Dingell *v.* Baltimore

Congress to revisit past year's controversy over paper coauthored by Nobelist David Baltimore; NIH will be grilled on ability to investigate allegations of misconduct

No one ever writes about John Dingell without referring to the Michigan congressman as the "powerful" chairman of the House subcommittee on oversight and investigations—a subcommittee that has been described as the "grand jury for the nation." *Newsweek*, which recently reported on the "Capitol campus," cited Dingell as the "class bully" for his rough committee hearings which have many of the trappings of a criminal trial.

Dingell has taken on Defense Department contractors and uncovered fraud. He has enhanced his reputation as a legendary investigator through his persistent probing of Drexel Burnham Lambert, Inc., the huge Wall Street firm known for its skill in corporate takeovers. But he was put on the spot recently in the Drexel case by a zealous aide who is accused of breaking the law to get evidence against a private detective.

Next week, John Dingell will take on Nobel laureate David Baltimore at a hearing billed as an inquiry into the process of science. A clash between two cultures seems inevitable. The principals in the case, including people from Tufts, the Massachusetts Institute of Technology, and the National Institutes of Health, suspect that Dingell's unstated agenda is to accuse Baltimore or one of his colleagues of fraud in a controversial paper on gene regulation in immunology. Dingell has scheduled 2 days of hearings to examine the now famous case of the "Altered repertoire of endogenous immunoglobulin gene expression in transgenic mice containing a rearranged Mu heavy chain gene"—a paper published in *Cell* 2 years ago this month.

Anyone who is the target of a Dingell hearing is well advised to consult a lawyer. Baltimore has two: One with a Washington firm experienced in representing people before Dingell, the other in Boston.

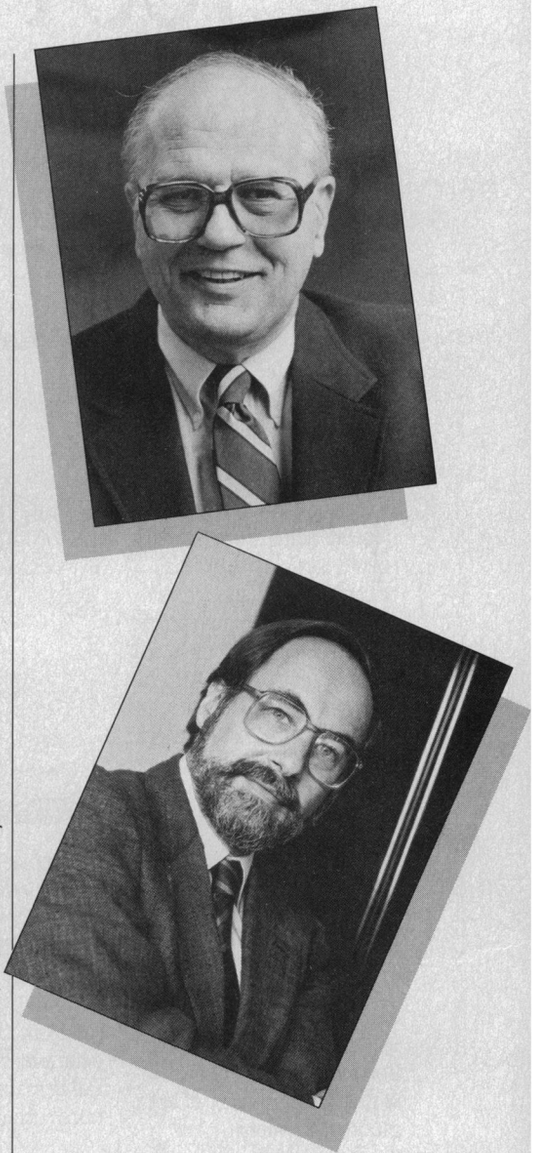
Baltimore says he feels "hounded" by Dingell. Many leading researchers see Dingell's pursuit of the Baltimore case as a way of attacking science itself. And many scientists are expecting the worst—in part because Dingell has not yet disclosed his agenda and a good deal of preparation for the dread hearings is based on rumor and suspicion. The subcommittee, which can subpoena that which is not "volunteered," has demanded scores of documents and, reportedly, has even called in the U.S. Secret Service to check their validity. Several people who are expected to be called as witnesses but have yet to hear from the subcommittee are waiting apprehensively for the phone to ring.

