Baltimore Throws in the Towel

The Nobel laureate abandons Thereza Imanishi-Kari and apologizes to Margot O'Toole—but his critics say his statement doesn't go far enough

TO MANY SCIENTISTS IT MUST HAVE SEEMED a stunning about-face. After 5 years of vigorously defending a 1986 *Cell* paper now deemed by the National Institutes of Health to contain fraudulent data produced by Tufts immunologist Thereza Imanishi-Kari (*Science*, 29 March, p. 1552), David Baltimore issued a statement to the media last week apologizing for "failing to heed the warnings" about the paper and doing "too little to seek an independent verification of [his coauthor's] data and conclusions." The Rockefeller University president and Nobel laureate also extended an extraordinary apology to Margot O'Toole, the former Imanishi-Kari postdoc who first challenged the *Cell* paper but who was rebuffed by Baltimore. Now the eminent biologist commends her for her "courage and determination" and expresses regret for "my failure to act vigorously enough in my investigation of her doubts."

> ***"I appreciate Dr. Baltimore's words of praise for me, but his apology does not go to the heart of the matter."***
>
> —MARGOT O'TOOLE

Baltimore's official reply to the harsh verdict of NIH's Office of Scientific Integrity last month wasn't all mea culpas. In addition to the statement that appears on p. 769, it includes a 6-page critique of some of the conclusions reached in OSI's 2-year inquiry into the paper. Baltimore disputes OSI's inclusion of several damaging remarks attributed to him, claiming that they were taken out of context. Specifically, Baltimore claims that comments such as "If those data were not real, then she (Dr. Imanishi-Kari) was driven by the process of investigation into an unseemly act" were "taken unfairly from an informational interview and do not appropriately represent my views." Baltimore also contests what he calls a "strongly worded attack" in the report's conclusions, arguing that OSI needlessly used "inflammatory language" in stating its determinations.

Despite the caveats, Baltimore's submission to NIH was just that: an act of submission. Aides to Representative John Dingell (D–MI), long a champion of O'Toole, say the credit for the conciliatory approach belongs to Benjamin Civiletti, a former U.S. attorney general hired by Baltimore just over 3 weeks ago in place of his previous legal advisers. "Civiletti met with us and basically said, 'Hey, we're a new team and we've got a new approach'," says a Dingell staffer. "We assume he sat down with the evidence and then told Baltimore, 'Fold your tent.'" Civiletti says only that Baltimore hired him to "review the prior inquiries and the history of the case."

Wide World Photos

Will the Nobelist achieve his goal of putting the matter behind him? Such a hope seems unlikely, as many Baltimore critics remain far from satisfied with the statement. Predictably enough, the harshest reaction was from O'Toole herself, who issued a strongly worded statement that read in part: "I appreciate Dr. Baltimore's words of praise for me, but his apology does not go to the heart of the matter....Dr. Baltimore's 1986 investigation was complete enough to discover that my objections were substantiated. However, he did not act upon them then."

In a brief interview with *Science*, Baltimore said it was difficult to examine Imanishi-Kari's data because she kept it in such a disorganized fashion. "[W]e talked about data, and I saw data, but...in retrospect, the real problem was that she didn't have an orderly way of maintaining her notes so that they were accessible in a routine form. I was simply too willing to excuse her disorganization." He added, "When we dealt with this problem in 1986, there was clearly too much informality, and not enough rigorous assessment of what the problem actually was."

In her statement, however, O'Toole reiterates a charge she first made publicly 2 years ago—that when she met in June 1986 with the paper's authors and Herman Eisen, an MIT immunologist charged with investigating O'Toole's challenges to the *Cell* paper, Imanishi-Kari "candidly admitted that she had not obtained results reported in the paper." According to O'Toole's statement, Baltimore then told her that "this kind of thing" was not unusual, and threatened to oppose any attempt O'Toole might make to correct the paper.

