Letters

Saving Science Education

Elizabeth Culotta's article "Can science education be saved?" (News & Comment, 7 Dec., p. 1327) left me feeling angry and disgusted. For the last 20 years, it has been obvious that U.S. education, not only in math and science, but in most subjects, has become more and more dismal. Perhaps only in athletics have our children been exposed to competent instructors.

The problem is not what should we teach, or how should we teach, but rather who should do the teaching. No one would suggest that to teach children to play good tennis, we should use a person who has never held a racket; and if we want our children to play basketball, we would not hire a coach who has never played the game. Yet we have teachers who have never studied science or math teaching our children in these subjects.

Unless we approximately double the salaries we pay to teachers in our pre-college schools, we are lost. All of the proposed improvements and changes in curricula, methods of teaching, changes in books, and rearrangement of schools will not solve the basic problems. Such changes will help partly because changes, in themselves, stir things up and often cause improvements. But such changes can, at best, only improve the situation by small amounts.

When the Russians put up Sputnik, the research and development groups that were asked to meet the challenge did one simple thing: they doubled salaries to attract the best scientists and engineers. This did the trick. When we saw that we might have to enter World War II, the government and the industrial research labs that were set up to produce new weapons "drafted" the best people they could find. They appointed the best man or woman to lead these teams. No one told these people how to do research, development, or preproduction. I was lucky enough to be put into one of these teams. We had no rules and no advice on how to do what we had to do. Money was not a restraint. We did well.

We are now in an economic and intellectual war. We must produce an army of highly educated people in all fields of knowledge. But as I read the proposals about how to accomplish this, there is no mention of the basic problem of economics. We want cheap help to do something that even with the best intentions they cannot do. (I realize full well that a small number of teachers work simply for love.)

I would not, for a moment, suggest that all we need to do is "pour money" into the educational system. What I suggest is this: Decide what level of education and expertise we need in a teacher for a particular phase of education. Write the specs to be much higher than the minimum required. Set up procedures such as written and oral examinations and trial periods before a man or woman can qualify to be a teacher. A teacher of physics should know physics, not be chosen for the number of courses he or she has taken in "education." Finally, we should raise the salaries to get those who can meet our very strict requirements in each discipline.

Raising our standards and paying for the people who meet them would not result in any short-term solutions. It would take time to clear out the dead wood by retirement and pressure of competition, but we must start now. If we don't, the United States will continue to slip economically, technically, and socially.

JACOB RABINOW
6920 Selkirk Drive,
Bethesda, MD 20817

Biology Textbooks

A 7 December News briefing (p. 1335) reported that the Texas State Board of Education (TSBE) voted 11 to 4 in November "to approve a new generation of eight major [high school] biology texts [that] give extensive coverage to evolution and none to creationism." Two of the books are hardly new with respect to the treatment of evolution: they are the sixth editions of textbooks first published in 1963. *Biological Science: An Ecological Approach* (Green Version) and *Biological Science: A Molecular Approach* (Blue Version), were developed by the Biological Sciences Curriculum Study (BSCS), a nonprofit organization founded in 1958 with financial support from the National Science Foundation.

The first editions of these books, written by practicing scientists and teachers and published by the commercial sector after extensive field testing, treated evolution as the central organizing theme of biology. BSCS texts have remained solidly evolution-based in their subsequent editions. Both books had been excluded from Texas for the last two decades because of the TSBE's capitulation to creationist pressure, notwithstanding the books' consistently high marks from teachers and scientists.

Of the nine books submitted during the 1990 adoption process, the BSCS books were rated first and second by the science textbook committee, which proposed eight books for final adoption by TSBE. The creationists found all of the books objectionable, itself an indication of the progress the science education community is making in defense of scientific integrity. Textbook publishers, justly criticized in the past for their failure to place good science ahead of profits, should be commended for the stand they have taken in Texas. There is still considerable room for improvement in the quality of high school biology textbooks, but the Texas decision clearly is a step in the right direction.

