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LETTERS

Peer Review or "Performance Review"?

Ivan Amato's article about our new results on diamond synthesis (News & Comment, 30 Oct., p. 736) sacrificed some accuracy for cuteness. His headline, "PR is a better system than peer review," is exactly what I have been at pains to prove in 25 years of systematically critiquing the peer review system. "PR," in my lexicon, stands for "performance review." Along with our wisest colleagues, such as Philip Abelson, who a decade ago advocated that research evaluation be based on "performance rather than promise" (*1*), I have developed and am seeing adopted alternatives to the silly practice of mailing out essays for "peer review" to a "jury of axe-murderers" (*2*, p. 104), including those who compete for the same turf. The alternative review procedures (*3*) are based on multiple-venue peer review of scientists' *performance*.

Amato might have informed the reader that we have been working on diamond synthesis since 1957; that it was I who, in 1984, brought from Japan and the U.S.S.R. and stimulated in the U.S. research community the awareness of the chemical vapor deposition diamond process; and that Penn State's Materials Research Laboratory set up one of the largest research programs and an effective knowledge transfer mechanism to a consortium of some 25 companies, which have backed their own peer review of our performance with the payment of substantial sums each year. No better "peer review" exists.

I was at some pains to point out in our press conference that good journalists can get better peer review than many agencies and editors if they work at it and avoid obvious pitfalls. Guidelines for reporters of major science journals should, in my opinion, require excluding *all* anonymous comments; a reporter should also always request from her or his contacts some back-up *evidence* (papers, patents), not only offhand comments, so that she or he can judge the comments' relevance or worth.

Amato reports incompletely why we chose to make a public announcement of our results. The question that some of us occasionally face is, How do you make public what you regard as a "significant" advance in a field and (this fact is the key) in which perhaps 150 to 200 laboratories are actively working all over the world and many are looking at the technological potential? The problem is compounded when the field and the finding *may* have industrial significance. If one proceeds by the traditional route, one files any necessary patents and then writes a paper and submits it to a journal. The journal sends it to, say, three anonymous referees, who are under no legal or formal moral obligation to keep this paper confidential or to not build on the result. Thus for a period of, say, 6 to 12 weeks (in fast journals), a subset of investigators, including, maybe, three at companies with a major economic interest in the area, has been given an enormous advantage over every other scientist in the world.

Our present approach for announcing major new results—for which we welcome alternative suggestions—is to submit a paper to a regular journal *first*, thus putting ourselves on record before our "peers," and then to announce the new findings in public through the media, abjuring any hype or exaggeration of economic benefits. I fully appreciate the difficulty this may cause science reporters who are choosing the most significant advances to write about. In response, I urge the use of "PR."

With respect to the eventual significance of our new low-pressure, solid-state source route to both the science and the technology of diamond synthesis, as in all of science, time will tell.

Rustum Roy
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REFERENCES

1. R. Roy, *Science* 211, 1377 (1981).
2. D. Shapley and R. Roy, *Lost at the Frontier* (ISI Press, Philadelphia, PA, 1985).
3. R. Roy, *Minerva* 22, 316 (1985); *Sci. Technol. Hum. Values* 10, 73 (1985); *Bull. Sci. Technol. Soc.* 2, 405 (1982).

I wish to clarify the direct and indirect quotations attributed to me by Amato in his article of 30 October. It was an embarrassment to me and to the Pennsylvania State University administration to have the implication made that the "PR-for-peer-review switch . . . was blessed by the university's provost and by patent attorneys." This is not true. The provost was aware that an announcement of an important result on diamond preparation was to be made at a public news conference, but we were not aware that a factual news conference would turn into a discussion of the peer review system. Amato quotes me as saying that

"This [PR campaign] is forging new policy at Penn State for protecting important results." In my statement, "this" referred only to the manner in which we had employed the services of lawyers outside the university and had nothing to do with a "PR campaign." Since making that statement, I have learned this is not new policy and that outside lawyers have been used in other similar situations at Penn State.

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Response: True, Roy's manuscript arrived in the offices of *Science* and *Nature* before he held his press conference, but only several hours before the press conference. There simply is no way that he genuinely put the work "on record before our [his] 'peers,' " as he states, before putting it before the press.

Messier's comment again raises the question, What was the point of the news conference? At the least, it was a highly unusual means of communicating science that effectively bypassed standard peer review and was okayed at the highest level of university administration. *Science* covered the press conference as a PR event. —***Ivan Amato***

Response: Science, like many other journals, releases papers at the same time to all news media (i) to help journalists, who will know that a paper has been peer-reviewed by experts in the field; (ii) to allow other scientists to see the actual data, so that they may assess any statements made about the paper; and (iii) to allow authors, knowing these procedures, to be more authoritative when they discuss their work.

Roy alleges that peer reviewers "leak" privileged information to those who can profit economically from it, but this editor knows of no cases when this has occurred in *Science*'s peer-reviewing process.