Part of the Dingell agenda is on the table. In an interview with *Science*, a staffer for the oversight and investigations subcommittee said the hearings will examine whether institutions can respond adequately when allegations of fraud or even error are raised. The subcommittee has trouble accepting the idea that scientific institutions can investigate themselves without running smack into conflict-of-interest issues that may compromise the objectivity or vigor of the investigation.

In the Baltimore case, a postdoc's challenges to the validity of the *Cell* paper were probed by researchers at both Tufts and MIT. Each acknowledged points of scientific difference; neither detected any evidence at all of fraud or misconduct. A subsequent investigation by an NIH panel also vindicated Baltimore and the others, while taking issue with the *Cell* paper on a couple of technical, scientific grounds. The NIH panel also reported that there were clear errors in the paper—errors which were corrected by one letter to *Cell* and will be further clarified in a second letter that will appear in the 19 May issue. The NIH review which, it was hoped, would bring the case to a close, is likely to come in for criticism by the subcommittee. Postdoc Margot O'Toole claims that NIH produced a "wholly inadequate scientific analysis of the facts." The three NIH panelists will be called before Congress to defend their report.

Each of the witnesses may be grilled about their handling of whistle-blowers. Many of Margot O'Toole's contentions about errors in the *Cell* paper have been substantiated since she first raised them in 1986. Where, asks a Dingell aide, is the paragraph in the report that praises her for daring to challenge her seniors? Or worse, "why was there once such a paragraph that was taken out?" Was there?

These questions are difficult enough. The scientists worry that their complex and nuanced answers will pale before chairman Dingell's blunt and selective questioning.



Congressman John Dingell *will take on* **David Baltimore** *over research paper and treatment of whistle-blower.*

But behind the questions of institutional conflict-of-interest and the honor accorded whistle-blowers is the specter of fraud. *Science* has learned that the subcommittee has launched a sweep for documents, presumably looking for a smoking gun. The principal cop on the case is Walter Stewart, an NIH researcher whose main line of work

The Story So Far—Chronology, Dramatis Personae

The following partial chronology and cast of characters reveals something of the ins and outs of what has become known as the "David Baltimore" case since it began 2 years ago, when a postdoc went to her superiors to say that the data in a published paper were in "error." The postdoc resolutely denied making allegations of fraud, but in the press and the Congress the line has blurred. The scientific community says that if allegations of error can end up in Congress, the entire scientific enterprise is in jeopardy, because you cannot do good science without making errors. The current case began in 1986.

April 1986. *Cell* publishes in its 25 April issue a paper called "Altered repertoire of endogenous immunoglobulin gene expression in transgenic mice containing a rearranged Mu heavy chain gene," by David Weaver, Moema H. Reis, Christopher Albanese, Frank Costantini, David Baltimore, and Thereza Imanishi-Kari.

May 1986. Margot O'Toole, a postdoc in Thereza Imanishi-Kari's lab, approached two Tufts colleagues with doubts about the validity of the *Cell* data: first, Brigette Huber, and then her mentor, Henry Wortis. Wortis convened an informal ad hoc committee—himself, Huber, and Robert Woodland, a visiting scholar in the lab—to check things out. Wortis gave a verbal report of the committee's findings to his superiors at Tufts. The report: a scientific disagreement, important, but nothing more than that. O'Toole's complaints about the data also go to MIT officials, where the dean asked immunologist Herman Eisen to conduct an informal investigation. Eisen met with O'Toole at his home in Woods Hole in late May and asked for a written summary of her concerns.

6 June 1986. O'Toole submits a five-page memo to Eisen, detailing her scientific arguments.

16 June 1986. Eisen meets with Baltimore, Weaver, Imanishi-Kari, and O'Toole.

17 June 1986. Eisen writes a memorandum for the record acknowledging the validity of some of O'Toole's points but generally concluding that they do not add up to an invalidation of the *Cell* paper and, generally, reveal errors too minor to warrant a published correction.

Summer/fall 1986. A colleague of O'Toole's contacts fraud-busters Walter Stewart and Ned Feder at NIH, soliciting their investigative expertise. Stewart and Feder subsequently draft a manuscript that is extensively examined by NIH before it is eventually submitted for publication and rejected by *Science*, *Nature*, and *Cell*.

Late 1986–early 1987. Stewart and Feder ask Baltimore and coauthors for access to their original data. Baltimore *et al.* refuse. Says Baltimore, "Your notion of doing an 'internal audit' of the data is not one I can accept. Such a principle, if established, would tie up the scientific community in continuous wrangles." Stewart and Feder also ask for copies of the Wortis and Eisen reports.

17 March 1987. Baltimore writes to NIH asking for an official review by an NIH-appointed panel because Stewart and Feder remain "unsatisfied."

