sustained in all nuclear-energy operations. Third, the problem of radioactive-waste management must be addressed in a way that is both technically adequate and politically acceptable. Fourth, tight barriers must be in place against leakage of nuclear-explosive materials from nuclear-energy systems into the hands of terrorists or proliferant nations. It is possible that these formidable conditions can be met. We ought to be trying. But they are not met as of today. Until they are, the role that nuclear energy will be able to play in reducing carbon emissions from the energy sector will remain uncertain.

JOHN P. HOLDREN[1] AND PETER H. RAVEN[2]
[1]John F. Kennedy School of Government, Harvard University, 79 John F. Kennedy Street, Cambridge, MA 02138, USA. [2]Missouri Botanical Garden, St. Louis, MO 63166, USA.

Defining Scientific Misconduct

IN HIS EDITORIAL "NEXT STEPS IN THE SCHÖN affair" (18 Oct., p. 495), Donald Kennedy appropriately discusses the need for further actions, by many parties, to try to prevent future instances of misconduct like the one described in our report (*1*) on the Schön affair. In his conception of the issues, Kennedy sees scientific misconduct and professional responsibility (the coauthor responsibility issue discussed in our report) as closely linked—"a distinction without a difference."

The issue is not so simple. As used in our report, the term "scientific misconduct" carries a precise definition, contained in the U.S. Federal Policy on Research Misconduct (*2*). Although "misconduct" may sound mild, it entails the most egregious offenses that can be committed in scientific research: fabrication, falsification, or plagiarism that is intentional or reckless. However, as is no doubt clear to all readers of our report, the committee strongly believes that even when coauthors are not guilty of scientific misconduct, they have a broader responsibility, and we welcome the ongoing discussion of this issue. We only wish to point out that there are important distinctions to be made between scientific misconduct and our broader responsibilities as scientists.

M. R. BEASLEY,[1] S. DATTA,[2] H. KOGELNIK,[3] H. KROEMER,[4] D. MONROE[5]
[1]Department of Applied Physics, Stanford University, McCullough Building, Room 362, Stanford, CA 94305–4045, USA. [2]School of Electrical and Computer Engineering, Purdue University, 1285 Electrical Engineering Building, West Lafayette, IN 47907, USA. [3]Bell Laboratories, Lucent Technologies, 600 Mountain Avenue, Murray Hill, NJ 07974, USA. [4]Department of Electrical and Computer Engineering, University of California at Santa Barbara, Santa Barbara, CA 93106, USA. [5]Agere Systems, 4 Connell Drive, Room 4E-707B, Berkeley Heights, NJ 07922, USA.

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1. See www.lucent.com/news_events/researchreview.html.
2. See www.ostp.gov/html/001207_3.html.

Should Coauthors Share Liability?

ACCORDING TO DONALD KENNEDY'S Editorial "Next steps in the Schön affair" (18 Oct., p. 495), "The hard question is this: If the benefits of authorship are enjoyed jointly and severally by all the authors, shouldn't the liability be shared in the same way?"

Actually, this is not a hard question at all; the answer is "no."

First, if the coauthors signed their names to a fraudulent paper in the sincere belief that it reported honest data, then they were the victims of a fraud, not its perpetrators. Second, the coauthors were not the only ones who sought to enjoy benefits from these fraudulent publications. Among others, editors who publish "high-impact" papers in their journals also get a share of the glory and advancement in their careers.

However, none of these beneficiaries acted unethically; they all believed the data to be honest, and they were all victims of a squalid deception. For this reason, none of them deserves to be stoned by the scientific community. Enough handwringing—resist the temptation to blame the victim.

MICHAEL PHILLIPS
Menssana Research, 1 Horizon Road, Suite 1415, Fort Lee, NJ 07024, USA, and New York Medical College, Valhalla, NY, USA. E-mail: menssana@bellatlantic.net

A Proposal for Transparency

THERE IS AN EASY, CHEAP, AND RESPONSIBLE way to link credit for published papers with responsibility for their contents, and I am surprised that it was not mentioned in Donald Kennedy's otherwise excellent editorial on the Schön affair ("Next steps in the Schön affair," 18 Oct., p. 495). This is for journals to publish for the reader a brief list of what the authors agreed had been their contributions—what part of the work reported each author actually did (*1*). My colleagues and I proposed this and devised a successful system that has since become common practice among the big medical journals, which were long ago forced to deal with irresponsible authorship and the phenomenon of multiple fabricated studies and coauthors disappearing whenever problems were raised (*2*). This system was devised to link credit to responsibility, transparency being a goal of science. The Beasley committee, in investigating Schön, would have had a far easier time, and been more convincing in their assessment, had they been able to see in print what Schön and his colleagues had asserted they had actually contributed to the work at the times when their joint papers were submitted. Then the committee, like the readers, would have plainly seen that the coauthors did nothing. *Nature* agreed to adopt this rule, but to make it voluntary. Because we all like credit without accountability, needless to say, almost no authors took up *Nature*'s offer. I now hope that both *Science* and *Nature* will change to this system, if only to remind authors that a scientific publication is, in Joshua Lederberg's words "an inscription under oath, a testimony" (*3*).