On at least one other point, O'Toole's statement refuses to let Baltimore's apologia rest. In a May 1989 congressional hearing called by Dingell, Baltimore dismissed Secret Service forensic evidence suggesting that data had been falsified. But Baltimore's statement now admits to a belated realization that "the better course would have been to suspend further comment on the matter" until he had taken time to study the new evidence. O'Toole, however, points out that she had charged in a letter to NIH 6 months earlier that Imanishi-Kari was defending the paper with two sets of data from experiments that O'Toole said had not been done—one of which OSI subsequently found to be fabricated—and that Baltimore had to be aware of her charges.

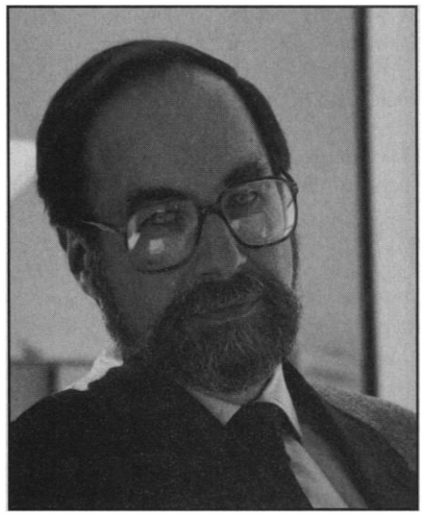
One of the handful of scientists who have publicly criticized Baltimore's handling of the case, Harvard molecular biologist and Nobel laureate Walter Gilbert also levels a strong indictment at Baltimore. "The thing that's profoundly wrong with David's statement is that there has been a problem in this [paper] all the way through.... [B]y early

David Baltimore's Mea Culpa

The following is the statement David Baltimore sent to NIH:

A draft report of the investigation conducted by the Office of Scientific Integrity (OSI) of the National Institutes of Health into the 1986 *Cell* article on transgenic mice has been issued. I welcome OSI's report because of its completeness and detail. I have now had the opportunity to study OSI's findings and to reflect upon the inquiries and proceedings related to both the paper and the data and experimentation that supported the paper's conclusions.

After an exhaustive review of forensic and scientific evidence, OSI has concluded that certain data contained in the notebooks of one of the paper's authors, Dr. Thereza Imanishi-Kari, were falsified and/or fabricated, and that in relying upon such data, Dr. Imanishi-Kari presented false and misleading information to the NIH panel charged with investigating the accuracy of the data and interpretations in the paper. I wish to state that if Dr. Imanishi-Kari did falsify data or make misrepresentations, I had no knowledge of the misconduct.



"I commend Dr. O'Toole for her courage...and I regret and apologize to her for my failure to act vigorously enough in my investigation of her doubts."

The findings do not undermine either the integrity of the work conducted by my postdoctoral fellow, Dr. David Weaver, under my supervision or the reliability of our records. However, the OSI was critical of my response to the mounting challenges raised to the work of Dr. Imanishi-Kari, my coauthor.

The completion of the NIH investigation has prompted me to make these comments, which will address OSI's observations about my conduct and will share the lessons I have drawn from this experience about the appropriate response to such allegations and the respect and candor which must characterize the partnership between the scientific community and the federal government.

OSI criticizes me for my strong defense of Dr. Imanishi-Kari, particularly at the May 1989 hearings before the congressional subcommittee, and for my failure to reexamine Dr. Imanishi-Kari's data more critically after serious questions had been raised. I wish to state at the outset that my defense of Dr. Imanishi-Kari was not due to any lack of regard for Dr. Margot O'Toole, the postdoctoral fellow who first uncovered certain discrepancies in Dr. Imanishi-Kari's research. I have tremendous respect for Dr. O'Toole, personally and as a scientist, and I have consistently maintained that I believe that her analyses were insightful, her expressions of concern were proper and appropriate, and her motives were pure. Rather, my defense of my coauthor was fueled by my respect for Dr. Imanishi-Kari's demonstrated abilities as a scientist, by my belief that the paper's scientific conclusions were sound, and by my trust in the efficacy of the peer review process.