JOSEPH D. MCINERNEY
Director,
Biological Sciences Curriculum Study,
Colorado College,
Colorado Springs, CO 80903

Carcinogens and Human Health: Part 3

Bruce N. Ames and Lois Swirsky Gold (Perspective, 31 Aug., p. 970) and Philip H. Abelson (Editorial, 21 Sept., p. 1357) raise questions about the interpretation and application of cancer information in regulating chemicals. They seem to suggest that (i) the rodent bioassay is misleading, (ii) risk assessment is too cautious, and (iii) these factors distort the regulatory process, creating public anxiety about phantom hazards while real risks are ignored. These suggestions involve a mix of science and politics. We wish to respond, point out alternative scientific perspectives, and discuss the appropriateness of the Environmental Protection Agency's (EPA's) approach.

In their statements about rodent bioassays, Ames and Gold and Abelson take a few often cited examples and generalize to other bioassays in ways that are contradicted by much of the accumulated scientific evidence. First, carcinogenicity is not necessarily a consequence of high-dose toxicity. Many bioassays have shown either toxicity without carcinogenicity or carcinogenicity without toxicity. D. G. Hoel *et al.* (*1*) have analyzed results from National Toxicology Program bioassays of 99 chemicals, of which 53 were positive. For only seven could target organ toxicity be the cause of all observed carcinogenic effects. Second, carcinogenicity has generally been confirmed at less than maximally tolerated doses. Of the 99 chemicals in the analysis by Hoel *et al.*, just three caused cancer at the highest dose only. Third, rodent bioassays are indicative of human cancer risks. Allen *et al.* (*2*) have analyzed results

of studies of 23 chemicals causing cancer in both rodents and humans. At high doses, rodent cancer incidences were good predictors of human cancer incidences. Because rodent carcinogenicity is not restricted to high doses, there is reason for concern about low-level human exposures.

Scientists in both industry and government have long recognized the need for careful interpretation of high-dose rodent bioassays, including consideration of supplemental information from other sources. They have improved the bioassay design to include, among other things, doses that do not cause substantial levels of toxicity. Rodent bioassays are critical in determining whether a chemical can cause cancer at some dose. Multiple-dose rodent bioassays are useful in distinguishing effects at high and low doses, as shown by the analysis of 2-acetylaminofluorene (2-AAF) by S. M. Cohen and L. B. Ellwein (Articles, 31 Aug., p. 1007).

The suggestion that risk assessment is too cautious, and that this caution no longer makes sense in view of the recent clarification of the mechanism of carcinogenesis in standard rodent bioassays, begs the question of whether a scientific consensus has emerged to support Ames's view of "the mechanism." Cancer comprises many diseases arising from a variety of mechanisms in rodents and humans, as Cohen and Ellwein illustrate with two mechanisms for mouse liver tumors induced by 2-AAF. High-dose toxicity is a mechanism for a few chemicals, but not the majority. For most chemicals, current data either support the likelihood of carcinogenic effects at low doses or are inadequate to rule them out.

The question is how to act when confronted with alternative risk projections that cannot be resolved with current data. EPA bases its risk assessments on health-conservative principles, properly so, because EPA has a responsibility to protect public health from the potentially damaging alternatives. Thus, when current data do not resolve the issue, EPA assessments employ the assumption basic to all toxicological evaluation that effects observed in animals may occur in humans and that effects observed at high doses may occur at low doses, albeit to a lesser extent.

That said, we point out that not all assumptions used in assessing risk are conservative in nature. For example, we generally have not studied potential synergistic interactions from exposures to multiple chemicals. We assume that risks are additive, although we know that for cases such as tobacco smoke and asbestos, the combined risk is much greater. As another example, there are almost no studies of cancer resulting from early life exposure. We assume that children are as sensitive as adults, although we know that for many pharmaceuticals, children are more sensitive than adults.

