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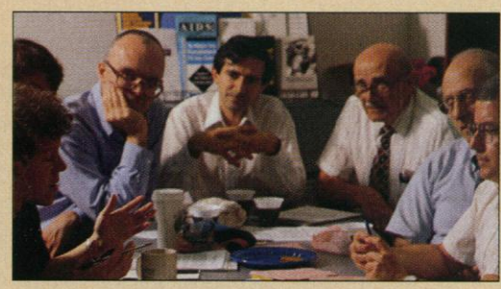
LETTERS

Conduct in Science: In the arena

The letters in response to the special section on Conduct in Science (23 June, pp. 1705–1718) were an interesting crop. Many deal with the case of Sarvamangala Devi and reflect the range of perspectives from which this matter is seen. We hear below from most of the major players in that case. Others discuss ethics courses, and Carl Djerassi describes his "science-in-fiction" technique of examining the conduct of scientists. Letters weren't the only vehicle through which the scientific community expressed itself on essential issues of conduct in science. Science's World Wide Web project "Science Conduct Online" also drew a lively and thoughtful group of messages. Some 3000 people visited our Web project; dozens posted responses to five "scenarios" that posed difficult choices in science conduct. The interactive part of the Web project is now finished, having concluded on 23 August (although "Science Conduct Online" can still be viewed on the Web at [address http://sci.aaas.org/aaas/]). But the energetic response to this, *Science*'s first World Wide Web project, indicates that in the future, readers with important things to say to *Science* will be able to choose from more than one medium to carry their thoughts.



The Devi Case and More

Eliot Marshall's article "Authorship: Dispute slows paper on 'remarkable' vaccine" (Conduct in Science, 23 June, p. 1712) describes the case of a postdoctoral fellow who feels that she was denied sufficient recognition for her achievement by the senior scientists who were her mentors. As ones who have made many attempts to resolve the dispute, we would like to add a few points that may help to clarify these matters. Sarvamangala Devi initiated the development of a vaccine directed against the pathogenic fungus *Cryptococcus neoformans*. She synthesized a cryptococcal polysaccharide conjugate and was the first author on the only publication deriving from this work to date. Moreover, she is the first-named inventor on the only patent application. The charges of failure to give appropriate credit brought by Devi against her associates were returned by the Department of Health and Human Services (DHHS) Office of Research Integrity (ORI) to the National Institutes of Health (NIH) for administrative resolution of the dispute. We wish to emphasize that a peripherally related investigation by the ORI into the data-reporting activities of one of her associates followed, but ORI found no evidence of misconduct. Although this dispute has delayed publication of Devi's mouse protection experiments, the development of the vaccine for potential clinical use by John Bennett and Peter Williamson progressed at a steady and reasonable pace despite the controversy.

In addition to recognizing the contribution of Devi, we reaffirm our respect for Bennett, who is internationally recognized for his contributions and expertise in the field of cryptococcal and other fungus infections, and for Rachel Schneerson and John Robbins, who introduced the concept of conjugates for polysaccharide-based vaccines and who are the developers of the *Haemophilus* type b conjugate vaccine for bacterial meningitis (now administered worldwide), a new vaccine for typhoid fever, and several other vaccines now undergoing clinical evaluation.

Marshall is correct in identifying the need to settle authorship disputes more efficiently within the scientific community. However, owing to the need to maintain confidentiality during resolution of this dispute, Marshall was unable to interview the involved parties. Consequently, an incomplete view of the controversy emerges. We all need to work constructively together to find ways to resolve conflicts of this kind without adversely affecting career prospects or distinguished scientific reputations.

Michael M. Gottesman
Deputy Director for Intramural Research, National Institutes of Health, Bethesda, MD 20892–0140, USA

Arthur S. Levine
Scientific Director, National Institute of Child Health and Human Development, Bethesda, MD 20892, USA

In their letter, Gottesman and Levine make several statements that I believe are seriously misleading and provide an incomplete account of facts.

They refer to an ORI investigation of "data-reporting activities." Lyle Bivens, the director of ORI, correctly described this investigation in a letter to me as an investigation of an "allegation of falsification of data."

It is true that ORI returned a part of my complaint for action by NIH, but with the statement to me that it "agrees with the report of the inquiry committee that the failure to credit your contributions to the research may constitute an inappropriate research practice." Gottesman fails to mention his administrative ruling that I should be included as an author in "all future publications" in which the specific clinical lot of the vaccine I prepared has been used for human trials. He also ruled that my authorship should be included in abstracts and papers that were published following presentations in three major international meetings in 1992–1993. Gottesman's rulings reflect the validity of my charges.