An author may believe that he or she has made an extremely important advance, but frequently peer reviewers disagree, so reporters attending a press conference could have difficulty evaluating questions about the novelty and importance of these new data in a timely manner.

Moreover, the idea that "everyone" gets the information at a press conference depends on who attends it, the accuracy of the information being distributed, and the circulation of the newspapers and magazines that report it. The accuracy of such information could degenerate rapidly and the number of reporters attending such conferences could decrease if authors were to scramble to get priority by publicity.

This editor does not accept the notion that a tried-and-true procedure for giving the public accurate information and protecting authors' rights should be discarded on the basis of undocumented allegations that peer reviewers have violated ethical responsibilities.—***Daniel E. Koshland, Jr.***

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AIDS Theories

It is not surprising that six scientists picked by the Wistar Institute to assess whether a polio vaccine made at Wistar and tested from 1957 to 1960 in Central Africa could have spawned the AIDS pandemic (*1*) stress the odds were against it (*2*) (Random Samples, 30 Oct., p. 738); however, the jury came in before inspection of the best evidence—old vaccine samples. Two theories posit that this vaccine might have been contaminated with a rare simian virus tantamount to the human immunodeficiency virus (HIV-1) (*3*).

Significantly, the committee urged a U.S. and worldwide shift away from manufacturing vaccines in primary monkey kidney cultures (*2*, pp. 7–8), a medium in which the presence of unknown and therefore undetectable infectious agents cannot be ruled out. Ronald Desrosiers of the Harvard Medical School and the New England Regional Primate Research Center, who wrote the final section of the Wistar report, has called vaccines made in monkey kidneys "a ticking time bomb" (*4*).

Finally, what *Science* terms the "putative coup de grâce" to the theory, the case of a British sailor said to have died of AIDS in 1959, does not necessarily debunk it. The seaman's health did not begin to fail, and his AIDS symptoms were not apparent, until December 1958 (*5*). By April 1958, more than 200,000 Central Africans had been vaccinated (*6*). Although rare, rapid deterioration and death after HIV-1 infection have been known to occur (*7*). The report says, without documentation, that the sailor returned to England before the vaccine testing began, but it does not address the possibility that a fellow sailor returning from Africa could have transmitted the virus to the sailor who died. Moreover, Wistar panel member David Ho has acknowledged (*7*) that, while the committee does not think this sailor's AIDS originated with the vaccine, it was not saying his case disproves that the Congo vaccine sparked the AIDS *epidemic*.

Tom Curtis
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REFERENCES

1. T. Curtis, *Rolling Stone* (no. 626) (19 March 1992).
2. C. Basilico *et al.*, "Report from the AIDS/Poliovirus Advisory Committee to the Wistar Institute" (Wistar Institute, Philadelphia, PA, October 1992).
3. B. Elswood and R. Stricker, unpublished manuscript; L. Pascal, "What happens when science goes bad" (Department of Science and Technology Studies, University of Wollongong, Wollongong, New South Wales, Australia, 1991).
4. S. K. Wickham, *New Hampshire Sunday News*, 22 November 1992, p. 6A.
5. G. Williams, T. B. Stretton, J. C. Leonard, *Lancet* ii, 951 (1960).
6. G. Courtois, A. Flack, G. A. Jervis, H. Koprowski, G. Ninane, *Brit. Med. J.* 2, 187 (26 July 1958).
7. D. Ho, comments at a news conference releasing the Wistar Institute report, New York University School of Medicine, New York, 22 October 1992.

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Support for Pluto Mission

I would like to clarify some statements about Pluto mission planning at the National Aeronautics and Space Administration (NASA) in Eliot Marshall's article of 20 November (News & Comment, p. 1296).

There has been a growing push in the planetary community for a mission to Pluto for some years. In 1991, after a substantive NASA study (*1*) was made, a reconnaissance flyby mission was placed in the 1994–1997 new start planning queue by the scientific advisory board (SSAAC) to Lennard Fisk's Office of Space Science and Applications. The high ranking given to a Pluto mission was in part due to the widely recognized scientific relevance of the Pluto-Charon double planet system, which bears directly on our understanding of planetary origins, comparative planetology, and the potential for revealing physical processes not expressed elsewhere in the solar system (*2*).

As a result of these endorsements (*3*), the NASA Outer Planets Science Working Group (OPSWG) was charged with documenting and guiding the scientific content of Pluto mission studies. This group consists of more than 20 leading planetary scientists from around the nation and has been working with the Jet Propulsion Laboratory (JPL) since March 1991 to study a spectrum of Pluto mission architectures and implementations. Progress reports describing possible large and small missions were made to NASA's Solar System Exploration Subcommittee (SSES) in May 1991 and February 1992. In July 1992, OPSWG formally endorsed the small mission that Robert Stahele's JPL advanced studies team devised, finding it to be low cost, scientifically exciting, and technically competent. Indeed, this repeat mission has been placed at the top of OPSWG's priority queue for all outer solar system missions. Our specific recommendations were reviewed by the SSES in early December. A detailed description of the small-mission concept and its scientific content is also scheduled to be