Letters

Soviet Role in SAGE

I was impressed and pleased with how quickly *Science* picked up on the important implications of preliminary observations from the Soviet-American Gallium Experiment (SAGE) ("Solar neutrino deficit confirmed?", Research News, 29 June, p. 1607). If a much smaller than expected number of solar neutrinos are detected in this experiment it will show, among other things, that neutrino masses are nonzero and that there is mixing between neutrino species.

The article did not, however, appropriately reflect that SAGE is predominantly a Soviet experiment. Soviet scientists have been involved in this area for almost 20 years. Credit for the idea of the use of gallium as a solar neutrino detector belongs to V. A. Kuzmin (*1*). A group of dedicated researchers from the Institute for Nuclear Research in Moscow has created an effective underground laboratory at the end of a 4-kilometer tunnel into Mount Andyrchi in the Caucasus Mountains. The Soviet scientists obtained the gallium required to carry out the experiment (60 tons). The American side of SAGE (Los Alamos National Laboratory, the University of Pennsylvania, Princeton University, and Louisiana State University) joined the experiment in 1986, providing some much needed electronics, detectors, and small computing systems. All of these elements are essential to carry out an experiment as difficult and subtle as SAGE. Groups in both countries have a role in the scientific decisions.

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Asbestos Policy

Bernard Goldstein's letter (13 July, p. 111) about Robert Pool's article "Struggling to do science for society" (News & Comment, 11 May, p. 672) underscores the complexity and the differing perceptions of environmental issues.

Although the legal process may now have supremacy over scientific knowledge "as the primary driving force in regulatory activities" in the United States, these exercises are not carried out in a vacuum. If our legislators propose new laws that influence regulatory policy, they are able to do so because they support their laws with some form of data. They are advised by persons who are held to have expertise in a particular area. Sometimes this advice is not good.

We believe the initiative to form policies should be taken by the federal agencies with the committed support of their scientists. Only where there is inept or timid leadership do these roles fall, by virtue of default, outside of the scientific spheres. Goldstein's letter serves to emphasize the failure of the leadership at the Environmental Protection Agency (EPA) to communicate the uncertainties regarding asbestos fiber inhalation in buildings and perceived risk to passive occupants.

Pool's article accurately reflected the impressions of many of us in the asbestos field that the federal regulatory agencies have not appreciated the differing biological potentials exhibited by the various asbestos fiber types. These differences have not been reflected in their public statements, current policies, or regulatory statutes.

We are gratified to learn that Goldstein and Jack Moore "clearly understood that there is a relative difference in the toxicity of the different fiber types." Following the advice of Goldstein, we have checked our letter and briefing records and find that one of us (M.R.) could not convince Don R. Clay (former director of EPA's Office of Toxic Substances) of these fiber distinctions in letters of 17 December 1981 and 21 April 1982, could not convince former EPA Administrator William D. Ruckelshaus of these distinctions at a 9 July 1984 briefing, and could not convince Goldstein and Moore of these distinctions in a letter of 28 February 1985. Another of us (A.M.L.) failed in his attempt to introduce fiber type as a factor to be considered in building inspection in several of the school guidance documents. Either Goldstein is practicing a form of federal revisionism, or the cadre of EPA's middle-management did not follow the directions of its leadership. It appears that EPA officials still do not accept the opinions expressed by Goldstein in 1990, as EPA specialist Tom Tillman was quoted in August 1990 as saying, "we don't differentiate between types of asbestos" (*1*).

We are distressed that Goldstein appears to fall victim to the oversimplification that he condemns. He states that "sweeping generalities" and "superficiality" in reporting science has led to an inappropriate conclusion regarding asbestos in buildings. His statement that he "did not fall for the obfuscation promoted by the asbestos industry that there is a form of asbestos that poses no health risk" is exactly this kind of statement. We have heard, on many occasions, representatives of the chrysotile-producing industry from Canada state that risk of disease from asbestos is a function of the nature of the disease itself, fiber type, dose, state of aggregation (fiber length and diameter), and social and other host factors. None of us has ever heard anyone from industry suggest that inhalation of chrysotile fiber at levels which existed in the workplace in the past has not been associated with disease.

The regulatory agencies should move forward in lockstep with the best current science. Strong, knowledgeable, leadership must be willing to take an unpopular stand, based on science, in the face of pressures from vested interest groups. As Goldstein states, we should not fall prey to obfuscation (which may be promoted by *any* vested interest group—industry holds no monopoly on potential for villainy). EPA must be supported for its efforts to formulate policy on the basis of science, and we applaud Robert Pool, who fairly communicated the concerns by scientists about current policy decisions.

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Young Scientists and the Future

Joseph Palca's article "Young investigators at risk" (News & Comment, 27 July, p. 351) deserves high marks for making clear just how difficult it is to start up an independent research laboratory. However, the article seems to suggest, that unless a trainee (particularly one finishing a postdoctoral fellowship) follows the career path of his or her mentor, the training has essentially been wasted.