In response to the suggestions that these factors distort the regulatory process, creating public anxiety about phantom hazards while real risks are ignored, and that current levels of synthetic chemicals are of little importance compared to background levels of natural substances, we believe that substantially higher levels of synthetic chemicals might be found in food, water, and air if the current system of regulatory limits were not in place. This system is mandated under a number of laws enacted to reflect a long-standing public demand for action on controllable chemicals that present hazards to human health or the environment. To see the wisdom of this approach, one need only look at countries that have not controlled environmental contamination. We are far from convinced that Ames and Gold have made a persuasive case for allowing unrestricted addition of pesticides to the food supply.

At the same time, we agree with Ames and Gold that there are likely to be natural substances that warrant attention and testing. In the meantime, EPA cannot ignore its responsibility to evaluate and contol synthetic chemicals just because there may also exist natural risks that we cannot entirely eliminate. We note that the testing that Ames advocates would involve the animal tests that Abelson characterizes as "an obsolescent relic of the ignorance of past decades," since no one, including Ames and Gold and Abelson, has yet devised an acceptable alternative.

Finally, EPA's current and evolving approach to risk assessment and risk management is founded in scientific consensus on methods and peer review of practice. It provides a consistent and responsible way to evaluate scientific information and make informed judgments in an area of science that is relatively new and constantly changing. It allows the public to see what we are doing. This provides an opportunity for scientific scrutiny, which we welcome as a framework for evaluation and improvement. In the meantime, we cannot and should not be too quick to abandon approaches that, despite certain limitations, have served the public well.

VINCENT JAMES COGLIANO
WILLIAM H. FARLAND
PETER W. PREUSS
JEANETTE A. WILTSE
LORENZ R. RHOMBERG
CHAO W. CHEN
MARC J. MASS
STEPHEN NOSNOW
PAUL D. WHITE
JEAN C. PARKER
SUZANNE M. WUERTHELE
U.S. Environmental Protection Agency, Washington, DC 20460

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Response: The letter from scientists at the Environmental Protection Agency (EPA) raises many of the same issues about the use of bioassay data to estimate human risk that were discussed in earlier letters from Frederica Perera (21 Dec., p. 1644) and David P. Rall (4 Jan., p. 10) and in the article by Jean L. Marx (News & Comment, 9 Nov., p. 743). We have responded to these points in our comment on Marx's article (Letters, 14 Dec., p. 1498) and in our replies to Perera (Letters, 21 Dec., p. 1645) and to Rall (Letters, 4 Jan., p. 12), as well as in our earlier papers (*1*). For example, we have restated our view that mitogenesis markedly increases mutagenesis, that toxicity at high doses can cause mitogenesis, and that mitogenesis should not be ignored in models of carcinogenesis. We have explained that the analysis of D. G. Hoel *et al.* (*2*) cannot address the question of the role of mitogenesis in high dose animal cancer tests because mitogenesis was not measured. We have also suggested that research on mitogenesis be a high priority and that it can improve the regulatory process.

As evidence that "rodent bioassays are indicative of human cancer risks" the EPA letter discusses an analysis by B. C. Allen and colleagues (*3*) and concludes that "at high doses, rodent cancer incidences were good predictors of human cancer incidences." We disagree with this interpretation because the analysis of Allen *et al.* did not attempt to predict cancer incidences. Instead, it examined the rank order correlation between carcinogenic potencies estimated from animal bioassays and from epidemiological studies. Moreover, this analysis was based on 23 chemicals that caused tumors in *either* rodents or humans, not, as stated by the EPA letter, on chemicals that induced tumors in both rodents and humans; nine of the chemicals lacked sufficient evidence of carcinogenicity in either rodent tests or human epidemiological studies. The Allen paper was discussed by several toxicologist-sand statisticians, none of whom considered the work indicative of prediction of cancer *incidence* from animals to humans (*4*).

The EPA letter questions whether there is a scientific consensus to support the view that effects of mitogenesis at high doses can

be unique to high doses. It also states that risk assessment is an "area of science that is relatively new and constantly changing . . ." and that current practice "provides an opportunity for scientific scrutiny, which we welcome as a framework for evaluation and improvement." Our papers should be seen within the context of that scientific scrutiny and evaluation. We recognize that current regulatory procedures are grounded in peer review of methods and practice. Our view is that the consensus that developed in the 1970s was based on assumptions that recent evidence suggests are wrong. The high proportion of carcinogens among chemicals tested at the maximum tolerated dose (MTD) emphasizes the importance of understanding cancer mechanisms in order to determine the relevance of rodent cancer test results for low-dose human exposures. A list of rodent carcinogens is not enough.