DRUMMOND RENNIE*
Univerisity of California, San Francisco, San Francisco, CA 94118, USA.
*Deputy Editor, *JAMA*.

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3. J. Lederberg, *Scientist*, 8 February 1993, p. 10.

Response

WHAT MY EDITORIAL ACTUALLY SAID WAS, "That sounds like a distinction without a difference." What I had in mind was that to most people, under most circumstances, scientific misconduct and professional responsibility really are linked. For example, most institutional rules hold failures of professional responsibility by scientists to be sanctionable. Under such rules, research misconduct would surely lead to sanctions, and has. I would agree with Beasley and his colleagues that although all research misconduct entails failure of professional responsibility, not all failures of professional responsibility constitute research misconduct.

It is easy to agree with Phillips that victims shouldn't be blamed. I find it harder to accept a world in which coauthors enjoy benefits but never have to accept risks. Rennie offers an ingenious way to finesse that problem, but it would make for complex (and costly) presentation in the journals.

DONALD KENNEDY

Nature Versus Nurture Redux

IN PATRICK BATESON'S REVIEW OF *THE Blank Slate* ("The corpse of a wearisome debate," Books *et al.*, 27 Sept., p. 2212), he attacks the book's author, Steven Pinker, on three levels. First, Bateson argues that Pinker has set up a caricature of

the current social sciences. In particular, Bateson argues that most social scientists fully acknowledge the importance of biology and, thus, there are no more "blank slaters." Second, Bateson suggests that the focus of Pinker's treatise is the battle between the social and natural sciences, turf that has been so well trodden that it is worn out. Third, Bateson seems to feel that Pinker's views are naïve with respect to issues of genetics, environmental influences, and evolutionary theories. Unfortunately, Bateson has created something of a caricature of Pinker's views. This is a shame, as both are terrific, clear-headed scientists, with many overlapping areas of interest.

It is certainly true that within the halls of academia, many social scientists have come to appreciate the role of biological constraints. This is by no means a consensus view, however. Many educators believe that a child's knowledge of the world is constructed as a result of experience and that children are like blobs of clay, ready to be molded by society. I have run up against extraordinary roadblocks in trying to suggest to educators that they might tap a child's innate abilities for simple mathematical abilities—they argue that math is purely a cultural construct.

In terms of Pinker's audience, I also believe that Bateson has this wrong. Although Pinker certainly does wish to show how the social and natural sciences need not conflict, he has a much larger audience in mind: the nonacademic public who fear biology. He is right to have this target, and I personally thank him for taking it on, as it is important to show why biological accounts of human behavior are neither misguided nor nefarious.

Finally, Bateson considers Pinker to be a naïve nativist and a somewhat slipshod evolutionary theorist. Again, I think that at the core, Pinker and Bateson agree on the details of development and evolution. To make this clear, simply consider the lines of research that have made both scientists famous: Bateson's imprinting work on chicks and Pinker's studies of child language acquisition. Both argue that chick and child have developed mechanisms that constrain learning. Chicks imprint on the first moving object, but henlike objects have more power than other objects. Children have similar kinds of constraints on word acquisition. Although children hear the sounds of cars, dogs, and coffee machines, these are not the sounds emanating from their mouths. Pinker doesn't believe that the environment is irrelevant. He also doesn't believe that genetics tell the entire story. Both Pinker and Bateson are interested in how systems develop and, ultimately, in understanding how different factors contribute to both universal aspects of human nature and cultural variation.

There are things to disagree with in Pinker's book, but I don't believe that Bateson focused on the correct ones.

MARC HAUSER

Department of Psychology, Primate Cognitive Neuroscience Lab, Program in Neurosciences, Harvard University, 33 Kirkland Street, 10th floor, William James Hall, Cambridge, MA 02138, USA.

Letters to the Editor

Letters (~300 words) discuss material published in *Science* in the previous 6 months or issues of general interest. They can be submitted by e-mail (science_letters@aaas.org), the Web (www.letter2science.org), or regular mail (1200 New York Ave., NW, Washington, DC 20005, USA). Letters are not acknowledged upon receipt, nor are authors generally consulted before publication. Whether published in full or in part, letters are subject to editing for clarity and space.