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# LETTERS

## NIH Support for Graduate Students

I believe the National Institutes of Health move to cut back on tuition aid to graduate schools (ScienceScope, 23 July, p. 415) is a step in the right direction. The ultimate goal should be to eliminate wholesale tuition waivers and stipends as a major incentive for entering graduate school. Such a course of action would be in the best interests of future scientists and perhaps the future of science. The present system tends to shield students from the prospects (or lack thereof) of eventual employment. In other words, it encourages students to enter graduate school for the wrong reasons. The supply of scientists should be driven by the demand for scientists, not the demand for graduate students.

Some people would argue that ending subsidies to graduate science education would lead to a shortage of scientists. I disagree. What it would lead to is a shortage of scientists at cheap wages—certainly not a catastrophe for anyone. After all, no one seems to worry about a shortage of doctors or lawyers because they have to finance their own education!

Graduate education in science should return to being what it purports to be: job training, not the job itself.


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## NCRR Funding Mechanisms

Donald D. Brown and *Science* readers should know that several of the suggestions to improve funding for the National Institutes of Health (NIH) that he makes in his Policy Forum "NIH funding mechanisms need appraisal" (2 July, p. 16) are currently supported by the National Center for Research Resources (NCRR).

His suggestion that "group grants should fund . . . the purchase of expensive, multiuser equipment that is shared by multiple, individually funded investigators in a single location" is an exact description of NCRR's Shared Instrumentation Grants, which fund equipment costing between $100,000 and $400,000 that is used by three or more Public Health Service–funded investigators.

His comment about the need for regional centers calls for transgenic mouse facilities, such as the one proposed in the recently published NCRR Request for Applications; cell culture centers, such as those funded through NCRR's Biological Models and Materials Research Program; animal colonies, such as those supported by NCRR's Comparative Medicine Program; and synchrotrons and other expensive equipment, such as those in NCRR's Biomedical Research Technology Centers (BRTCs). These are all evaluated at least every 5 years, as Brown suggests.

Brown correctly cites the high costs of clinical trials. NCRR is combating these costs through a network of 72 clinical centers around the country that are available to researchers with approved protocols. By paying for a dedicated clinical research unit, including dieticians, nurses, and biostatisticians, NCRR is reducing the costs of the individual research grants while ensuring that patients and researchers have the best care available.

In short, NCRR's programs are designed to be cost-effective, multidisciplinary approaches to biomedical research. Readers who want more information about NCRR's resources and how to use them can write to the Office of Science and Health Reports, NCRR, 5333 Westbard Avenue, Room 10A15, Bethesda, MD 20892.


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## Environmental Hazards: Real or Exaggerated?

Philip H. Abelson's 23 July editorial "Toxic terror; phantom risks" (p. 407) rightly decries the exaggeration and hysteria over toxic risk. Unfortunately, in taking the side of the strident critics, he falls into the familiar trap of painting in black and white.

Abelson quickly slides by the toxic agents that have had a demonstrably adverse effect on human health. Tobacco is the most notorious; but asbestos, diethylstilbesterol (DES), MER/29, thalidomide, and lead come quickly to mind. Asbestos workers and their families, for example, are surely justified in their concerns about the hazards of occupational toxins.

Abelson also does not confront the uncertainty endemic to toxicity assessments. Toxicologic evidence of carcinogenicity is

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