Gottesman and Levine state that the "dispute" (over the allegations investigated by ORI) delayed the publication of my mouse protection experiments. This statement is false. As I have stated previously, the delay was caused by actions of senior NIH officials, including Gottesman and Levine. For more than a year, NIH delayed the approval of my manuscript that included the very same experiments which NIH had already approved in 1992 and early 1993 for presentations in international meetings, including the 1992 Maxwell Finland Award Lecture by John Bennett. These facts are well documented.

Gottesman and Levine refer to the "development of the vaccine for potential clinical use by John Bennett and Peter Williamson." This reference is misleading. As documented in my complaint of 2 September, Bennett and Williamson were not involved in the slightest in the development or preparation of a cryptococcal vaccine for potential clinical use. They used the vaccine that I alone had developed and prepared over a period of many months, and they arranged for its administration to the human volunteers.

My allegations were of scientific misconduct against four NIH researchers, not all of whom were senior scientists or my mentors, as stated by Gottesman and Levine. The seniority and the past experience of my former colleagues were not the issue. By failing to address the issues raised, by exaggerating the role of their colleagues and by minimizing my role in the development of the cryptococcal vaccine, Gottesman and Levine are publicly supporting my former colleagues' pattern of behavior that led me to file my allegations. The letter does not mention that Levine was among those I charged with taking steps that hurt my career. He should be the last person to publish a letter on the issue of resolving conflicts of this kind "without adversely affecting career prospects."

The NIH official with primary responsibility for handling these allegations was and still is Gottesman. What appears to be a profound bias on the part of the top NIH and ORI officials has been the greatest obstacle in defending my rights. The letter by Gottesman and Levine shows the depth of this problem.

Sarvamangala J. N. Devi
c/o Self Help for Equal Rights, Garrett Park Post Office Box 105, Garrett Park, MD 20896, USA

By portraying Devi's allegations of "theft of research" and plagiarism, detailed in her

scientific misconduct charge filed with NIH and ORI, as a squabble among co-authors, Marshall's article trivializes important issues.

The article correctly states that Devi did not agree to be a co-author on a manuscript. It does not mention Devi's charge that the manuscript plagiarized her work. It does not state that her co-authors refused for several months to let her see the data for the manuscript bearing her name. She was finally allowed to see some data, but under bizarre and restrictive conditions.

The article does not mention that Devi developed serious concerns about the integrity of the data, nor does it mention our allegations that some of the data had probably been fabricated. We were told the allegations would be investigated by ORI, but we have not been informed of the outcome, nor does Marshall describe it in his article. The article does not mention that NIH did not inform patients receiving vaccine in a clinical trial that we and Devi had made allegations about the authenticity of data offered by Schneerson, Robbins, Bennett, and Williamson. Despite our repeated urging, NIH has apparently not taken this simple step.

The article makes it sound as if Devi submitted her paper to NIH for clearance in the spring of 1995. In fact, a version of the paper was submitted for clearance in January 1994 and a second version, revised slightly to meet criticisms, in May 1994. Thus we believe NIH officials and her former colleagues, not Devi, were responsible for the 1-year delay in NIH's approval of the Devi manuscript.

DHHS policy forbids Devi to discuss the facts in this case, facts that bear directly on patient care and on the integrity of science. This policy is contrary to the norms of science and serves the public poorly. In following this policy, NIH does not live up to its best traditions.

We suggest that the cryptococcal vaccine be called the "Devi vaccine" in honor of its discoverer.

Walter W. Stewart
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Ned Feder
9609 Wadsworth Drive,
Bethesda, MD 20817, USA

Marshall's article buries the fact that the charges leveled by Devi against us of scientific misconduct, plagiarism of ideas and of data, and falsification of data were rejected by the DHHS ORI. Similarly her charges of discrimination based upon race, religion, and gender and sexual harassment were rejected by the NIH Office of Equal Opportunity Employment. Marshall does not report that the "delay" in publishing results of an animal experiment was caused by Devi's failure to prepare a manuscript for about 2 years. Finally, contrary to Marshall's reporting, we did *not* resubmit the article to *Science* in August 1993.