The study that gave rise to the paper was conducted as a classic collaboration, with each laboratory performing independent research in its particular area. Mutual respect is the bedrock of any professional collaborative effort, and it was a key ingredient in our particular collaboration, because Dr. Imanishi-Kari provided the expertise in serology that I lacked—she possessed proven ability in this field.

Dr. O'Toole initially brought her concerns to immunologists at Tufts University. Those experts concluded in June 1986 that there was no evidence of deliberate falsification or misrepresentation and characterized the availability of alternative interpretations of the data as "the stuff of science." A later review at MIT reinforced that conclusion. The expert there found that Dr. O'Toole had correctly identified a minor error, but explained that the error was too insignificant to warrant a retraction in light of "a substantial body of other data that is clear and impressive." The MIT report echoed the sentiments of the Tufts reviewers and noted that "other issues raised by O'Toole, which are largely matters of interpretation and judgment, are best dealt with by allowing the scientific process to take its course." I fully expected that this paper, like all others, would be subjected to the rigors of the scientific peer review process, and that efforts by other laboratories to replicate or extend our findings would ultimately test whether they were correct.

In January 1989, the first NIH panel to investigate the matter concluded, as Tufts and MIT had, that the science in the paper was essentially sound. The report did, however, raise many issues about the way the data for the paper was produced and, in retrospect, it is evident that I gave too much weight to the overall conclusions of the report and did not appreciate that the report might have had the implication that the results had not been obtained as reported.

In 1989, during hearings before the congressional subcommittee investigating the matter, the Secret Service revealed certain preliminary findings regarding its ongoing review of Dr. Imanishi-Kari's notebooks. At that time, I continued to base my defense of Dr.Imanishi-Kari upon the two university reviews, the January 1989 NIH report, and my knowledge of her abilities. I now realize that I erred in failing to heed the warnings and that the better course would have been to suspend further comment on the matter until I had a full opportunity to review and digest all of the new information.

In good conscience I feared a rush to judgment, and I accorded my colleague the benefit of every doubt. I now recognize that I was too willing to accept Dr. Imanishi-Kari's explanations, and to excuse discrepancies as mere sloppiness. Further, I did too little to seek an independent verification of her data and conclusions. I acknowledge that, for too long, I focused narrowly ➤

on the question of whether the paper could stand; what was important to me was that the solid molecular data gathered by my laboratory seemed to lend credence to the serological findings. In other words, as a scientist, my concern was always for the science: Is the result correct? Can it be replicated and built upon?

The OSI report raises very serious questions about the veracity of the serological data. I am shocked and saddened by the revelations of possible alteration and fabrication of data. These discoveries are deeply troubling not only because of their impact upon our article, which has been retracted in light of these revelations, but because such allegations of fraud undermine public confidence in the entire scientific community. Science must be an objective search for truth. It was my belief in science and faith in my fellow scientists which led me to set my threshold of suspicion so high.

I wish to state unequivocally that I have never condoned falsity by a scientist. I do not believe it could ever be appropriate to represent that a test which was not performed was in fact completed, or that anything other than the actual results were obtained. Fraud in the laboratory is not only wrong from a moral and legal standpoint, but it impedes the progress of science, as it makes the review and retesting of hypotheses and conclusions impossible. Deliberate falsification demeans all members of the scientific community because it undermines public trust and confidence in our enterprise.

For their work, scientists are entrusted with public funds. I have come to better appreciate the legitimate role of government as the public sponsor of scientific research and to respect its duty to protect the public interest and hold the scientific community accountable for its stewardship of public funds. Such accountability can be entirely consistent with the essential objectivity of scientific inquiry.

