

David Baltimore Responds

Nobel Prize winner David Baltimore testified May 4, 1989, before a congressional subcommittee regarding allegations against a scientific paper he coauthored and that was published in Cell in April 1986. The controversy over the paper has engendered a year-long debate about misconduct in scientific research. Dr. Baltimore, director of the Whitehead Institute for Biomedical Research, and a member of both ASBMB and AAI, appeared before the House Energy and Commerce Subcommittee on Oversight and Investigations, which has been investigating the matter. The accompanying text is an edited excerpt of Dr. Baltimore's oral testimony to the subcommittee.

I am a molecular biologist. I have worked for almost 30 years at understanding the processes that allow living systems to function. During that time I have published almost 400 articles, many of which have represented significant advances in the understanding of how viruses grow, what causes cancer, and how our immune systems function. In 1970 I discovered a fundamental property of cancer-causing viruses and 5 years later shared the Nobel Prize for that discovery. That discovery was also the reason that when the AIDS epidemic struck, the scientific community could so rapidly find and characterize the virus that causes the disease: the initial assay for the AIDS virus was based on my 1970 discovery. At the present time, I am working toward a new form of an AIDS vaccine.

Mr. Chairman, there are many scientists here today who are concerned about this investigation and hearing. They argue that it could have a chilling effect on science in America.

The problem, Mr. Chairman, is that many in the scientific community do not understand what you are doing. Think about it. They received a manuscript, based on only 17 pages from a laboratory notebook, implicitly accusing me of fraud. They read in the newspaper a statement of a staff member of your Subcommittee, before any hearings had been held, accusing me of "fraud or misrepresentations." Recently, they have read stories about this Subcommittee calling in the Secret Service to analyze lab notes. Mr. Chairman, you must agree that their concern is understandable.

Let me be clear. I do not question the right of this Subcommittee to conduct this investigation. I do not question the right of this Subcommittee to demand accountability for government funding of scientific research. But I do question, in the most serious way, the manner in which this investigation has been pursued.

Let me address the study. This was a collaboration in the classic sense. I supervised Dr. David Weaver in my own laboratory and Dr. Thereza Imanishi-Kari supervised several scientists in her laboratory. This wasn't our first collabora-

tion, and in previous work together I had learned to trust her. I sought her out because she was among the most qualified scientists in the world to undertake her part of the study.

Her lab and mine kept in close touch throughout the study and published our results in *Cell* in April 1986. I should note that the study, as designed, was consciously redundant so that the one approach would provide a check on the other as well as providing complementary results.

Our results were somewhat surprising and we fully expected that they would invite further work by other scientists. Even Dr. Imanishi-Kari and I had different interpretations of the results, and those differences are reflected in the *Cell* paper.

A few words about Dr. Margot O'Toole. She was not involved in this research, but she had begun work in Dr. Imanishi-Kari's lab as a postdoctoral fellow. Some time after the paper was published, Dr. O'Toole challenged the paper, arguing that the data could be interpreted differently. She took her challenge to Tufts University, which appointed a panel of qualified scientists who reviewed all of the data and ultimately supported our work. I did not know about this at the time because she challenged only the work from Professor Imanishi-Kari's lab.

Dr. O'Toole next took her challenge to MIT. Again, a review, noting that Dr. O'Toole's disagreements were not frivolous and should be tested by further experimentation, supported our work. Let me say that I believe that what Dr. O'Toole did up to this point was healthy and proper and should be enthusiastically encouraged. But at this juncture what was needed to decide the merits of her arguments were new experiments, and those she did not do.

Verdict First, Evidence Later

Enter Mr. Walter Stewart and Dr. Ned Feder of the National Institutes of Health. Neither Stewart nor Feder was an immunologist and neither was qualified to understand the science. They had obtained from Dr. O'Toole 17 pages of the data from the *Cell* study—out of a thousand or so pages gathered. Some of these 17 pages had to do with failed experiments, something that isn't unusual in science. None of the data from my laboratory was involved.

On the basis of these 17 pages they wrote a long manuscript attacking the *Cell* article. They also began a campaign to discredit me, my work, and to obtain evidence to support their faulty conclusions. This is a classical case of verdict first, evidence later.

The manuscript was rejected by at least three journals. In response, Stewart and Feder mailed their manuscript to numerous scientists across the country. They peppered me with calls and letters demanding an audit of the data. Stewart and Feder also prompted calls to me from reporters, which

increased the pressure. I refused to cooperate with them for two main reasons. First, they had already reached their conclusions. Second, they were absolutely unqualified to evaluate the science of this paper.

The campaign they waged against me was so vitriolic that in 1987 I requested that the NIH investigate the matter formally and that all concerned agree to abide by the results of this review, which, I hasten to remind you, would have been the third review. Stewart and Feder said no, insisting that they should be part of the review despite their announced prejudice.

