Nobelist Entangled in Fraud Case Resigns as Head of Rockefeller U.

President of Rockefeller U. Retracts Scientific Paper That NIH Office Says Contains Fabricated Data

By DAVID L. WHEELER

David Baltimore, president of Rockefeller University, last week retracted a scientific paper that has been the centerpiece of a three-year Congressional investigation into scientific fraud.

Mr. Baltimore, a Nobel Prize ... had long defended the ...

'I Am Innocent,' Embattled Biologist Says

The Baltimore affair

Ignoble

The New York Times

A Scientific Watergate?

Five years after disturbing questions were raised about a research paper written in part by the Nobel laureate David Baltimore, the celebrated case is finally moving toward a verdict. Federal investigators have concluded, in a draft report, that the paper contained fraudulent data and ...

Hero in Exposing Science Hoax Paid Dearly

By PHILIP J. HILTS

WASHINGTON, March 21 — When Dr. Margot O'Toole, a junior researcher in molecular biology, raised uncomfortable questions in 1986 about the validity of a senior colleague's work, she felt alone.

Dr. David A. Baltimore, a Nobel laureate who was a co-author of a research paper that used the disputed work, described her as a "disgruntled postdoctoral fellow." She lost her job and her house and feared that her husband's job was in jeopardy as well. She took work answering phones at her brother's moving company.

Dr. Baltimore, who is president of Rockefeller University in New York, was a senior adviser on the paper who

For challenging her boss's data, a scientist lost her job and home.

Did Imanishi-Kari Get a Fair "Trial"?

The National Institutes of Health's Office of Scientific Integrity (OSI) must be reeling under the onslaught of bad publicity it has received in recent weeks. A group of 143 scientists, including some eminent ... nologists, recently compl... denied basic rights ... Imanishi-K...

US finds fraud in research at MIT

Report also assails Nobel laureate

By Peter G. Gosselin
GLOBE STAFF

Nobel Scientist Apologizes to Whistle-Blower

Co-Author of Flawed Study Concedes His Oversight Was Inadequat[e]

Associated Press

David Baltimore, a Nobel laureate who was co-author of a study that prompted a prolonged investigation of possible scientific misconduct, has apologized to the young scientist who prompted the investigation and admitted that he made mistakes in defending the flawed work of a co-author.

Baltimore, now president of Rockefeller University in New York

nology, where O'Toole was her research associate.

When O'Toole challenged the accuracy of data reported by her boss in the article in Cell, there were investigations first at MIT and later at Tufts University, where Imanishi-Kari now works.

The university investigations uncovered only minor errors. O'Toole was fired.

When NIH and the House Energy

Office of Scientific Integrity co[n]cluded that Imanishi-Kari "repe[at]edly presented false and mislead[ing] information" and made statemen[ts] she knew to be false.

Baltimore responded then that t[he] findings raised "very serious que[s]tions." He withdrew the Cell paper.

In his 14-page statement to N[IH] investigators, Baltimore ackno[wl]edged that "for too long" he had d[e]fended the now discredited paper.

Scientific Fraud and Misconduct in American Political Culture: Reflections on the Baltimore Case

by Daniel J. Kevles

The so-called Baltimore case first surfaced publicly in 1988, but it had been simmering in part of the biomedical research community for almost two years. The case originated in May 1986 with a young scientist named Margot O'Toole. She was a postdoctoral fellow in the laboratory of Thereza Imanishi-Kari, a cellular immunologist then on the faculty at MIT. O'Toole found serious fault with a paper that Imanishi-Kari and five coauthors, including David Baltimore, had published in the journal *Cell* in April of that year. O'Toole protested that the paper's central claim was not supported by the raw data. Investigations of her contentions at Tufts Medical School and at MIT concluded that the paper suffered only from minor, inconsequential errors and that O'Toole's quarrels with it amounted to a scientific dispute that could only be resolved by further research.

Why did scientific fraud become such a salient issue in American political culture during recent years?

What kept the case simmering was the zealous pursuit of it by two scientists at the National Institutes of Health, Walter Stewart and Ned Feder, and they reported it to the staff of Congressman John Dingell, a Michigan Democrat. Dingell in turn brought it to the public's attention in April 1988, when it was featured in hearings that he called "Fraud in NIH Grant Programs." In January 1989, a panel appointed by NIH to investigate the matter cleared Imanishi-Kari of fraud, but later that year the new Office of Scientific Integrity reopened the investigation. In 1991 the OSI found Imanishi-Kari tentatively guilty of fraud, a conclusion that was reiterated by its renamed successor, the Office of Research Integrity. In 1991, Baltimore was forced to resign the presidency of Rockefeller University for having defended Imanishi-Kari. As the result of an appeal, however, in June 1996 Imanishi-Kari was exonerated on all charges of fraud that had been leveled against her, and Baltimore was widely recognized for the courage he had shown—and for the costs he had borne—in resolutely defending her for a decade.

