Letters

Science and the Press

In his editorial on "Credibility in science and the press" (1 Nov., p. 629), Daniel E. Koshland, Jr., states that science and the press are similar in that "each profession is accountable in the establishment of procedures that responsible journalists and responsible scientists are expected to maintain."

Koshland seems to suggest that editors should collectively adopt a policy and a set of procedures that would ensure that their coverage of scientific developments is "responsible." This is tantamount to having the editors of, say, the *New York Times*, the *Wall Street Journal*, and the *Washington Post* telling the editors of other newspapers and magazines, including the "News and Comment" section of *Science*, what they should publish or not publish. I doubt that Koshland or any other editor would tolerate such outside dictation.

The concept of a "responsible" press implies censorship of some sort, since some authority has to determine what is "responsible" and what is "irresponsible." In this country the First Amendment effectively and deliberately removes any responsibility or accountability from the press. There is no responsibility to print only what is credible or truthful about science or any other subject, and no editor can be held accountable for failing to do so.

Science, as well as society, has benefited from the freedom of the press to publicize "scientific" reports released at press conferences, "findings" that haven't undergone peer review, opinions of "experts" who spend more time in court than in the laboratory, and "alarms" from false Jeremiahs. Rarely has a scientific development been subjected to as rapid and as thorough peer review as "cold fusion," largely because of attention from the lay press. Innumerable quack cures for cancer and arthritis, ignored by the medical and scientific press because they were incredible, have been forced out of the country because they were publicized in the lay press. Bad science and false prophets can't long survive in the glare of publicity, even favorable publicity.

As for the credibility of the press, most successful lay publications strive to publish accurate and credible stories, not because their editors feel a responsibility to do so, but for the same reason scientific journals like *Science* try to ensure the credibility of the articles they publish: credibility helps sell newspapers or magazines or journals.

As one of my editors wrote several years ago: "A newspaper editor who becomes overwhelmed with his sense of duty and decides that some news ought not be printed because it would be bad for the public to know about it will quite likely find one day that he has no newspaper to be an editor of" (*1*).

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REFERENCES

1. V. C. Royster, *A Pride of Prejudices* (Knopf, New York, 1967), pp. 128–130.

Response: I hate to disagree with a distinguished reporter of science like Jerry Bishop, but my editorial specifically said that there was no way that *Time* or the *New York Times* could dictate standards to the tabloids. What I complained about was the double standard by which science, which depends on freedom and individual initiative in exact analogy to the media, is asked to be responsible for all its miscreants whereas the press shrugs off all of its irresponsible behavior by saying any criticism threatens freedom of the press.

I asked for no censorship, only that a scientific opinion be accompanied by information about whether it was obtained from a peer-reviewed article, a press release, or a personal opinion whispered in the reporter's ear. Standards for good journalism are no more a threat to the press than standards for good science are to science.

—DANIEL E. KOSHLAND, JR.

Coverage of the "Gallo Case"

Jon Cohen's article about the *Chicago Tribune*'s coverage of the "Gallo case" (News Report, 15 Nov., p. 946) may help clarify the issues involved, perhaps even provoke a much needed debate about how science ought to be reported in the lay press. Considering the complexity of this particular case, I am happy that Cohen's dissection of my *Tribune* articles led him to conclude that I haven't made "many major errors of fact," even though this implies that I have made some major errors of fact. Scrutinizing the reporting of others, however, inevitably risks committing the complained-of sin. In arguing that the *Tribune* has "conveniently" omitted three relatively arcane pieces of information from its coverage of the Gallo affair, Cohen himself manages to omit many of the most salient facts.

The least-understood aspect of the Gallo case, and the most important, is the history of the development of the blood test for AIDS. Everyone now acknowledges that workers at the Pasteur Institute of Paris discovered the AIDS virus, called LAV, in 1983. It is less generally known that Pasteur also developed the first HIV ELISA (human immunodeficiency virus enzyme-linked immunosorbant assay). Months before Robert Gallo had even a single HIV isolate in continuous culture, the French made their first ELISA from LAV grown in peripheral blood cells, followed by virus from EBV-transformed B cell lines. Shortly afterward, Gallo obtained HIV antigen for his AIDS test by growing the French virus LAV (under the rubric "MOV") in a subclone (H4) of the HUT-78 human leukemic T cell line. When the Centers for Disease Control (CDC) arranged a blind comparison of the LAV and MOV ELISAs in early 1984, the French model actually scored better. Gallo later put his HTLV-IIIB AIDS virus (which he has lately acknowledged is also LAV) into another HUT-78 subclone, H9, that was licensed by the federal government to five American companies, including the Gallo lab's three principal contractors, for commercial production of the HIV ELISA. Cohen chides me for having failed to report, in a sidebar to my November 1989 history of the discovery of HIV, that the French B cell line did not yield enough virus for commercial production of the Pasteur's blood test. The story to which he refers, however, was about the race by the Pasteur and Gallo laboratories to infect a permanent cell line for research purposes, not about the commercial production of the AIDS test. Whatever their other accomplishments, neither lab claims to have manufactured and marketed a commercial test for AIDS. As evidenced by the CDC's results, the Pasteur's ELISA was more than adequate for establishing the etiology of AIDS. Cohen also suggests that Gallo's permanently infected human T cell line was a necessary prelude to the commercial production of the AIDS test. But the Pasteur licensee in this country, Genetic Systems Corporation of Seattle, never used a permanently infected cell line as a commercial source of HIV antigen, choosing instead to infect successive batches of a T cell line called CEM. Not only did the batch method provide more than sufficient quantities of LAV antigen for commercial ELISA production, it enabled Genetic Systems to avoid the higher number of false-positive results recorded by early ELISAs made with Gallo's H9/HTLV-IIIB (later attributed to the fact that uninfected H9 cells contain a surface antigen to which some HIV-negative individuals produce antibodies).

Next, Cohen mentions the 23 April 1984 news conference at which then Secretary of