May 1987. Unable to get their manuscript published, Stewart and Feder circulate a letter outlining their difficulties and summarizing the situation as they see it to some 100 leading U.S. scientists. NIH officials, Baltimore, Wortis, *et al.* come out looking like the bad guys.

11 April 1988. Representative Ted Weiss (R–NY) holds a hearing at which O'Toole, Stewart, and Feder are among the witnesses.

12 April 1988. Representative John Dingell (D–MI) holds a similar hearing, including O'Toole, Stewart, and Feder. Baltimore *et al.* are not invited to either hearing.

June 1988. NIH appoints a panel of experts to review the science. They are Joseph M. Davie of Searle, Hugh McDevitt of Stanford, and Ursula Storb of the University of Chicago. The panel is promptly dispatched to Boston to interview the principals and a report is promised "within weeks."

November 1988. A draft of the panel report is circulated. Baltimore, O'Toole and others reply. Baltimore challenges some of the fine points of the panel's scientific observations but applauds its finding of neither fraud nor misconduct. O'Toole challenges the whole thing.

January 1989. NIH releases a "decision memorandum" signed by director James B. Wyngaarden that clears Baltimore *et al.* of fraud or misconduct but accepts the NIH panel's findings of serious errors in the *Cell* paper.

May 1989. Congressman John Dingell will hold hearings to review the entire case. ■ **B.J.C.**

these days is fraud-busting. At Dingell's request, NIH has loaned Stewart to the subcommittee where he has been for several months. Stewart and his NIH colleague Ned Feder have been pursuing Baltimore from the start of this affair.

For years, Baltimore has received support from the American Cancer Society. *Cell* coauthor Thereza Imanishi-Kari has applied for a cancer society grant for research related to the disputed work in gene regulation and immunology. Although she did not get one (she did get support from NIH), the subcommittee wants to know what she wrote in her application. Thus, it has asked the society for copies of all correspondence with Baltimore and with Imanishi-Kari dating back to 1982. There is particular interest in a letter from Imanishi-Kari dated 25 January 1985 that allegedly contains data that conflict with data published in *Cell* the following year. "When Walter Stewart asked for things, he specially mentioned that letter," Imanishi-Kari reports. "My recollection is that I did have data that is in the *Cell* paper." She has no copy of the letter.

"It's almost as though they want to create a feeling of fear." Rumors that Secret Service experts will testify that some of her notebooks have been altered or written post facto have only added to the apprehension. Dingell's people decline to comment.

Another document sweep that is a source of concern is the subcommittee's demand for letters and lab notes from Frank Costantini of Columbia University who, until now, has been left out of this fight. Costantini developed the transgenic mice that are at the experimental heart of the *Cell* paper. He has had to turn over copies of his correspondence with Baltimore, Imanishi-Kari, and with any and all of their lab partners at the time the *Cell* experiments were going on. What, if anything, they show is not yet known.

And *Science* has learned that a confidential letter from Baltimore to Herman Eisen of MIT is in the subcommittee's hands. In May 1986, the dean of MIT asked Eisen to conduct an informal but officially authorized review of O'Toole's complaints. One of them focused on the fact that a particular reagent called Bet-1 did not, in fact, work the way Thereza Imanishi-Kari claimed it

did in the *Cell* paper. Somewhere along the line that summer, Imanishi-Kari told Eisen that she knew Bet-1 did not work the way she claimed. At least, that is what Eisen thought he heard Imanishi-Kari tell him. Eisen told Baltimore.

On 9 September 1986, Baltimore wrote, "The evidence that the Bet-1 antibody doesn't do as described in the paper is clear. Thereza's statement to you that she knew it all the time is a remarkable admission of guilt." Adding that neither he nor principal author David Weaver knew anything about it, Baltimore wrote, "Why Thereza chose to use the data and to mislead both of us and those who read the paper is beyond me."

In the same letter Baltimore counsels against a public retraction for two reasons: (i) Bet-1, good or bad, is not central to the overall conclusions of the paper. (ii) "A retraction would be difficult because David Weaver would be identified as senior author and he really had nothing to do with those data."

There is, Eisen says, a perfectly good explanation. Imanishi-Kari, whose native tongue is Portuguese, is notoriously difficult to understand. When questioned about the astonishing admission about Bet-1, Eisen says, it became clear he had misunderstood her the first time around. The truth, says Eisen, is that Imanishi-Kari never said Bet-1 was no good. Rather, she said that she knew how difficult a reagent it is and that some preparations of the antibody assay were better than others. "Further discussion [in September 1986] with Imanishi-Kari made it clear that though she knew all along of Bet-1's difficulties there was no doubt that good preparations did, indeed, have the properties . . . [described in the paper]," Eisen wrote in a memorandum.