John B. Robbins
Rachel Schneerson
John E. Bennett
National Institutes of Health,
Bethesda, MD 20892, USA

What surprises me most about Marshall's article is not the curious spin that Devi puts on her actions at NIH. Rather, it is the portrayal of Gottesman as mediator. Almost 2 years ago, in November 1993, Gottesman was asked to intervene in a constructive manner to resolve the authorship dispute. Instead, Gottesman, in response to external political influence manufactured by Stewart and Feder, launched multiple rounds of investigations that were not intended to resolve the authorship issue. This tactic removed Gottesman from having to defend the outcome of any negotiated settlement between the parties. In addition, the way in which these investigations were conducted created such a climate of fear that not only

the present project but the entire field was adversely affected. For example, breaches in confidentiality contributed to halting the development of a second-generation vaccine in February 1994, because of concerns by collaborators outside NIH. Maybe if this climate of "scientific terrorism" can subside a bit, all of us in the field will be able to go back to working for our patients, which should be our first (and only) priority.

Peter R. Williamson
University of Illinois at Chicago,
Chicago, IL 60612, USA

Marshall's article mentions that unnamed officials have stated that the misconduct charge by Devi was dismissed in April 1995 in a "precedent-setting" decision by DHHS's ORI. Several years earlier, I had filed a charge with the Office of Scientific Integrity (OSI) alleging that my mentor had misappropriated my Ph.D. dissertation. Upon review of my allegations, OSI stated that a mentor was expected to provide "intellectual input" into a doctoral dissertation and determined that there was insufficient evidence of scientific misconduct because it believed that I had not proven that the "ideas" of my dissertation were solely my own. The OSI suggested that it would be collegial for the mentor to share credit for an approved dissertation, but it is not scientific misconduct if he does not. The OSI and now the ORI maintain that, in most cases, plagiarism cannot occur between authors in collaborative research (*1*). These principles and policies established by the OSI in my case in 1991 appear to have been applied by ORI to the Devi case in 1995.

Marshall's article portrays Devi as the villain who slowed progress on the vaccine she invented. It is probably true that had she not complained in a charge filed with ORI that her superiors were transferring to themselves the credit for her discovery, the delay would not have happened. But if they had given Devi the credit she thought she was due, she would not have complained. It is unfortunate that in biomedicine, superiors often credit themselves for work that they did not participate in. Had the NIH and ORI investigations been quicker, the delay would have been shorter. The Public Health Service, as employer of all involved, could even have required that development continue while Devi's complaint was being dealt with. With these other links present in the chain of causation, it is unfair to single out Devi as being responsible for the delay.

Jane E. Rosen
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1. *ORI Newsletter* **3**, 3 (December 1994).

Marshall notes correctly that ORI found it was not misconduct when credit for Devi's work was progressively annexed by her superiors. But ORI has a peculiar definition of misconduct, described in a letter it sent to Devi on 3 June 1994, when it decided against her.

It is ORI's longstanding policy that disputes over authorship or other credit in publications or presentations arising from research conducted by former collaborators or by those who have a past mentor trainee relationship generally do not warrant examination as plagiarism or other serious deviations in reporting research as defined in Federal regulation. Because the process of collaboration and the training of scientists involve a voluntary sharing of ideas, plagiarism in the form of theft of ideas generally cannot occur in these settings.

This decision was not a first. The OSI made a similar decision when it wrote Jane Rosen on 5 December 1991

It is expected that a mentor will provide intellectual input, biological systems, and research resources for a doctoral student; the NYU report and the OSI review indicated there is insufficient evidence that the ideas for use of BzAF were solely your own, to serve as a basis for investigation.

As a precedent this decision had little effect because, like the Devi decision, it was not made public. The result is one-sided case law. The criteria for guilt are displayed in published ORI decisions, the criteria for innocence are concealed. Concealment continues even after a complaint is made. Only after wasting months of Rosen and Devi's time did OSI and ORI send them the quoted passages that said they could not have won.

Charles W. McCutchen
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Marshall's article focuses on the problem of young, creative scientists, particularly women, who have their research efforts usurped by their supervisors. Most do not choose to complain. Many leave research completely, depressed and disillusioned. Those few who do complain often are a lone voice against more senior scientists, with little chance of being helped by the management or others.

Devi's development of the C. *neoformans* vaccine was a major accomplishment for any scientist and should have earned her many accolades.

Devi is the undisputed first-named inventor of the vaccine (*1*). Robbins, Bennett, and Schneerson have not signed a declaration that is necessary for patent approval and the subsequent commercial production of the vaccine.

