay in Washington (at least $1500), the total cost of attending the Congress is approximately $2550, or the equivalent of about 8 months of my salary. The total official allowance currently available for foreign travel at our institute is $500. These figures clearly indicate that many Venezuelan and Latin American geologists will not be able to attend the most important international meeting in their profession. And this situation is likely to worsen in the future.

Therefore I would like to urge the organizing committees of *international* meetings to take these considerations into account and to seek to provide facilities for Third World participants. Otherwise, international congresses will just be regional, rich-country meetings.

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Risk Assessment

Risk assessment may have its funny side, as noted by Daniel E. Koshland, Jr. (Editorial, 17 Apr., p. 241), but current mismanagement of risk by regulatory agencies is no laughing matter. Identifying, controlling, and setting priorities for risks within the areas that Congress has designated for federal activity has been extraordinarily inconsistent and unprotective. Koshland's reaction is not unlike that of most environmentalists, who have long worried that the practice of risk assessment to date has not improved health or advanced policy.

Unfortunately, the special Risk Assessment issue of *Science* (17 April) does not provide a fresh examination of issues, in large part because the authors selected have familiar and entrenched positions. Instead, it reinforces three persistent fallacies: First, that the only primary concern is cancer; second, that the data on exposure are reliable; and third, that bare calculations of health risk can be expected to guide human behavior.

Richard Wilson and E. A. C. Crouch (p. 267) have long lamented the failure of the public to rationalize their "risk portfolios," which suggests that the authors rather than the public are slow to learn that no one makes choices solely on the basis of simple equations or point estimates. Physicist-sociologists of risk need to note that some of the recent work in the study of economic behavior has provided a framework for a more complex analysis of consumer choice in the marketplace in place of simple comparisons of marginal benefit and cost. The proposal by Bruce N. Ames *et al.* (p. 271) for ranking risk of carcinogens, while elegant in structure, is not realistic or implementable. First, as a basis for the HERP (*H*uman *E*xposure dose/*R*odent *P*otency dose), it relies heavily on the assumption that there are reliable data on exposure. Assessment of exposure remains the weakest aspect of evaluating risks for regulatory purposes. The failure to require meaningful information on new chemicals and overreliance on models rather than on monitoring have resulted in a void of information for calculating human exposure. When this lack of data is factored into an equation already burdened by the range of unresolved issues and uncertainties of risk assessment (*1*), it is doubtful how much practical use the approach of Ames *et al.* can be. Second, any comprehensive system ranking risk should be capable of devolution to deal with risk control decisions at the margin. That is, it is important to be able to determine how to deal with, for instance, risks of dioxin from incinerator emissions in populations who smoke, eat certain foods, sunbathe, or otherwise engage in risky business. It is hard to know how to use the approach of Ames *et al.* for this critical assessment.

Finally, the approach of Ames *et al.* and much of the discussion of risk assessment in *Science* and elsewhere continues to confine our national debate to one end point—cancer risk. While evaluating the potential risks of chemicals as carcinogens is important, the human disease and dysfunction that can reasonably be associated with impacts of chemical exposure and environmental modifications are likely to be expressed in many other outcomes. The debate on risk assessment needs to be radically revised; it should start with an assessment of health status in the United States and then move to a consideration of which impairments of health might reasonably be associated with exposure to chemical agents, with the use of such techniques as biological markers to support proposed linkages (*2*). After such an analysis, rational ranking might occur.

This method would revise our current practice of going from the chemical by means of its toxicology to the estimation of health impact, the Environmental Protection Agency dogma of hazard identification, risk characterization, exposure assessment, and then to risk assessment, as explicated by Milton Russell and Michael Gruber (p. 286). Such an approach, while radically different from current science policy, could avoid some of the silliness of current regulatory practice, which provokes not only the amusement of scientists but also the disgust of the public as it observes continued failure to deal efficiently, at the source, with obviously significant environmental risks like lead, sulfur dioxide, radon, formaldehyde, and asbestos.

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Response: Silbergeld does not emphasize the importance of setting priorities in research and regulation, so that efforts to protect public health are not diverted from the most important issues. Since regulation of carcinogens has been based largely on results of rodent bioassays, it is necessary to recognize that about half of all chemicals tested at the maximum tolerated dose are carcinogens in rodents, whether the chemicals are natural or man-made. We believe that our attempts to provide a framework for setting priorities among human exposures to rodent carcinogens is of practical use. One contribution is to show that possible carcinogenic hazards to humans from current levels of pesticide residues or water pollution are likely to be of minimal concern *relative* to the background levels of natural substances, although one cannot say whether these natural exposures are likely to be of major or minor importance. Another contribution is to examine the many uncertainties in relying on animal cancer tests for human prediction given our current understanding of the mechanisms of carcinogenesis.