In a recent telephone conversation, Baltimore told *Science* that he regarded his letter to Eisen as "an extension of a conversation" the two had had the day before. "If it really had been true that Thereza said Bet-1 was not good, we would have had to write to *Cell*. I would have come to that opinion myself," Baltimore said, "even though it might have hurt David Weaver. But it didn't come to that." (Weaver did the molecular biology in the study; Imanishi-Kari was the expert for the serology.)

Anticipating a rough time for Baltimore *et al.*, one of his colleagues at MIT has launched a campaign to enlist the support of the scientific community nationwide—a preemptive strike aimed at getting Dingell to back down. In a "Dear colleague" letter, Phillip A. Sharp has asked scientists to help "in countering the continuing activities of Representative John Dingell's subcommittee in Congress." *Science* has a copy of that letter, in which Sharp says "It seems obvious that the congressional subcommittee has decided to continue to hassle David and the other authors and this has serious implications for all of us." Sharp is urging letters to every member of the Dingell committee, as well as newspaper editors around the country, and has offered a sample letter. "The most serious aspect of the subcommittee's actions is that they have repeatedly rejected the judgment of qualified scientists . . . " in this matter. If Dingell cannot be stopped by scientists maybe he can be brought around by congressional colleagues. A special effort is being made to contact the Republican minority on the subcommittee for support.

As *Science* goes to press, it is too early to gauge the response to Sharp's call for an outpouring of outraged support. But some are saying Dingell will become the Joe McCarthy of science, because the case is being exaggerated out of all proportion to its significance.

The subcommittee, however, views the notion of a preemptive strike with what might be called bemused scorn.

Furthermore, Dingell staffers vehemently reject the common perception that by pursuing the Baltimore case, Dingell is out to get science. "Hell, Dingell's father was involved in setting up NIH," one staffer told *Science*. "His brother works there. Dingell has always been a big supporter of NIH, but he sees a problem" and he wants to resolve it. "These hearings will be more than fair. You'll see." ■ **BARBARA J. CULLITON**

Previous Science *articles on this case include the following: "Baltimore cleared of all fraud charges," 10 February, p. 727; "A bitter battle over error," part I, 24 June 1988, p. 1720; part II, 1 July, p. 18.*

Wyngaarden to Leave NIH

James B. Wyngaarden, director of the National Institutes of Health since 1982, will leave NIH at the beginning of July. Wyngaarden announced his departure to NIH senior staff late on the afternoon of 20 April, just after Health and Human Services secretary Louis Sullivan called to say that President George Bush wants his own appointee in the job.

Wyngaarden has been saying privately for months that he was ready to yield the director's post. "The pressures are utterly relentless and wearing," he says, as is the frustration of not having nearly as much authority as the director is credited with having. At one point, he said in an interview with *Science*, he had mentally set November 1988, when his pension became vested, as a departure date. But several factors compelled him to stay on. "I wanted to see the human genome program get off the ground," he said. "The fraud in science issue was heating up last summer and I thought we had to reorganize NIH's offices on that. And Vince DeVita resigned as director of the cancer institute. I didn't want to leave NIH with two presidential-appointee slots vacant at the same time." So he put off leaving but, he says, "it became clear around the time of the inauguration that the President wanted turnover in this office."

Did abortion figure in Bush's decision? "It never came up," Wyngaarden says.

The President has agreed to conduct a traditional academic search for Wyngaarden's successor and a committee will be named to work under the assistant secretary for health. Presidential science adviser designate, D. Allan Bromley of Yale, is also expected to play a role in the decision and he has begun contacting people for names of candidates.

If a present NIHer gets the job, bets are that it will go to Anthony S. Fauci, AIDS scientist and director of the National Institute of Allergy and Infectious Diseases. During a campaign debate, the President cited Fauci as one of his "heroes" and there have been rumors about Fauci moving up ever since.

What next for Wyngaarden? One possibility is that he will return to Duke, where he was professor of medicine for more than 20 years before coming to NIH. Another is a yet unidentified position in science policy. "I'm very concerned about what's happening to science," he says. "The anti-intellectualism, fraud issues, animals in research, fear of recombinant DNA—all of these need to be dealt with." For the moment, he's open. ■ **BARBARA J. CULLITON**