The fact is that very good scientists, "even the very best," can find rewarding careers in the private sector or in branches of the public sector other than those that rely solely on federal grants. Simple arithmetic shows that training in a top laboratory at a top institution, combined with the requisite number of high-quality publications, does

not by itself ensure anyone a position similar to that of his or her mentor. Most top laboratories graduate two or three postdoctoral fellows a year. Over a 10-year period, top laboratory X could produce 20 to 30 assistant professors. Multiply that by the large number of top laboratories in the country, and it becomes clear that even in good times not all these young investigators and their research programs can be absorbed into the National Institutes of Health system. Yet common sense does not prevail, and many heads of laboratories are content to propagate the notion that "If they fail to obtain funding [at the assistant professor level] . . . all of the 10 years you spent in training them is down the drain" (*1*).

By equating inability to set up an independent laboratory with some dark unfathomable fate (being "at risk"), the article is doing its part to propagate this myth. The corollary expressed in the article that bright young people may not go into science if it is openly discussed at the outset how difficult it is to follow the paths of their mentors is misguided. Universities are here to prepare young people to become productive citizens in the larger world outside the academic one. University departments do not exist simply to expand one another's ranks.

One need only look to the pages of *Science* itself for proof that exciting research occurs in the private sector. The widely expressed sentiment that choices other than academia are second rate could, by its small-mindedness, be making its own contribution to turning young people off to the study of science.

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Palca highlights the difficulties experienced by outstanding young investigators in winning National Institutes of Health (NIH) grants and suggests that they may leave their frustrations behind for greener career pastures. While I hope Congress will respond by increasing federal support for research, I fear that graduate and postgraduate students may react to such articles by fleeing research careers in even greater numbers. A more balanced view would acknowledge that it may be equally hard to become independent in any other field. Doesn't it take several years, at least, to establish a successful clinical practice or small business; to be voted into office; to produce a movie or play; to write and publish a book, or to hold a one-man show?

Young investigators still have many options for survival in academia. Science has its "starving artists": courageous, talented, and dedicated people who work sometimes without academic rank (and sometimes even without salary), who are driven by a need to discover and for whom quitting is out of the question. Science, too, has its "thousand points of light": generous, benevolent senior investigators whose bench space and supplies provide a haven for the younger ones until they can become independent. Private individuals and foundations support pilot projects that are promising and contain genuinely new ideas, but are not yet ripe for NIH funding. Finally, scientific societies offer a variety of activities to sustain and nurture their members.

Yes, the funding crisis is real. But to be effective lobbyists, we must communicate

that our work is so exciting and urgent that we will not allow ourselves to be deterred at any cost.

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The Cellular Basis of Memory

Marcia Barinaga's Research News article (29 June, p. 1603) reporting on the recent results presented at the Cold Spring Harbor meeting on the cellular basis of memory was well written and accurate. However, we were surprised that the article contained no hint of the controversy that exists over the results of quantal analysis of hippocampal synaptic enhancement.

In a study of 33 synaptic connections obtained with a minimal stimulation technique similar to that used by Robert Malinow and Richard Tsien (*1*) as well as simultaneously recorded cell pairs, we obtained no evidence for an increase in the number of quantal components of the excitatory postsynaptic potential (EPSP). Rather, we found a significant increase in quantal size (*2*).

We see several possible sources for the apparent discrepancy between our results and those of Malinow and Tsien (*1*).

1) The increase in estimated quantal release observed by Malinow and Tsien accounts for only about half of the EPSP growth. Because their method of analysis is known to overestimate quantal release, we think it is likely that increased quantal size is actually the *predominant* effect.

2) We have shown that the method of data selection employed by Malinow and Tsien could bias the results toward increased quantal release, although this probably does not account for the entire discrepancy.

3) The conclusions of Malinow and Tsien are based on the assumption of uniformity of quantal size. For purposes of illustration, consider the extreme case in which *some* sites at which quanta are released contain *no* receptors. Addition of receptors (a postsynaptic modification) would show up in their analysis as increased quantal release.

4) Our experiments were carried out on adult tissue rather than tissue from animals in which the hippocampus is not yet mature and in which substantial synaptogenesis and structural rearrangement are known to be in progress. This difference could account for much of the discrepancy. It would hardly be surprising if the same patterns of activity that lead to an increase in the strength of responses at existing release sites in adult brains lead to both modification of existing sites and to formation of new ones in neonates.

We think a more balanced treatment of this controversial issue would have better served the scientific community.

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Erratum: In the Perspective "Too many rodent carcinogens: Mitogenesis increases mutagenesis" by Bruce N. Ames and Lois Swirsky Gold (31 Aug., p. 970), the last paragraph on page 970 (continuing on page 971) was incorrectly printed. It should have read, "One major group of natural chemicals in the human diet are the chemicals that plants produce to defend themselves, the natural pesticides (*4*). We calculate that 99.9% (by weight) of the pesticides in our diet are natural. Few natural pesticides have been tested in at least one rodent species, and again about *half* (27/52) are rodent carcinogens. These 27 occur commonly in plant foods (*10*). The human diet contains thousands of natural pesticides, and we estimate that the average intake is about 1500 mg per person per day (*4*). This compares to a total of 0.09 mg per person per day of residues of about 100 synthetic pesticides (*4*). In addition, of the mold toxins tested at the MTD (including aflatoxin), 11 out of 16 are rodent carcinogens." Also, in paragraph 3 on page 970, "47,000 8-hydroxydeoxyguanosines per cell" should have been "90,000" per cell.