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

Health and Human Services Margaret Heckler announced Gallo's "discovery" of the AIDS virus and the AIDS test. As shown by the stenographic transcript of Heckler's remarks and a videotape of the news conference, she made no allusion to the possibility that both the virus and the test had already been discovered in France, crediting these accomplishments instead to "our eminent Dr. Robert Gallo." As Cohen notes, Heckler did not read her prepared statement in its entirety, for some reason omitting a backhanded acknowledgment that Pasteur scientists had "previously identified a virus which they have linked to AIDS patients," as well as the prediction that the French virus "will prove to be the same" as Gallo's HTLV-IIIB (she couldn't have known how right she was). Cohen apparently thinks my not having taken account of Heckler's unspoken text in a recent *Tribune* article was somehow unfair to Gallo, although precisely how is unclear. He does not mention that the *Tribune* article described a National Institutes of Health (NIH) report questioning the veracity of Gallo's subsequent statements under oath—among them his assertion that, as of the day of the press conference, he did not consider LAV and HTLV-III to be "even substantially the same virus." Rather than helping reinforce Gallo's sworn statement, Heckler's unspoken prediction seems to contradict it. In that particular instance, the NIH report also happens to be wrong. In trying to link Gallo's state of mind with Heckler's, the report suggests, though without any substantiation, that "[i]t is unlikely that Secretary Heckler made this statement without input from Dr. Gallo." However, the credit for putting the words into Heckler's text, if not in her mouth, is taken by James O. Mason, then CDC director and now Assistant Secretary for Health. Moreover, had Heckler actually taken notice of a French virus "linked to AIDS patients," she would have called into question Gallo's sworn attestation—in a patent application filed that very morning—that he was "the original, first and sole inventor" of the AIDS blood test. The real story here, Sherlock, is why the dog didn't bark.

Cohen's final quibble concerns Gallo's authorship of the abstract for, and editing of, a paper Pasteur researchers published in *Science* in May 1983 (*1*). As the *Tribune* reported in 1989, the Gallo abstract and textual changes made it appear that Pasteur's virus might be the human leukemia virus HTLV-I, or at least a very close relative, whereas the Pasteur group had rightly concluded that its virus was not a leukemia virus at all and differed significantly in other ways from HTLV-I [the two Gallo papers (*2*) which appeared with the Pasteur article reported the isolation of HTLV-I from AIDS patients, in line with Gallo's hypothesis at the time that HTLV-I might be the cause of AIDS]. Although the Pasteur paper described the core proteins of LAV and HTLV-I as "different immunologically," the abstract stated that the French virus had "an internal antigen [p25] similar to HTLV p24" [the preliminary report of the NIH Office of Scientific Integrity (OSI) on the Gallo case terms this statement "clearly an overgeneralization from the material in the manuscript"]. The abstract also stated that antibodies from the serum of the French patient from whom the original LAV virus was isolated "react with proteins from viruses of the HTLV-I subgroup. . . ." The article actually reported that the serum recognized "a common antigen present on HTLV-I–producing cells and on the patient's lymphocytes" and cautioned that "the antigen recognized by the patient's serum may contain cellular determinants" (*1*, p. 870) (as was later shown to be the case). Cohen does not mention the conclusion of the OSI's preliminary report that "the sentence in the abstract must be viewed as an overstatement that does not accurately represent the views of the paper's authors." Nor does he point out that the OSI report characterizes as "a gratuitous, self-serving and improper act" Gallo's addition to the paper's text the statement that the French virus "appears to be a member of the human T-cell leukemia virus (HTLV) family." (After the paper left Pasteur someone, whether Gallo or an editor at *Science*, also added a second textual reference inducting LAV into the "HTLV family.") According to the OSI report, Luc Montagnier, the senior author of the Pasteur paper, stated that, upon recollection, he believes he was sent a galley proof of the article before publication, although he cannot find it in his files. Montagnier *is* certain, however, that he never agreed to the description of his nontransforming virus as a leukemia virus; in a letter to OSI, Montagnier states that he underscored the immunological differences between the two viruses in conversations with Gallo. Cohen wonders why the *Tribune* did not mention, in a recent article on the preliminary findings of the OSI investigation, that Montagnier might have seen a proof. The subtext here, of course, is that if Montagnier *had seen* a proof and failed to object then he must have agreed with Gallo's efforts to stress the (nonexistent) similarities between the French virus and HTLV-I. This is hard to imagine, considering Montagnier's public and private statements at the time, and especially considering the fact that the paper itself stresses the *dissimilarities* between the two viruses. In the end, however, the question is immaterial. The fact is that Gallo's characterizations of the Pasteur virus were not only unsupported, but contradicted, by the paper he held in his hands. Although Cohen does not mention it, even the preliminary OSI report concludes that whether Montagnier acceded to Gallo's changes or not "does not diminish the negative aspects of Dr. Gallo's actions, which were clearly uncollegial and inappropriate."

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REFERENCES

1. F. Barré-Sinoussi *et al.*, *Science* **220**, 868 (1983).
2. E. P. Gellmann *et al.*, *ibid.*, p. 862; R. C. Gallo *et al.*, *ibid.*, p. 865.