Devi prepared about 2500 human doses of the vaccine for clinical trials before leaving Robbins' laboratory. Gottesman, the NIH Deputy Director for Intramural Research, has told Robbins, Bennett, and Schneerson that any papers resulting from clinical trials using the vaccine prepared by Devi must also give her credit. But, according to Levine, Robbins and Schneerson will not use the rest of the doses in any additional projects. These actions are delaying the testing and production of this potentially life-saving vaccine.

What can be done? We ask that concerned scientists write to the NIH Director, Harold Varmus, requesting his intervention and urging him to do what is necessary to speed the release of the vaccine.

Billie Mackey
President, Self Help for Equal Rights, Garrett Park Post Office Box 105, Garrett Park, MD 20896, USA*

*A women's group at NIH

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1. S. J. N. Devi et al., *Infect. Immunol.* **59**, 3700 (1991).

Marshall's article about Devi points out a problem that is hounding science at the highest levels: proper accreditation for scientific ideas and research that are the result of collaborative effort. High intellectual capacity and achievements do not necessarily mean that a scientist will make a good mentor, nor that a successful scientist has high ethical standards. Competition is fierce, and production a necessity.

Devi's success with the cryptococcal vaccine was followed by her development of a *Vibrio vulnificans* vaccine protective against this oyster-related pathogen (*1*), thus affirming her continuing scientific productivity.

The "Guidelines for the conduct of research in the intramural research program of the NIH," which describes the proper supervision of trainees, state that "It is particularly critical that the mentor recognize that the trainee is not simply an additional laboratory worker" (*2*, p. 7). Mentors in science traditionally guide and supervise in a way that enhances the careers of their trainees. If NIH is to retain its prestigious worldwide reputation, it is particularly important that scientists at NIH heed the guidelines provided them.

Devi's case seems, in my experience, indicative of problems encountered by a number of women scientists at NIH (some on fellowships from foreign countries). Many

of the problems are not reported for fear of lasting career damage. It is not only that the public is badly served when promising scientific careers are destroyed. The wonderful work that may have been achieved during a fruitful research career is never accomplished. It makes us all a little poorer.

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2. *Guidelines for the Conduct of Research in the Intramural Research Program of the NIH* (U.S. Department of Health and Human Services, Washington, DC, 1992).

I worked at the National Institute of Child Health and Human Development in the 1970s and had the privilege and pleasure of working with John Robbins and Rachel Schneerson, in whose lab the dispute over a vaccine for C. *neoformans* recently arose. They represent what I consider to be the epitome of good science done by great persons. I wish that every medical student or young scientist had the opportunity to work with them in their lab. They would learn much about science done well, but even more about how to lead professional lives with integrity, generosity, and respect for the truth.

James J. Schlesselman
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Regarding my contribution to the research program in question of Schneerson and Robbins, I would like to point out that these two scientists have been my friends and colleagues for many years. During one of many informal discussions at the time, they described to me the low yields of *Haemophilus influenzae* type b polysaccharide, which could be bound covalently to proteins using 1,6-diaminohexane as a linker. I simply suggested that they use adipic acid dihydrazide instead and directed them to publications on this subject. I was acknowledged many times by these honest and excellent scientists for my suggestion, which assisted their successful work. I had never worked on polysaccharide-protein conjugates, and the concept of using these compounds as vaccines was not mine.

Meir Wilchek
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Response: Robbins, Schneerson, and Bennett make odd company here with Stewart and Feder, arguing that *Science* should have given more attention to Devi's dismissed charges of misconduct. I do not agree, first, because the scientific misconduct and harassment charges appeared to be symptoms of a deeper problem—a total breakdown of trust among co-workers. This calamity, not the federal inquiry, made the dispute relevant for a forum on ethics. In any case, an examination of the federal inquiry would not have been fruitful, for the government finessed the main issue (alleged credit theft), on the basis that mentor-trainee disputes categorically "do not warrant examination as plagiarism." Second, none of the principals who think the inquiry results should have received more attention was willing to discuss them, being constrained by federal rules not to do so. Without direct information of the charges and responses, it seemed best to limit comment on them.

Whether Devi or her NIH colleagues bear the greater responsibility for delaying publication of her C. *neoformans* mouse study is debatable. But Robbins *et al.* have identified one error in my article: they submitted a paper on Devi's research to *Science* (without Devi's permission) just once, not twice, as the article stated.

—Eliot Marshall

Although it seems somewhat petty to protest Jon Cohen's article "Share and share alike isn't always the rule in science" (Conduct in Science, 23 June, p. 1715), I take offense at the way in which he discusses the reputation of my laboratory. I feel it is important that I defend our record as responsible citizens in the scientific community, and I would like to set the record straight.