This case has highlighted the need to conduct our research and review in a manner the public can appreciate, because continued public support is necessary for the continued life of the scientific enterprise and the nurturing of the academic environment in which we enjoy the freedom to experiment and learn. It is only because public support has been translated into federal financial support that scientists have been able to expand dramatically the range of human knowledge and apply this new knowledge to achieve extraordinary practical advances in such fields as medicine and public health. In light of this creative partnership, I remain firmly committed to the importance of governmental oversight of federally funded projects, and I look forward to continuing to participate in a healthy and necessary dialogue to improve the process.

I have learned from this experience that the accountability to ensure the responsible use of public funds rests not only with each individual scientist but with the scientific and academic communities as a whole. Better self-policing and record-keeping will facilitate the government's oversight function and may obviate the need for the repeated hearings and investigations that were needed in this case. This matter has also highlighted the need for clear procedures which guarantee the prompt and thorough investigations of allegations, and I hereby commit myself to participate actively in the study and formulation of new guidelines. Questions raised, whether by junior or senior scientists, must be pursued with vigor, and since junior colleagues may be reticent to allege outright misconduct, it is incumbent upon those more senior to press for a full airing of their suspicions. Any procedures must include the means to protect those who raise concerns from retribution or discrimination. Scientists must ensure that they do not wait too long or set the threshold too high before calling for the application of close scrutiny to ferret out potential falsity. Finally, the questions raised in this investigation have also underscored the need for greater attention to detail in the handling and recording of data, to further effective peer review and to establish an impeccable record for verification of results.

In conclusion, I commend Dr. O'Toole for her courage and her determination, and I regret and apologize to her for my failure to act vigorously enough in my investigation of her doubts. I recognize that I may well have been blinded to the full implications of the mounting evidence by an excess of trust, and I have learned from this experience that one must temper trust with a healthy dose of skepticism. This entire episode has reminded me of the importance of humility in the face of scientific data.

1989, when he attacked the committee, it was clear that there was a fraud involved," Gilbert says. "He'd been told that, and even if he did not believe it, there was still a prima facie case to be answered. And yet he chose at that time to do an all-out attack on the committee.... [H]e simply refused to notice what was happening—and that's the best interpretation you can put on it."

Most of Baltimore's supporters, however, appear to be sticking by him. "I am surprised that some scientists have considered David Baltimore's apology inadequate, and I find it hard to see what they hope to achieve," says Harvard microbiology professor emeritus Bernard Davis. Warning that the case could lead to a "destructive inroad of bureaucracy in science," Davis argues that whatever Baltimore's errors, "it would be a tragedy if it should diminish the ability of society to benefit from his outstanding talents."

For now, at least, Baltimore appears to have the backing of Rockefeller's board of trustees. Richard Furlaud, chairman of the board, said that after the trustees' scientific affairs committee had reviewed the entire controversy in a meeting last week, it reaffirmed its support for Baltimore's presidency. "The question of his resignation was never an issue," Furlaud says. While the trustees and administration officials express unwavering support for Baltimore, faculty sentiment is harder to gauge. "I am sure if you were to take a secret ballot—emphasis on secret—a majority would ask for his resignation," says one faculty member who objected to Baltimore's appointment as president a year and a half ago.

Such support from the trustees could vanish abruptly if Baltimore becomes mired any more deeply in the controversy—one reason, perhaps, that he seems to have given up the fight. But Baltimore's statement and the new openness of his legal team have so far failed to remove the threat of action by the Dingell committee. "I agree with Mr. Baltimore that we need a major cleanup of the way science handles these matters of fraud," Dingell said in a statement. "I very much regret that reaching those conclusions has been so long and painful for some." Aides to the congressman suggest, however, that more revelations could be on the way. Although close-mouthed about the exact nature of these surprises, the aides are quick to admit their interest in the question of just how carefully Baltimore examined O'Toole's original challenge to the paper. The issue should be aired fairly soon: Dingell's committee is putting the final touches on a report about what it calls "the coverup," and plans to hold two more hearings on the case, perhaps by late June or early July.

■ DAVID P. HAMILTON

With reporting by Karen Wright.