Next, Stewart and Feder apparently convinced the staff of this Subcommittee that the verdict they had reached was correct. The staff thus joined them in this relentless campaign. The Subcommittee called a hearing in April 1988 in which the accusers were allowed to testify but those of us accused were not given an opportunity to tell our story nor were we even notified of the hearing.

After the hearing, NIH finally appointed an investigatory panel and reviewed all the relevant data and issued its draft report. This report concluded that there was nothing to suggest fraud or misrepresentation but raised the issue of certain errors and ambiguities. The director of NIH accepted the report and requested that the authors amplify several points in letters to *Cell*, which has been done. Nothing the NIH found altered the conclusions of the paper.

But even this third report was not enough for Stewart and Feder. At a public conference, while in the service of this Subcommittee, Stewart made the loathsome comparison of scientific fraud, of which he accuses me, to the Nazi Holocaust. On Tuesday of last week we learned that even the U.S. Secret Service had been brought in to bolster their allegations.

The Secret Service apparently conducted a 9-month forensic analysis of Dr. Imanishi-Kari's lab notes. In a charade of helpfulness, they presented a partial oral summary of their findings on Tuesday, April 25. That presentation was designed to terrify without providing any substance. Finally, last Sunday some written materials were provided. Based on those and what I have heard today, there is nothing from the Secret Service investigation that causes me to doubt the validity of the *Cell* paper.

In their investigation, the Secret Service came up with only one question that relates directly to data from my laboratory. They discovered that Figure 4 in the paper was a composite of different exposures of a single autoradiogram. I could have told them that! This is common practice in presenting such data. I have included, in my prepared statement, an attached sworn affidavit from Ben Lewin, Editor of the journal *Cell*, affirming that *Cell* customarily publishes such composites and requires no explanatory footnote or statement.

I must tell you, Mr. Chairman, I am very troubled about how this situation got so out of hand. I have a very real concern that American science can easily become the victim of this kind of government inquiry. But Professor Imanishi-Kari is also a victim. English is her fourth language. She has difficulty communicating in English, as the history of this controversy painfully shows. She deserves my support and the support of all scientists, for any of them could be in her shoes.

It was in fact the difficulty communicating with Dr.

Imanishi-Kari that gave rise to a misunderstanding between Dr. Herman Eisen and me that in turn led to a letter from me to Dr. Eisen, a letter of which I am not proud. In September 1986 Dr. Eisen had a chance conversation with Dr. Imanishi-Kari and thought he heard her say that a reagent used in the study didn't work. He told me about it and, instead of calling Dr. Imanishi-Kari and asking her about it, I fired off a letter to Dr. Eisen based only on this misunderstood statement and suggesting that since the supposedly faulty reagent wouldn't change the paper's conclusions, we not formally retract the paper but only acknowledge the error to fellow scientists. This was bad judgment, and I'm confident that if I had thought about it a little longer, I would have wanted to write a letter to *Cell*. However, Dr. Eisen spoke to me within a day or two and explained that he had misunderstood Dr. Imanishi-Kari. The reagent was very well suited to its task, although some misprepared batches did not work. So I forgot about the letter to Dr. Eisen, until it turned up in a search for documents to send to the Subcommittee.

I trust that the Subcommittee understands my profound regret for writing this letter and will accept the statement that I fully understand that when a serious error has been made, it must be fully acknowledged.

Legitimate Disagreements

So, what is the case against us? That there are legitimate disagreements over the results of our work? This I recognize and encourage. That we were not as perfect as, in hindsight, we might have been? This I concede. That scientists keep notes in ways that are sometimes orderly and sometimes not? Of course. That we committed fraud, created data, or misrepresented the facts? This I reject categorically. The Subcommittee can point to nothing that supports these allegations.

What about the science reported in the *Cell* article? We have all been so consumed by the pyrotechnics of this hearing that maybe we have lost sight of the most important and central issue before us. The *Cell* paper has fared very well in the 3 years since its publication. As I mentioned before, no result of the paper has been proved wrong, a number of results have been replicated, and significant progress has been achieved by scientists building on the paper's conclusions.

In closing, I should like to reflect on what I have learned from this affair. First, having been forced to think through how scientific data is verified, I am committed more than ever to the classic mode of verification by scientific replication and further development. Second, I realize that with the increased sensitivity about scientific fraud, it is more crucial than ever that scientists keep records that can easily be referred to, so that the scientific basis for published results is easily determined at any time. Third, it is important that the institutions that support science—the universities, the research institutes, and the NIH—respond quickly to any allegations of irregularities in the scientific process, and in a way that safeguards the reputation of those accused as well as those who accuse. Fourth, scientists and politicians must figure out a better way to communicate our concerns than through headlines in newspapers. We share the same goals and aspirations for scientific research in this country and we simply must develop a more productive way of sharing our ideas and problems.