Silbergeld states that it is a fallacy to treat

cancer as "the only primary concern." We agree: it is also desirable to set priorities for chemicals that cause other toxicological problems. In both cases it is counterproductive to focus on quantities that are minute relative to their toxic level. Although our work focused on cancer, our methods are also relevant to other biological end points, including reproductive damage. Ranking priorities among possible teratogenic hazards is important, especially since fully one-third of the 2800 chemicals tested in laboratory animals have been shown to induce birth defects at maximum tolerated doses (*1*). Humans are ingesting enormous excesses of natural chemicals compared with man-made ones. For example, we ingest about 10,000 times more of nature's pesticides than man-made pesticide residues (*2*). Thus, one priority should be to estimate whether their toxicological effects might be in about the same proportion. There is no convincing evidence, either epidemiological or toxicological, to suggest that pollution is likely to be of great teratogenic interest relative to the background of natural chemicals.

Silbergeld's reference to dioxin pollution seems to imply that new incinerators should not be built until we know that dioxin poses no harm "to people who smoke, eat certain foods, sunbathe, or otherwise engage in risky business." Such an approach is impractical toxicologically and is an invitation to paralysis. To attempt to avoid all exposures that might cause some type of harm to someone under some circumstances ignores the background of natural hazards, the benefits of technology, and the hazardous side effects of the alternatives when some technology is eliminated. Is dioxin of importance at the tiny levels people are exposed to from incinerators when compared with the "risky business" people are already engaged in? Silbergeld's letter has prompted us to compare dioxin and alcohol in terms of the exposures to humans relative to the dose levels that have been shown to be teratogenic to mice in laboratory experiments. Unlike dioxin, alcohol is a known, and important, human teratogen. The teratogenic dose of alcohol for mice is more than a million times greater than the teratogenic dose of dioxin, similar to the difference in carcinogenic doses for the two chemicals. However, because the dose of alcohol in a bottle of beer is very high, drinking a daily beer would pose a possible teratogenic hazard about the equivalent of eating a daily kilogram of dirt contaminated with 1 part per billion of dioxin. Soil ingestion is considered by government regulatory agencies to be the main possible route of exposure (*3*). Given the information available concerning Silbergeld's example, our highest priority should be to warn people about the carcinogenic and teratogenic hazards of smoking and alcohol and of the carcinogenic hazards of sunbathing and to investigate the dietary imbalances that appear likely to be major causes of cancer.

Silbergeld laments the quality of exposure data. Yet our society has made an enormous effort to measure exposures to man-made pollutants and to regulate them at a large economic cost. We have turned up remarkably little of public health interest aside from occupational hazards. Additional measurements of parts per billion or per trillion of man-made pollutants do not seem likely to make a major contribution.

Silbergeld states that the public is concerned with more than "bare" calculations of health risks. That may be, but it is the job of scientists to provide the best estimates that they can about possible hazards. This includes putting worst-case estimates of hypothetical human risks in perspective. Our work suggests that traces of pollutants are likely to be of only minimal concern relative to the background of natural chemicals. Epidemiological evidence indicates that there is no epidemic of cancer (other than that due to smoking) or of birth defects.

The biological understanding of the causes of cancer and birth defects is progressing remarkably rapidly, considering the complexity of the problem. Silbergeld's suggestions are not likely to change the priorities of the many accomplished scientists working in this area.

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Response: The criticism by Silbergeld should primarily be addressed to the risk management procedures of the federal government and society in general. One possible reason that risk management has been inconsistent is a failure of regulatory agencies to properly inform the managers in the same agencies. For example, the Office of Drinking Water Standards of the Environmental Protection Agency, in a discussion of risks of organic hydrocarbons (*1*), omits any *mention* of chloroform, thereby withholding from the Administrator and from the public the instructive comparison with risks of trichloroethylene in our table 2 and on page 269 of our article.

We agree that no one makes choices solely on the basis of simple equations or point estimates and have said so in almost all of our writings, including the last paragraph of our article in *Science*. However, that is no excuse for not accurately determining the point estimate—and the uncertainty of that estimate—and for putting these numbers into perspective by comparison.

Public health officials, both in private and public, have in the last century emphasized acute effects that occur as a result of a short, high exposure. For these it is generally assumed that a low exposure means a risk close to zero. Risk assessors follow public demand in addressing the risk of cancer—a chronic effect arising from long exposure, often at lower levels. For these it is often assumed that there is linearity between response (probability of cancer) and dose. However, as we emphasized, the risk calculations for cancer can be a surrogate for other end points also.

Since for chronic effects risk is approximately dose times potency, dose information is vital. When it is available, a direct comparison such as, for example, for the radiation doses in our table 1, is less uncertain, and we find that people are helped by this. Again, however, we find that regulatory agencies and newspapers often omit this comparison, thereby failing to adequately inform the public of the risk and its meaning. This makes the risk assessment useless and any decision less well based than it need be.

We would also like to note, as kindly pointed out by Ernest V. Anderson, that in the discussion in our article of "Expression of risks" (p. 270, paragraph 2, line 24), an arithmetic error occurred: 0.0047% should have been 0.023%.

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Erratum: In the Research News article "Taking a closer look at AIDS virus relatives" by Jean L. Marx (19 June, p. 1523), Beatrice Hahn was incorrectly identified as a member of the Gallo–Wong-Staal group. Although Hahn collaborates with Gallo and Wong-Staal of the National Cancer Institute, she is in the Department of Medicine of the University of Alabama at Birmingham.