Cohen states that we had distributed mice to 116 laboratories. He does not mention, however, that many of these laboratories received multiple strains of mice. We have delivered in the neighborhood of 300 to 400 breeding pairs of a dozen different strains to laboratories in all parts of the world. In my opinion, this is a significant effort, and I would not mind it being compared with contributions made by other laboratories.

I have always believed in the sharing of reagents as a basic principle of science. And indeed, my laboratory has a long record of sharing reagents freely and expeditiously with other members of the scientific community. In 1984, within days after our publication of the T cell receptor genes, we had sent out dozens of probes; to this day, we have supplied more than 1000 laboratories worldwide with about 5000 T cell receptor probes. In one case, we sup-

plied an investigator in the United States with all the T cell receptor genes we had ever cloned (more than 100).

Cohen cites "critics" in his discussion of the reputation of my laboratory. With the sheer number of requests we get for mice, it is unavoidable that delays sometimes occur in delivering them. Yet I believe our turnaround time is competitive with, or perhaps faster than, the time it might take to receive delivery of similar mice from other laboratories. In addition to breeding and typing, there is a large volume of work associated with delivering mice, particularly out of the country, which requires the preparation of numerous documents, such as health and customs certificates, export permits, and courier forms. In the end, it may be inevitable that some who requested mice did not feel entirely satisfied with our speed in meeting their requests. It is unfortunate, however, that Cohen highlights the dissatisfaction of a few rather than the views of the many.

Tak W. Mak
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The views of Robert Huber concerning the distribution of coordinates from solved crystal structures (Conduct in Science, 23 June, p. 1715) are not consistent with my understanding of the dynamics of the drug design process. Most industrial laboratories that are serious about the use of structural information to design drugs will not care to wait even 1 year, let alone the 2 that Huber has adopted as his personal standard, to bring critical-path structural information to bear in house. The power of the methodology is best expressed when multiple structures of the target macromolecule under investigation are solved, with ligands bound to them, and in a timely fashion (*1*). That process requires a familiarity with the structure of the target that cannot be obtained by simply importing coordinates from the outside. A tacit assumption in discussions of coordinate deposition is an inflated estimate of the intrinsic value of raw coordinates and the difficulty associated with their independent reconstitution by a suitably motivated industrial lab. Beyond that, of course, structural biology represents only a part—albeit an important one—of the more comprehensive effort that drug design entails. Clinical success in this arena requires the cooperation, feedback, and interplay of a broad spectrum of disciplines.

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While Gary Taubes quotes me accurately ("McGill: Analyzing the data," Conduct in Science, 23 June, p. 1714), that quote does not do justice to the paper by Judith Swazey *et al.* (*1*). Their paper is one of very few that provides quantitative information about any aspect of misconduct in science, it carefully describes what was done and what was found, and its conclusions do not go beyond the scope and reliability of the data. It demonstrates that a high proportion of both faculty and students report observing or having other direct evidence of various kinds of misconduct. It is hard to see how this useful information could have been collected in any other way. That the paper does not answer the more basic question about the frequency of misconduct itself is hardly the fault of the authors; nobody else has any good way to develop reliable data on that matter, either.

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The "Responsible conduct of research" course at the Stanford University School of Medicine is characterized (Conduct in Science, 23 June, p. 1711) as an ethics course that increased attendance by bringing in big-name speakers ("big guns"). As a participant, I can say that the course was not about ethics, but about money—it was required by NIH to ensure funding. This was reflected in the fact that Stanford graduate students were ordered, not requested, to attend. It was also reflected in the tone and scope of the lectures and breakout sessions, which were geared more toward "what you can't get away with in science research" than "what the ethical thing to do is."

Questions of ethical conduct of institutions, such as medical-related corporations, university departments, and granting agencies, were avoided, especially any treatment of the key issue of the influence of corporate representatives on large-scale research funding decisions of government agencies. Perhaps the most honest treatment was given by David Botstein, who stated, "We live, for better or for worse, in a capitalist country—in a capitalist society. . . ."

To characterize high course attendance

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as an attraction to big-name speakers encourages an unfortunate reverence for authority figures that is unbecoming the skeptical scientific mind.

Gregory B. Barnes
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Science is to be commended for dedicating a portion of its 23 June issue to the topic "Conduct in Science" and for including in it Gary Taubes's descriptions of novel approaches to formal teaching about research conduct. I wish to make two comments, one general, the other partly self-serving (not untypical for a scientist).

