Letters

The Dingell Investigation

In the 19 May issue of *Science*, Barbara J. Culliton reports (News & Comment, p. 765) that, while talking to reporters during a break in the congressional hearing, I made new allegations in the Baltimore–Imanishi-Kari affair and cited evidence that had not been made public. This is incorrect. What I said was simply a repetition of my own testimony or that of the Secret Service. Specifically, I was questioned by reporters about whether the authenticity of certain data could ever be determined. I restated my belief that the question would be answered definitively and that the forensic evidence would show I was telling the truth.

Walter Stewart then reminded the reporters about the forensic analysis of gamma counter output tapes described by the Secret Service during the 4 May hearing. When complete, this analysis alone could settle the question of whether or not the data are authentic. However, as stated at the hearing, the Secret Service needs some additional information before issuing a definitive report. As Representative John Dingell (D–MI) stated at the hearing, resolution of the issues has been delayed by the authors' decision not to answer questions from the investigating staff of the congressional subcommittee.

The same article reports, "As one charge is answered, another is made, leading scientists to charge O'Toole with creating a constantly moving target." My charge is, as it has been from the beginning, that the central claim of a paper by D. Weaver *et al.* (*1*) is not supported by the underlying data. I am not the target of the investigation, and the movement of the target is not under my control. The history is as follows. In May 1986, I discovered that there were serious misstatements in the published paper. I brought my concerns in writing to the authors. The authors acknowledged some of the misstatements, but refused to submit a correction. Herman Eisen, who was solely responsible for the Massachusetts Institute of Technology's investigation, has testified that he did not even look at data before reporting to the MIT administration that the paper required no correction. I then dropped the matter, but others pursued it.

Two years later I was called to testify at a congressional hearing. I stated that the authors had refused to correct known errors. A panel of the National Institutes of Health then investigated and issued a report confirming many of my assertions about the published data and stating that the errors were "serious." However, the panel ruled that the central claim of the paper was supported by unpublished June 1985 subcloning experiments. The NIH report called for these subcloning experiments to be published in "replacement" of the published data. (The authors, however, replied that they have a difference of opinion with the investigative panel. Instead of replacing the published experiments with the unpublished experiments, they simply submitted a small sample of the subcloning data.)

When I received the panel's report, I immediately informed NIH that in May 1986 I had been told that no such subcloning experiments had ever been done. Thus, I am now raising questions about the authenticity of certain data submitted to the NIH panel. Some of this questioned data has recently been published in *Cell* (*2*) in the form of a correction required by NIH. As James B. Wyngaarden testified, NIH relied on statements from others involved in the case in concluding that my assertions were not likely to be correct.

Since then, however, the Secret Service has determined, and the authors do not dispute, that at least some of the data was entered in the notebooks after I asked to see them. Forensic experts also testified that certain alterations of laboratory records were probably done with an "attempt to conceal" the changes and that certain experiments, not just the recording of data, may not be authentic with respect to time. In light of these and other revelations that Wyngaarden described as "disturbing," the newly reopened NIH investigation will include the complete forensic analysis.

The article by Culliton incorrectly states, "O'Toole called into question the validity of experiments compiled by coauthor Thereza Imanishi-Karl after she had examined just 17 pages of Imanishi-Kari's raw data." As all involved have acknowledged, this is not what happened. In fact, I met with Imanishi-Kari and reviewed all data she presented before I reported to Eisen.

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REFERENCES

1. D. Weaver *et al.*, *Cell* **45**, 247 (1986).
2. T. Imanishi-Kari, D. Weaver, D. Baltimore, *ibid.* **57**, 515 (1989).

I am concerned that readers of "Dingell *v.* Baltimore" (News & Comment, 28 Apr., p. 412) will not "see the forest for the trees." We should remember that the central issue of who shall defend ethical science against scientific misconduct far transcends the details of this case. Representative John Dingell's investigation is a serious encroachment on the free pursuit of science and upon scientists' right for self regulation. Just as all citizens have a right to a jury of peers for civil offenses, so must scientists have a right to be judged by their peers in cases of alleged scientific misconduct. A congressional subcommittee hearing is not the forum for judging the validity of complex experimental results. The establishment of infrastructures in research institutions for the investigation of scientific misconduct and unprofessional behavior would better serve science as well as the patrons of science: Congress and the public.

