Letters

Battle Over Error

We write to correct factual errors published in the article "A bitter battle over error (II)" by Barbara J. Culliton (News & Comment, 1 July, p. 18).

1) Culliton indicates in her article that when we completed our manuscript analyzing the *Cell* paper (*1*), we decided against contacting its authors and that we contacted them only after authorities at the National Institutes of Health told us to. In fact, senior NIH officials sent us two memoranda telling us *not* to communicate with the authors of the *Cell* paper. We repeatedly asked that this prohibition be lifted. When NIH eventually granted permission, we promptly wrote the authors and sent them our manuscript.

2) According to the article, David Baltimore "did agree" to give us copies of two reviews, namely, the Eisen and Wortis reports. The article fails to mention that despite our repeated requests, neither he nor anyone else at Massachusetts Institute of Technology or Tufts ever sent us copies of these reports.

3) The article states that Baltimore demanded from us "an agreement to drop the subject" if a committee of immunologists concluded that the norms of scientific research were not transgressed. Baltimore actually said that Stewart must stop both public and private discussions of the issue. The article indicates that we rejected Baltimore's proposal. In fact, we agreed to the investigating committee, but not to the condition that we be silent.

4) The article states: "Without access to all of the original data, it is impossible to fully evaluate Stewart and Feder's analysis." In fact, our analysis can be fully evaluated by examination of the data on which it is based.

5) The article asserts that Stewart favors the correction of published errors that are minor. In fact, Stewart's stated view is that the need to correct errors parallels their importance and that it is generally appropriate to ignore minor errors.

6) The article states that in 1986 Feder was officially rated, after an appeal, as having only "partially met" a certain requirement of his job. It fails to mention that this was overruled 2 months later on further appeal; the performance rating was changed to "fully successful."

7) Culliton writes that a "graduate student" challenged the data. At the time, Margot O'Toole had been a postdoctoral fellow for 7 years.

8) The article states that our phones "ring constantly" with calls from those alleging scientific error or misconduct. In fact, we were receiving perhaps two calls a week.

9) According to the article, we "would settle for nothing less than access to the data" for the *Cell* paper. This is misleading; we simply asked the authors for access to their data, and they rejected our request.

10) Culliton describes us incorrectly as saying that we can no longer do science: "They say it is because NIH has so cut back their research resources that they can no longer do science." We did not make such a statement, and indeed we carry out laboratory research on a limited scale with the resources that have been assigned to us.

11) Culliton claims that we had agreed with NIH authorities to restrict our studies of professional practices. We did not agree to this restriction: it was imposed on us despite our objections.

12) The article erroneously states that a member of the National Academy of Sciences occupies a windowless basement laboratory on the same floor as ours.

The article contains some other errors and misleading statements that are not discussed here.

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1. D. Weaver *et al.*, *Cell* **45**, 247 (1986).

Response: Stewart and Feder's letter is subtly and sometimes downright misleading throughout. The following points are illustrative.

Item 1. Stewart and Feder wrote their manuscript critical of the *Cell* paper and submitted it to NIH officials for routine clearance without having contacted the paper's authors. Stewart and Feder are correct in noting that NIH officials then asked them not to contact the authors until the NIH fraud office could contemplate the issue. Initial discussion took place on 28 October 1986. On 12 December NIH scientific director Edward Rall gave them permission to contact the authors.

Items 3 and *9* can be considered together. Stewart and Feder state that they agreed to an investigating committee and that *Science* was misleading in saying that they "would settle for nothing less than access to the data." According to a chronology of events written by Stewart and Feder themselves, they rejected David Baltimore's suggestion that the NIH appoint a committee to look into the dispute. "We suggested instead that he himself appoint a committee and that *we* [emphasis added] and the committee jointly be given access to the original laboratory findings." Their letter to Baltimore was dated 18 March 1988, months after they began asking for access to the data.

As part of his proposal that NIH appoint a committee, Baltimore asked Stewart and Feder to stop discussion of the issue if the committee found the paper to be within scientific norms. As Stewart and Feder confirm in their letter to *Science*, Baltimore asked them to agree to drop the matter in both public and private discussion. Baltimore's request was made in a 17 March 1987 letter to NIH scientific director Rall.

Science obtained a copy of this and more than 100 other pages of pertinent memos and correspondence through the Freedom of Information Act.

Item 4. Stewart and Feder's criticisms of the *Cell* paper are based on 17 pages of laboratory data. The authors assert that those 17 pages do not represent all of the data. It is not possible to resolve the question of whether the paper misrepresents the data on which it is based without reviewing that data in toto.

Item 6. Stewart and Feder circulated a copy of Feder's 1986 performance review as described in our article. Perhaps they refer to a different review than the one they sent out.

Item 8. Perhaps it would have been more accurate to state that on those occasions when this reporter has been in Stewart and Feder's lab, their phones were ringing constantly and that they indicated it was a recurring phenomenon. Indeed, *Science* has no firsthand evidence to support their statement.

It is worth noting with respect to Stewart and Feder's statement that they get "perhaps two calls a week" that they say they do not keep any data to verify that statement. On 21 June, during an interview with *Science* that was interrupted by frequent phone calls, Stewart said that he and Feder receive "100 allegations or problems a year that appear to be meritorious or at least not delusional," and added "I wouldn't want to keep records or have that minute an accounting." In testimony before the House of Representatives on 12 April, Stewart and Feder said that "perhaps the majority" of those 100 allegations are about past events. (Hearing transcript, p. 92.)