The EPA letter states that, when confronted with alternative risk projections that current data do not resolve, "EPA assessments employ the assumption basic to all toxicological evaluation that effects observed in animals may occur in humans and that effects observed at high doses may occur at low doses, albeit to a lesser extent." The main rule in toxicology, however, is that "the dose makes the poison": at some level, every chemical becomes toxic, but there are safe levels below that. A consensus developed in the 1970s that we should treat carcinogens differently, that we should assume that even very low doses might cause cancer; this consensus was based on the precedent of radiation, which is both a mutagen and a carcinogen; radiation gave credence to the idea that there could be effects of chemicals even at low doses although we lacked the methods for measuring such effects. This idea evolved because it was expected that (i) only a small proportion of chemicals would have carcinogenic potential and (ii) testing at high dose would not produce a carcinogenic effect unique to the high dose. In our papers and replies to letters in *Science*, we have discussed in detail the accumulating evidence from a variety of disciplines suggesting these assumptions are wrong and therefore that it is time to re-evaluate them.

The risk assessments on which regulations are based are not scientifically justified. Testing chemicals for carcinogenicity at near toxic doses in rodents does not provide enough information to predict the numbers of human cancers that might occur at low-dose exposures. We have discussed the importance of ranking possible carcinogenic hazards and the uncertainties in risk assessments (*5*). Therefore, the public might be better served if EPA were to present its risk assessments as comparisons to its estimates of risks from cups of coffee, beers, and so forth, given the enormous natural background of potential rodent carcinogens.

The EPA letter points out that not all assumptions used in their risk assessments are conservative, for example, the potential synergistic interactions among chemicals. We agree that some interactions can potentiate carcinogenesis; however, interactions can also be inhibitory, and at low doses defenses in humans are usually inducible. The main conservative assumption is that the effects of mitogenesis at high doses can be ignored in low dose extrapolations.

With respect to regulatory policy, the EPA letter states that if current regulatory limits were not in place then higher levels of synthetic chemicals might be found in air, water, and food. Our papers do not argue to discontinue regulation nor, as EPA misrepresents us, to allow "unrestricted additions of pesticides to the food supply." Regulation involves trade-offs, and the best science is necessary so that regulation does not become counterproductive. We have discussed these important trade-offs (*1*).

BRUCE N. AMES
Department of Molecular and Cell Biology, University of California, Berkeley, CA 94720
LOIS SWIRSKY GOLD
Carcinogenesis Potency Database, Lawrence Berkeley Laboratory, Berkeley, CA 94720

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Erratum: In Marcia Barinaga's article "Was Paul Biddle too tough on Stanford?" (News & Comment, 11 Jan., p. 157), the photograph of Paul Biddle on page 157 should have been credited to Damian Marhefka.

Erratum: In the 21 December response by Bruce N. Ames and Lois S. Gold (Letters, p. 1645) to the letter by Frederica P. Perera (p. 1644), the last sentence of the third paragraph in column three should have read, "Both natural arsenic in water and natural radon in indoor air are present at high levels at some locations and were long neglected, while major efforts were put into miniscule amounts of industrial pollutants." In the 4 January response by Bruce N. Ames and Lois S. Gold (Letters, p. 12) to the letter by David P. Rall (p. 10), reference 11 should have read, "D. G. Hoel, J. K. Haseman, M. D. Hogan, J. Huff, E. E. McConnell, *Carcinogenesis* **9**, 2045 (1988)"; reference 14 should have read, "E. Marshall, *Science* **250**, 900 (1990); R. Doll, *Eur. J. Cancer* **26**, 500 (1990); C. Hill, E. Benhamm, F. Doyon, *Lancet* **336**, 1262 (1990); S. Freni, *ibid.*, p. 1263."