Response: Crewdson criticizes the implications and omissions in my article about his Gallo coverage. To clarify the implications: I did not mean to suggest that Crewdson did make "some" major errors of fact; rather, I was attempting to applaud his accuracy by noting that the errors he did make—and he did make errors—were mostly, as I wrote, "not significant." You could split hairs about the significance of some of the errors, but I'd rather give the compliment and leave it at that. As for the impression that the *Tribune* should have worked harder to interview Gallo, I did not state (nor do I think) anything of the sort.

Crewdson's sweeping contention that I focused on "three relatively arcane pieces of information" is undermined by his own arguments. He calls one of the three—the development of an effective blood test—the "least-understood aspect of the Gallo case, and the most important." That inconsistency aside, the other "arcane" item dealt with the credit afforded the French and the charge that Gallo steamrolled Montagnier into seeing things his way.

Crewdson states that his sidebar comparing the French and American cell lines was about infecting "a permanent cell line for research purposes, not about the commercial production of the AIDS test." Crewdson's sidebar was headlined "Whose laboratory was first to mass-produce an AIDS virus?" In the sidebar, he quoted Gallo announcing in April 1984, "We have the problem of mass production solved. That's one of the significances of what we're telling you today." It is misleading to presume that Gallo was highlighting the significance of the *mass-producing* permanent cell line solely because it allowed his lab to prove etiology. As even Margaret Heckler, then Secretary of Health and Human Services, made clear that April day, it was because the Gallo lab had success with the permanent cell line that "we

now have a blood test for AIDS which we hope can be widely available in six months." Crewdson then says I suggested that a permanently infected cell line was necessary for the development of the blood test. This is semantic sleight of hand: I wrote about the need for infecting a permanent T cell line—which is distinct from a permanently infected T cell line—to produce a blood test on a commercial scale. And the former scientific director at Genetic Systems, Robert Nowinski, confirmed to me that until the French successfully infected the permanent CEM line, which occurred after Gallo's lab had infected its permanent line, they did not have a commercially viable blood test.

With respect to the Heckler press conference, Crewdson recounts facts he has already reported, oddly assumes that I was chiding him for being unfair to Gallo, and then shares a lesson he learned from Scotland Yard. What he does not do is tackle my criticism. We know the dog didn't bark and, yes, that is the real story. We know that Heckler's pronouncements were nationalistic boosterism. We know that Gallo did not generously share credit when standing before the bank of microphones. But what the OSI document I referred to revealed was that the real story is less clearcut than Crewdson would have you believe. Heckler's press statement, which was passed out to journalists that day, credited the French and strongly linked LAV to HTLV-III. The OSI document went as far as to suggest that it was "unlikely" Heckler would have emphasized the similarity between the viruses "without input from Dr. Gallo." What's more, as Crewdson knows, both the French and American labs believed there were important differences between their two viruses at that point. Because Crewdson's arguments ultimately raise questions of perjury, fraud, and coverup, these facts deserve an airing—inconvenient as they may be.

Finally, Crewdson responds to my spotlighting his failure to note the OSI's conclusions regarding Montagnier's responsibility for the abstract. I am not challenging the facts Crewdson presents here on this matter. But I am pointing out that by not reporting the OSI's finding that "the content of the paper ultimately rests with [Montagnier]," Crewdson again is omitting an inconvenient fact, and one that I believe readers would find relevant. The OSI did, writing in its draft report, "The OSI believed that if Dr. Montagnier had received the galleys, and had acceded to Dr. Gallo's revisions, then Dr. Gallo's actions could not be considered possible scientific misconduct." Instead of reporting this in his 15 September 1991 article on the draft report in the *Chicago Tribune*, Crewdson stressed that the OSI "concludes that Gallo's summary, written at Montagnier's request, misrepresented the data in the article. . . ."—JON COHEN

Erratum: In reference 5 of the report "Defining protective responses to pathogens: Cytokine profiles in leprosy lesions" by M. Yamamura *et al.* (11 Oct., p. 277), some of the primer sequences were given in the 3′→5′ rather than the 5′→3′ direction. The correct sequences should have been as follows. IL-3, ATGAGCCGCCTGCCCGTCCTG and AAGATCGCGAGGCTCAAAGTCGTCTGTTG; IL-5, ATGAGGATGCTTCTGCATTTG and TCAACTTTCTATTATCCACTCGGTGTTCATTAC; IL-7, ATGTTCCATGTTTCTTTTAGG and AGCTTTTCTTTAGTGCCCATCAAAATTTTATTCCAACA; and IL-8, ATGACTTCCAAGCTGGCCGTG and TTATGAATTCTCAGCCCTCTTCAAAAACTTCTC.

Erratum: In the caption of the photograph accompanying the News & Comment article "Moths take the field against biopesticide" by Ann Gibbons (1 Nov., p. 646), a cabbage field was incorrectly identified as "a watercress field."

Erratum: In the report ."Functional contribution of neuronal AChR subunits revealed by antisense oligonucleotides" by M. Listerud *et al.* (6 Dec., p. 1518), the name of co-author Píroska Devay was misspelled.

Erratum: The note at the end of page 1287 in the News & Comment article "Advisory committee urges changes at OSI" by Ann Gibbons (29 Nov., p. 1287) contained an error. The conference "Misconduct in Science" that was held on 15 and 16 November 1991 was cosponsored by the AAAS and the Department of Health and Human Service's Office of Scientific Integrity Review (OSIR), not the National Institutes of Health Office of Scientific Integrity (OSI).