The special section emphasizes the biomedical sciences, where NIH training grants predominate, with their concomitant requirement for some formal lecture-teaching exposure to ethical issues. There is, however, no coverage of the physical sciences, where training grants are rare. As the only chemist member of the original Institute of Medicine–National Academy of Sciences committee on the ethical conduct of research, I called attention to this operational difference in research support, which led me to suggest that recipients of research *grants* should also be obligatorily exposed to such instruction. This has not yet happened, which makes the question of pedagogic experimentation even more relevant.

Taubes's description of pedagogic experiments omits one that I believe merits some emphasis, as it also serves to enlighten the general lay public about conduct in scientific research. The general public knows little enough of *what* we do, but it knows even less *how* we do it. I am currently working on a tetralogy of novels in the infrequently used literary genre of "science-in-fiction" (not science fiction) to illustrate in an accurate way in the guise of fiction the behavior of contemporary research scientists.

The reception of the first novel, *Cantor's Dilemma* (Penguin, New York, 1991) has convinced me that "science-in-fiction" is a pedagogic tool well worth implementing, as it can cover the gamut from the general public to graduate students and postdocs. In the afterword of *Cantor's Dilemma*, I said, "Publications, priorities, the order of the authors, the choice of the journal, the collegiality and the brutal competition, academic tenure, grantsmanship, the Nobel Prize, Schadenfreude—these are the soul and baggage of contemporary science. To illustrate them . . . I write about behavior and attitudes surely more common than we like to admit." This novel has been translated into six languages, and was serialized daily in Germany's largest newspaper, the *Frankfurter Allgemeine Zeitung*. More relevant to the coverage of teaching such issues, it has become a text or recommended reading in many American colleges and universities.

Finally, nowhere in the otherwise extremely well-done coverage of scientific conduct do I find comment on the gender aspects of our science research culture, other than tangentially through description of the legal travails surrounding Sarvamangala Devi. There is more to it than just featuring women as whistleblowers or plaintiffs. How to compete on the tenure-track treadmill while pregnant and how the rest of the scientific establishment responds are issues well worth exploring as part of a broad overview of conduct in science, rather than in a group of articles about the special problems facing women in a tough laboratory science. I have made this a key element in my science-in-fiction series and have found from my lectures and even book reviews that it raises more questions and comments than any other.

Carl Djerassi
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Government agencies are eager to reduce science funding. But this could turn into wild enthusiasm when it is generally realized that the money supports an enterprise in which costly duplication of effort predominates, an enterprise in which the participants positively hinder the efforts of talented colleagues in order to advance their personal careers. It is good that problems of scientific misconduct and lack of cooperation between scientists are discussed openly. However, unless the scientific community deals effectively with the problems, it may provide those wishing to reduce spending on science with their most powerful weapon.

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Readers of the special section "Conduct in Science" may be interested to learn that a program at the State University of New York at Stony Brook is attempting to introduce the study of these issues into the secondary school science classroom. A team of science and philosophy faculty from Stony Brook and Dowling College in conjunction with the university's Center for Science, Mathematics, and Technology Education, is operating a series of summer institutes supported by the National Science Foundation that enable science teachers to analyze case studies and grapple with ethical issues that often emerge in their classrooms and laboratories. The teacher participants in this program, which is in its second year, develop ethics and values teaching materials for integration into their classroom science lessons.

These matters are much too important to be put off until citizens become undergraduate or graduate students. Secondary school students and teachers can engage in serious discussions about the conduct of science and the ethical obligations of scientists. At a time when so few of our citizens have any notion of what the enterprise of science is about, we must take advantage of all opportunities to shed light on topics with such important implications.

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Although I appreciate how issues of credit arise when important scientific discoveries are made, the article "The culture of credit" by Jon Cohen (Conduct in Science, 23 June, p. 1706) omits one important point. No issue of credit will ever diminish the thrill of understanding an astonishing fact of biology for the first time, or the scientific self-confidence that arises from a major discovery. Discussing only how credit is awarded gives a one-sided impression of the research process. In the end, no issue should be relevant other than the beauty of the science itself. For me, the overwhelming lasting memory remains the view under the microscope, not above it.

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The Johns Hopkins Institutions

The article "Management overhaul at Johns Hopkins" by Eliot Marshall (News & Comment, 30 June, p. 1842) calls for some clarification about the structure and governance of the health care enterprise at Hopkins.

First, it should be made clear that there are two distinct Johns Hopkins institu-