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Responsibility

Most scientists agree that scientific misconduct falls into two categories: fraud, a rare event but well publicized when discovered, and negligence, which is far more common. The problem of willful fraud in science probably has no simpler solution than that already evident: intellectual curiosity and open debate. Negligence is a more difficult problem, for which there is also a simple solution: scientific journals should require a "responsible author" (RA) for each manuscript. This person would assume responsibility for the methods and results described in the published article and, should there be misconduct by any of the authors, the RA would be culpable. Corrective measures taken by granting agencies would also be the responsibility of the RA.

Among the items of particular concern to RAs in biomedical manuscripts would be understandable and reliable descriptions of methods; molecular sequences of macromolecules that are correct; and the number and type of controls for such techniques as the polymerase chain reaction and in situ hybridization. The proper and humane care of experimental animals and the longevity and health of control animals is fundamental to good science and should be assured by the RA. If cell lines or long-term cultures were used, the RA should determine whether or not the cultures have been recently tested for infection with organisms such as mycoplasma. Theoretical and experimental work in chemistry, physics, and earth sciences should share the same general principles.

Designating an author to assume responsibility for the reliability of methods and data may seem a part of being the "senior" author of an article. However, since customs

and order of authorship vary in institutions and countries, the current practice is too vague to allow identification of any but the most eminent authors.

Publicly funded scientific research has so far been a matter of trust. Some members of our government are now concerned that this trust has been abused. It seems incumbent on the scientific community to demonstrate an open, constructive attitude to this criticism and to seek to mitigate its cause.

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Corrections

The Perspective "Gamma-ray observations of orbiting nuclear reactors" by Joel R. Primack (28 Apr., p. 407) incorrectly identified the Gamma-Ray Spectrometer on the Solar Maximum Mission satellite as having been blinded by radiation from Soviet satellites. The seventh paragraph should have read, in part, "Gamma-ray detectors are surrounded by charged-particle detectors, so that events initiated by gamma rays can be distinguished from background events initiated by electrons. But positrons can annihilate on other parts of the gamma-ray detector spacecraft such as the SMM shield, and the resulting 511-keV gamma rays can appear to be astronomical gamma-ray signals. The SMM-GRS picked up such signals an average of eight times per day for much of 1987 and early 1988, each time causing brief interference with astronomical observations. The data storage capacity of the Gamma-ray Burst Detector (GBD) on the Japanese Ginga satellite was sometimes saturated by such events, so that it could take no more data until the next pass over its ground station (which could be on the same orbit or as many as 14 orbits later); this effectively blinded the GBD about 20% of the time. The sensitive detectors aboard Gamma Ray Observatory . . . ." [The remainder of this paragraph is as published.]

In the report "Geomagnetic origin for transient particle events from nuclear reactor-powered satellites" by G. H. Share *et al.* (28 Apr., p. 444), the following corrections should be noted. The last sentence of the second full paragraph on page 445 should have read, "Their report provides detailed confirmation of the origin of the SMM events." On page 446, the last sentence of the caption of figure 3 should have read, "Rate is in counts per 0.5 s." On page 447, text references to figures 2 and 3 were interchanged. The sixth sentence of the fifth full paragraph should have read, "The concentration of particles on this L shell explains the peak observed by the GRS." The fourth sentence of the sixth paragraph should have read, "The spike near 12 min coincides with the time when SMM reached L shells on which positrons had been deposited about a minute earlier." Reference 12 should have read, "*Solar Geophysical Data Prompt Reports, No. 535 (Pt. 1),* H. E. Coffey, Ed. (National Geophysical Data Center, Boulder, CO, 1989)."

In the report "Distribution and detection of positrons from an orbiting nuclear reactor" by E. W. Hones and P. R. Higbie (28 Apr., p. 448), the following corrections should be noted. The first sentence of the caption for figure 1 should have read, "Locations of SMM (dots) and Cosmos 1176 (triangles) at the times of 21 of the most intense 511-keV gamma events recorded by SMM during the 29 April to 2 September 1980 operating period." The first sentence of the caption for figure 3 should have read, "Estimated differential energy spectrum of positrons escaping from Cosmos 1176 per joule of fission energy." On page 450, the second sentence of the first full paragraph should have referred to event 5, not event 59.