During the same period, several other cases of scientific fraud and misconduct achieved comparable salience, if not so prolonged a life. And in almost every one of them, the defendants were ultimately found not guilty. Each had been victimized by procedural flaws that denied them elemental rights of due process. Taken together, the scientific fraud cases of the late 1980s and early 1990s are puzzling, for two reasons. First, many observers say, and I think they are right, that scientific fraud is rare. Second, at the time scientific fraud as such compelled little if any attention in other scientifically vital nations. Why did scientific fraud become such a salient issue in American political culture during recent years?

One answer is that scientific fraud and misconduct were made and sustained as a public issue primarily by Congressman John Dingell. Dingell did not stop with the 1988 hearing. He held additional hearings on scientific fraud, focusing on the case of Imanishi-Kari, in 1989, 1990, and 1991. These hearings occurred with such regularity in successive springs that Imanishi-Kari's lawyer got mixed feelings about the blooming of the cherry blossoms in Washington; their opening meant it was time to testify, yet again.

Another answer is the media. The Baltimore Case, which both advanced attention to the issue of misconduct and was taken to be exemplary of it, provided a delicious target for the press. The whistle-blower, Margot O'Toole, was a compelling, highly articulate figure, a woman who, as one of Dingell's staffers put it, just "reeked with integrity." O'Toole's dogged insistence that she

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d Baltimore said yesterday
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University after an 18-
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had "created a climate of
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ten N. Wiesel, an eminent
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in New York City until a
an be found. Dr. Baltimore,
, will remain a professor at
ity.

ller Defense of Paper

more, who won a Nobel
his discovery of a crucial

would become such an extended personal travail for everyone involved. Trying to govern the university under these conditions has taken a personal toll on me and my family which I can no longer tolerate."

The spectacle of a Nobel laureate and president of one of the most prestigious American research universities fighting day by day over scientific fraud made the whole matter larger than life, with an effect greater than that of any case of science fraud in memory.

Dr. Baltimore's handling of the dispute led to controversy at Rockefeller University in 1989 when he was being considered for the job of president, and the dispute among faculty members

Continued on Page C8, Column 1

June 23, 1996: Thereza Imanishi-Kari and David Baltimore celebrate her exoneration on all charges two days earlier. A year later Imanishi-Kari was associate professor with tenure at Tufts University, and Baltimore had been named president of Caltech.

was right and Imanishi-Kari was wrong lay at the heart of the affair. In the media, she became a symbol of the heroic young scientist who takes a stand against the system and prevails over powerful figures like David Baltimore. In a column in *Time* magazine, the commentator Barbara Ehrenreich captured the overall take of most of the media. "Baltimore pooh-poohed O'Toole's evidence and stood by while she lost her job. Then, as the feds closed in, he launched a bold, misguided defense of the sanctity of science." She added, "What he lost sight of, in the smugness of success, is that truth is no respecter of hierarchy or fame. It can come out of the mouths of mere underlings, like the valiant O'Toole."

Yet neither John Dingell nor the media would have gotten anywhere with the fraud issue if it hadn't resonated with broader concerns in American life. The fact of the matter is that in the 1980s it struck a chord in recent developments in American political culture and the relationship of science—in particular, biomedical science—to those developments.

* * *

Remember the '80s? At the opening of the decade, some of the most powerful forces operating in American political culture were energized in the legacy of Watergate and the war in Vietnam. Both had generated a deep distrust of public and governmental authority and institutions. That distrust by no means diminished during the 1980s. Its continuation was fueled by the savings and loan scandals, evidence of waste and profiteering in the huge defense buildup, the sale of political favors in episodes such as Abscam, the indulgence in influence peddling, and the Iran/Contra controversy. Corruption in high places has been a constant motif of American life, but some eras have been worse than others. The '80s will likely be recorded in the history books as among the worst because, as in the era of Ulysses S. Grant, the corruptions of public life seemed to echo loudly the *Bonfire-of-the-Vanities* values—greed and careerism often coupled to lying and deception—that were so manifest in the private sphere, including the regions that touch on public interests.