Item 10. Stewart and Feder have not only told *Science* that they can no longer do research. They have told others.

As long ago as 29 August 1985, they wrote to John Maddox, editor of *Nature*, and said, "As you know for the past two years we have done almost no laboratory work. Dr. Roth [their supervisor] has expressed strong dissatisfaction with the pro-

gress we have been making in our laboratory work. . . . He has taken laboratory space from us, has transferred our neurophysiology equipment to another group. . . ." Later in the letter to Maddox, which concerned pending publication of a paper on fraud, they repeated themselves. "For almost two years we have worked almost exclusively on this report and have done almost no laboratory work." That, in fact, is why Roth reduced their laboratory resources.

In memoranda to NIH director James B. Wyngaarden, one dated 10 September 1985 and another 30 April 1986, they said, "As you know, it is our intention to return to our usual laboratory work as soon as we can." They have not yet done so.

Item 5. Stewart and Feder's letter of correction to *Science* permits the reader to judge the extent to which they believe that it is "generally appropriate to ignore minor errors."—BARBARA J. CULLITON

Revival of the Ethics Advisory Board

A window of opportunity has opened, and biomedical and behavioral researchers are well advised to take advantage of it. The Department of Health and Human Services (DHHS) announced in July plans to revive its Ethics Advisory Board (EAB), a body that ceased to exist in 1980. The EAB will advise DHHS on the ethical acceptability of the conduct and funding of certain proposals for research involving human subjects. Comments on the EAB's proposed charter (*1*) must be received by 14 November 1988. Among the issues now open for discussion is the board's membership. The draft charter calls for at least one third, but not more than half, of the 21 members to be scientists, physicians, and other health professionals. The balance of the members would be drawn from law, ethics, and the general public.

Research with human subjects falling under the EAB's purview may include that with demented patients, children, prisoners, AIDS patients, fetuses, and human sperm and eggs. In the case of human sperm and eggs, the prolonged absence of an EAB has stifled research in human in vitro fertilization (IVF). Such a board is required by federal regulation to advise DHHS as to the ethical acceptability of such research before grants can be funded. Investigators have not been submitting proposals involving human IVF because of a widespread awareness of the de facto ban on such research. The chilling effect of this moratorium on IVF research is such that the National Institutes of Health estimate they might receive more than 100 grant applications related to human IVF if the EAB existed.

Revival of the EAB should not be taken as a green light for the funding of biomedical and behavioral research that raises ethically sensitive issues. Rather, it represents installation by the DHHS of a functioning traffic signal on a road that has long stood unused. Researchers, through their timely comments on the draft charter of the EAB, can help to calibrate that traffice signal. In addition, it is incumbent on biomedical and behavioral researchers to begin submitting research proposals that may have previously been withheld. In the Reagan Administration's waning days, the DHHS has laid out a plan to consider funding some of the most ethically—and politically—sensitive areas of research in all of science and medicine. Biomedical and behavioral researchers are challenged to respond.

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Research at USAMRIID

I am writing to object to the use of terms such as "biowarfare research," in particular, in William Booth's article "Post Office nixes germs by mail" (News & Comment, 1 July, p. 15). This appears to be part of a continuing trend that misrepresents the nature of biological research by the Department of Defense and the U.S. Army.

Since the early 1970s, the Army has conducted research at the U.S. Army Medical Research Institute of Infectious Diseases (USAMRIID) that is "research for defensive purposes only, such as to improve techniques of immunization and medical therapy" (*1*). Hence, these programs are accurately referred to as "biological defense research programs."

Although the difference between the phrases may seem trivial, it dramatically affects the way a reader perceives the purpose of the programs. Phrases such as "implements of biological warfare" conjure up images of biological weapons development. This is simply not what goes on at USAMRIID. A major portion of our work deals with development of vaccines that are not only of significance to the military, but also have relevance and application in the civilian sector. It is the policy of USAMRIID that research is unclassified and is routinely presented and published in peer-reviewed, scientific journals. In addition, we have an open policy of collaboration with scientists in academia and industry around the world. An example of this collaborative effort was noted in John Walsh's article about the Rift Valley Fever outbreak in Senegal (News & Comment, 10 June, p. 1397).

I comment particularly because of the use of the word "biowarriors" in the article of 1 July. I am a cell biologist doing postdoctoral research, not a "biowarrior." This term is inappropriate and derogatory. It implies that anyone (regardless of whether they are associated with a government or civilian institute) studying highly infectious organisms has only warfare in mind. As with any biological system, there are a number of reasons scientists study these infectious organisms. We should not close our minds to the wealth of information contained in these systems because of such a misunderstanding.

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1. *National Security Decision 35* (National Archives Record Group 273, Washington, DC, 25 November 1969).

"La Niña" or "El Viejo"?

Richard A. Kerr's use of the term "La Niña" (Research News, 26 Aug., p. 1037) for the anti–El Niño phenomenon is unfortunate. The term "La Niña," coined by George Philander of Princeton University is sexist because it implies that girls are the opposite of boys, when they are really two versions of the same thing. It also implies that girls are cold while boys are warm (perhaps they are at Princeton). More important, as far as we know, Christ did not have a sister. In my review of El Niño North (*1*), I used the term "El Viejo" (the old man) for the cold water phenomenon because of its association with Old Man Winter.

Then again perhaps they should be called Tweedledum and Tweedledee.

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