little money—just $2 million a year for the 6000-plus researchers. And the system is not as selective as Pačes and Viklický make it sound—through resubmissions of slightly revised grant proposals, many researchers whose proposals failed in 1991 succeeded in 1992 and 1993. It is my understanding that, in making his statement as strong as he did, Pečenka pushed his description of the situation to extremes in order to suggest that Czech science would benefit if the system were more selective.—***Steven Dickman***

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Misconduct in Science

Howard K. Schachman, in his 9 July Policy Forum (p. 148), presents his view of how federal agencies should define misconduct in science and takes issue with some points I made in my earlier Policy Forum (29 Jan., p. 584). In some significant ways his positions also differ from those taken by the National Academy of Sciences report (*1*), which my paper was addressing.

Schachman objects to broad phrases like "other serious deviation from accepted practices" that occur in the federal agency definitions. He says that Congress in the Health Research Extension Act of 1985 prescribed a limited definition in terms of "fraud" that excluded such broad phrases. In fact, the legislative history of that Act shows that Congress used terms like "fraud" and "misconduct" without construing them narrowly. Since the publication of federal agency definitions, Congress has never indicated that they were too broad. On the contrary, in 1993 two congressional reports attached to agency authorization acts have supported the "other serious deviation" language.

Every federal agency has the intrinsic authority to issue regulations that protect the programs it funds. This authority does not derive from the 1985 Act, as Schachman seems to assume, and that Act does not limit the definitions that any agency may publish in its regulations. Moreover, the Act has no relevance to the National Science Foundation (NSF).

My Policy Forum offered criteria for judging a practice to be misconduct in science in terms of whether it violates the ethical standards of the scientific community and does serious damage to the processes of science. Schachman does not discuss these criteria or offer his own. He also does not appear to take into account the safeguards against overly broad interpretations of the definition that are provided by the NSF regulation and its appeal to community standards. He instead appeals to a general fear of government by referring vaguely to government suppression of science in other countries.

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References

1. *Responsible Science: Ensuring the Integrity of the Research Process* (National Academy Press, Washington, DC, 1992), vol. 1.

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Schachman addresses the question of "What is misconduct in science?" As he points out, the definition proposed by the National Academy of Sciences/National Academy of Engineering/Institute of Medicine panel in 1992 restricted it to "fabrication, falsification, or plagiarism, in proposing, performing, or reporting research." The arguments have centered on whether additional phrases like "other serious deviation from accepted practices" used in current agency definitions make them too expansive and vague. Schachman and others have expressed their concerns from the

perspective of scientists interested in the freedom of scientific inquiry. While this may be the central concern of the scientific community, it falls short of the necessary and legitimate concern of a public agency using public moneys.

For example, Schachman contends that "governmental intervention" is never appropriate for "concerns regarding errors in collecting and interpreting data, incompetence, poor laboratory procedures, selection of data, authorship practices, and multiple publications." This contention can only be defended if one believes that public funds for academic research occupy a special, privileged position far different from public funds for defense, health care, enterprise zones, welfare or any other legal use of such funds. It is reasonable to ask that scientists not be punished for innocent mistakes, but it is not reasonable to expect that grossly negligent scientific practices supported by government funding are outside the realm of government intervention.

Thus, I believe that the current debate is too limited in scope. The phrase that is of principle concern to Schachman—"other serious deviation from accepted practices"—is a significant concession to the scientific community. It essentially invites that community to establish a form of "common law" governing the behavior of its members in the legitimate use of public funds. It would be well for the scientific community to accept that invitation and work on this broader issue rather than endlessly debating the more limited issue. Our failure to do so might mean that it will be addressed and settled by others—perhaps in unfriendly congressional hearing rooms.

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Proton Decay Interest

Charles Mann and Robert Crease briefly discuss the ICARUS proton decay detector in their 3 September Research News article (p. 1276). They give the impression that interest in proton decay is now lacking and write that the ICARUS detector is "on stand-by." This is not correct. There is now very great interest in the proton decay mode that produces a strange particle and an anti-neutrino according to a prediction of the Supersymmetric Grand Unified Theory. Observation of this decay would also imply the existence of massive supersymmetric particles that might be detected by the Large Hadron Collider at CERN (the European Organization for Nuclear Research), thus indicating that accelerator and nonaccelerator experiments provide complementary information about elementary particles. The ICARUS detector is designed to particularly detect this and many other modes of proton decay.

During the past few years, the ICARUS detector concept has been tested at CERN, where we have recorded events in the form of "electronic pictures" that are as clear as bubble chamber pictures. A 5000-ton detector is in the final stage of design, and the ICARUS team hopes to install this module in the next 5 years.

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Caltech Biology in Perspective

In their letter of 17 September (p. 1505), Robert L. Sinsheimer and Norman H. Horowitz criticize my book, *The Molecular Vision of Life* (Oxford University Press, New York, 1993), as a distortion, and Robert