This is not to indict the '80s but to place the emergence of the fraud and misconduct issue in its larger historical frame. That issue was, in fact, first broached publicly in the late 1970s in several post-Watergate-flavored articles in *Science* magazine by Nicholas Wade and William Broad, who collected their case studies into a book that was published in 1982 under the title *Betrayers of the Truth*. And the issue was not introduced into Congress by John Dingell. It was first explored on Capitol Hill in April 1981, in hearings titled "Fraud in Biomedical Research," by then-Representative Al Gore, the Tennessee Democrat.

Most of the Gore hearings and most of Broad and Wade's book were devoted to tales of individual cases of fraud and misconduct. Certain common themes ran through them: whistleblowers were not listened to and some suffered retaliation; academic institutions tended to be desultory in their response to whistle-blowing

As an ally of business and a ward of government, science, too, was vulnerable to suspicion, and the biomedical sciences were especially so. During the era of Vietnam and Watergate, they had prospered steadily, more than did the physical sciences, obtaining enormous support from public and private sources.

challenges and to be reluctant to jeopardize grants by acting on those challenges and, when they did act on them, tended to deliver minimal punishments, if any at all. Taken together, such outcomes appear to have prompted Gore, as well as Broad and Wade, to pose questions about the scientific enterprise in the U.S., questions that resonated with the distrust of authority and dissatisfaction with the emerging culture of the '80s.

As an ally of business and a ward of government, science, too, was vulnerable to suspicion, and the biomedical sciences were especially so. During the era of Vietnam and Watergate, they had prospered steadily, more than did the physical sciences, obtaining enormous support from public and private sources. But that period was also the time of the wars over recombinant DNA. Recombinant DNA opened the door to the transformation of organisms—plants, animals, and possibly even human beings—at the core of their hereditary essence. Many people, including many scientists, found these prospects unsettling. A number of biologists worried that recombinant micro-organisms might threaten life or health or whole ecosystems. Some questioned the reconfiguring of life itself as an act of hubris that would lead to unpredictable and dangerous consequences.

In the later 1970s, local and state governments and the United States Congress geared up to legislate tough, mandatory restrictions on such research. Norton Zinder, a biomedical scientist at Rockefeller University, denounced some of the recombinant regulatory bills in Congress for setting up "vast bureaucracies, cumbersome licensing, harsh penalties and tedious reporting procedures." By 1980, biomedical scientists had beaten back the threat of intrusive bureaucratic controls and obtained even a major relaxation of the strict guidelines that NIH itself had initially imposed on recombinant research. Nevertheless, the recombinant DNA wars left many of them skittish about anything that smacked of government intrusion into the practice of research.

Congressman Gore, a sturdy moral liberal, had gotten involved with policy making for recombinant DNA when he entered the House, in 1977. He held that the corruption of fraud raised serious questions about the ability of scientists to deal reliably with the "ethical judgments" that now confronted biological science "in great magnitude" —by which he meant genetic engineering, including the "spectre of cloning." At the 1981 hearings, Congressman Gore emphasized the importance of maintaining the American people's trust in science, especially given the billions of dollars they were providing it. Gore said he suspected that one reason for the persistence of scientific misconduct was "the apparent reluctance of people high in the science field to take these matters very seriously." The principal scientific witnesses were not reassuring on the point. They were Philip Handler, the head of the National Academy of Sciences, and Donald Fredrickson, the head of NIH. Handler acknowledged that the scientific community, both at large and in its local institutions, had "never adopted standardized procedures of any kind to deal with these isolated events [of misconduct]. We have no courts, no sets of courts, no understandings among ourselves as to how any one such incident shall be treated." Both Handler and Fredrickson implied that such courts or understandings were unnecessary and might even be dangerous. They insisted that fraud in science was rare and that false claims were exposed by the scrutiny that scientists gave each other's work. Fredrickson contended that the perpetration of fraud shook scientists "to our very core": when fraud did occur, the penalties were harsh, the equivalent of "excommunication" for the sinner.

The biomedical community's post-1970s supersensitivity to government controls was evident in the testimony. Fredrickson called it "frightening" that the larger, lay culture might intrude "roughly" upon scientific affairs while "failing to understand the requirements of the scientific method or the fact that its own correctives are in place." Although Fredrickson acknowledged that NIH had to be sensitive and responsive to changing public perceptions of science, he held that the agency "certainly cannot guarantee the behavior of scientists, or certify the quality of their work through a whole system of independent analyses or fraud squads, or even special statutes."

The Handler-Fredrickson defense prompted Congressman Robert Walker, a Republican member of the Gore subcommittee from Pennsylvania, to find "a certain amount of arrogance" in "a lot of the testimony." He doubted that a policy of "self-policing" was adequate for oversight of the rapidly burgeoning biomedical community. Alluding to Abscam, he warned: "We in politics would like to think that the people who stuck $50,000 into their pockets at some townhouse here in Washing-

Rep. John Dingell of Detroit, whose House committee claimed jurisdiction over NIH, used his Subcommittee on Oversight and Investigation to pursue the Imanishi-Kari case.

Margot O'Toole and Walter Stewart (middle) and Ned Feder, the two NIH staff scientists who brought O'Toole's complaint to Dingell's attention, testify at the Dingell Subcomittee Hearing on Fraud in NIH Grant Programs.

ton are an aberration in our profession, too. It doesn't mean . . . that we should not be conscious of the need to clean up problems of that kind. . . ." Walker predicted that if the press continued to report these aberrations and nothing was done, the "credibility [of science] will decline pretty quickly and the public will think of everybody in science as they think of everybody in politics—that is, as somehow a little crooked."

In the course of revising the health services act in 1985, Congress required institutions expecting to receive grants on behalf of their faculty to establish administrative processes for dealing with reports of fraud in biomedical research sponsored by the Public Health Service, the parent agency of NIH. The PHS promulgated the necessary specific regulations in June 1986, but in the meantime, American universities had been dilatory in establishing reliable local procedures. By the time John Dingell began to take an interest in the matter, in 1988, seven years after the Gore hearings, NIH itself had gotten around to assigning about 1.5 staff people to oversee the matter in all its grantee institutions.

By the late 1980s John Dingell was a hero to many Americans, especially those who opposed the decade's careering and corruption. To be sure, Dingell, whose district is in the area of greater Detroit, fought hard to protect the auto industry from foreign competition and to thwart the imposition of stricter emissions and safety controls on American car manufacturers. Nevertheless, Dingell is an unashamed liberal, particularly on economic issues; in 1989 he tallied a 75 percent rating from Americans for Democratic Action. During the 1980s, by dint of hard work, high intelligence, and political flint, he built the Energy and Commerce Committee, which he chaired, into one of the most powerful committees in the House, the gateway to some 40 percent of the bills that went through the chamber. It claimed "jurisdiction," it was said, "over every-

thing that moves, burns, or is sold," including securities markets, energy, railroads, telecommunications, defense contractors, consumer protection—and the NIH.

Dingell, who is the son of a New Deal Democratic congressman, is shrewd and knowledgeable in the ways of Capitol Hill, and during the 1980s he used his committee chairmanship to counter some of the chief injuries that careerism and greed in and out of government were doing to the public interest. He aggressively exploited the chairmanship of his committee's Subcommittee on Oversight and Investigation to raise questions about former Reagan aide Michael Deaver's influence peddling; to skewer EPA administrator Anne Burford for selling environmental policy to the highest bidder; to expose the Pentagon for apparent extravagances like the $640 toilet seat; and to reveal that General Dynamics deemed a dog's kennel fees a defense contracting expense worthy of federal reimbursement. Civil libertarians, especially those of a conservative bent, deplored Dingell's methods, including trial by press leak, intimidation of witnesses, and the pit-bull tactics of some of his huge staff. But many people tended to overlook the prosecutorial and persecutorial means he used, partly because he was so powerful but also, I think, because in the circles of those out of sorts with the political culture of the 1980s, he pursued the right enemies.

But then the Dingell subcommittee took on scientific fraud. Whatever NIH and its grantee universities might have done since the Gore hearings to establish reasonable procedures for dealing with fraud and misconduct, Dingell and his staff believed, not without reason, that it was flabby and inadequate. More cases of fraud had surfaced in the press, along with accompanying evidence of dilatory institutional responses. Two of Dingell's key staff members—Peter Stockton and Bruce Chafin—tell of people coming to them with stories about scientific corruption going unpunished and research institutions covering up to protect themselves. Chafin holds that the system failed to deter wrongdoers or to take whistle-blowers seriously. In 1988, Dingell declared that he was "shocked to find out that the National Institutes of Health relies completely upon the universities to investigate themselves. Apparently here we have the possibility of the fox actively investigating the chicken coop."

"We do not wear lace on our drawers as we conduct our investigations," Dingell told a reporter. "I'm not paid to be a nice guy. I'm paid to look after the public interest."

Congressman Dennis Eckart, of Ohio, another member of Dingell's subcommittee, explicitly identified the seeming trends in science with the '80s culture of corruption, noting that defense contractors tell us they have systems to catch fraud, but the rest of us know those systems don't work. "What is at issue here, much in the same way [as] within the defense industry, over at NASA, over at the accounting profession, over at savings and loans, are the adequacy of safeguards which will give you and me and the public confidence that waste, fraud, abuse, or misconduct do not occur with taxpayers' dollars." Dingell and his subcommittee colleagues said they initiated the hearings on fraud in NIH grant programs so as to expose the ongoing flaws in the system and get the academic community to heal itself.

Dingell had a special way of doing such public business, however. He was not known as a legislative initiator, and he did not hold hearings mainly to gather information for the purpose of drawing up bills. It is a longstanding tradition in Congress that committees use the instrument of hearings to get the attention of the executive branch and to pressure it to change policy without legislation. Dingell was a man of that tradition. He used the hearing room to spotlight an issue by probing into someone, or some case, or some practice exemplifying the matter. When he and his staff began gearing up to take on the issue of scientific misconduct, they followed their normal operating procedure, which was to seek out an ongoing high-profile case that would show the system still in need of repair. They found it in the challenge that Margot O'Toole had raised against MIT, Tufts, Thereza Imanishi-Kari, and David Baltimore.

Dingell's subcommittee pursued the matter relentlessly—by subpoena, by leak, and by bullying. "We do not wear lace on our drawers as we conduct our investigations," Dingell told a reporter. "I'm not paid to be a nice guy. I'm paid to look after the public interest. Our purpose is simply to compel universities and scientists to clean up their act and to see to it that public money is properly spent." Ostensibly to that end, the Dingell subcommittee pursued the Imanishi-Kari case implacably.

The congressional criticism that began in April 1988 had, in the meantime, prompted NIH to initiate reforms of the way it handled allegations of scientific misconduct. In the fall of 1988, the agency announced that it was contemplating the creation of an office of scientific integrity and invited comment on the scope and definition of scientific misbehavior. Contemplation turned into action in the face of moves on Capitol Hill that winter, including a draft bill from John Dingell's subcommittee, to legislate the creation of such an office with powers that would include random audits of the notebooks of NIH grantees. James Wyngaarden, then head of NIH, worried that

O'Toole said several times that she deserved no special credit for her courageous whistle-blowing, that she did only what an honest scientist should do. But at other times she engaged in a kind of self-fashioning that resonated with the sort of heroes that many people hungered for in the 1980s.

congressional action would give control of misconduct cases to an inspector general, a lawyer who knew nothing about science. "We needed a preemptive strike," he said later. On March 8, 1989, NIH created an Office of Scientific Integrity on its own. OSI would monitor inquiries at its grantee institutions, conduct its own investigations when necessary, and be run by scientists.

Wyngaarden picked Brian Kimes to head the office temporarily, until a regular director could be found. Kimes is a biochemist by training who loves science and the NIH, where he had been a research administrator in the National Cancer Institute for almost 15 years. He took the job of setting up OSI reluctantly, telling Wyngaarden that he would stay only until November 1, and that "it was not my ambition to send scientists up the river." What *could* send scientists up the river was spelled out in the definition of scientific misconduct that the Public Health Service issued in midsummer. It included "fabrication, falsification, plagiarism." It also included "other practices that seriously deviate from those that are commonly accepted within the scientific community"—a feature that a number of scientists had objected to, no doubt fearing that it raised "serious risks of undue pressure for scientific conformity," as an official in the Office of Management and Budget had put it when the proposition first came up. Many scientists were likely relieved, however, by the Public Health Service's stipulation that misconduct "does not include honest error or honest differences in interpretations or judgments of data."

The small OSI staff was burdened with a backlog of 80 to 100 cases. "We were over our heads . . . ," Kimes says. "We had to figure out where the political issues were and where the scientific issues were." But they knew that one case dominated all the rest. Congressman Dingell had told Wyngaarden that he regarded the matter of Imanishi-Kari as a "crucial test" of the ability of NIH "to deal with cases of questioned science." NIH officials were concerned that the agency not appear to be remiss. "We were taking a lot of hits from Dingell on this with the public," Kimes notes. "Wyngaarden was extremely worried about the politics of it because those are things that could impact all of NIH. It was a very uncomfortable situation."

OSI had not issued a set of rules and procedures. Kimes says that the staff was so busy that they had to make them up as they went along. Kimes himself wanted OSI investigations to be open, with everyone having access to all the data and testimony. In August, he assured Baltimore and Imanishi-Kari that once the allegations were completely formulated, they would be made available to "all subjects of the investigation," together with all related evidence. He told Imanishi Kari's lawyer, Bruce Singal, that Imanishi-Kari would have "every opportunity to see and comment" on all the relevant information and that she could be confident that OSI's inquiry would be "conducted independently" of the subcommittee's. Dingell, however, was reportedly very upset by the scenario. To Kimes, it was evident that Congressman Dingell "did not want the process to be open; he wanted us to investigate."

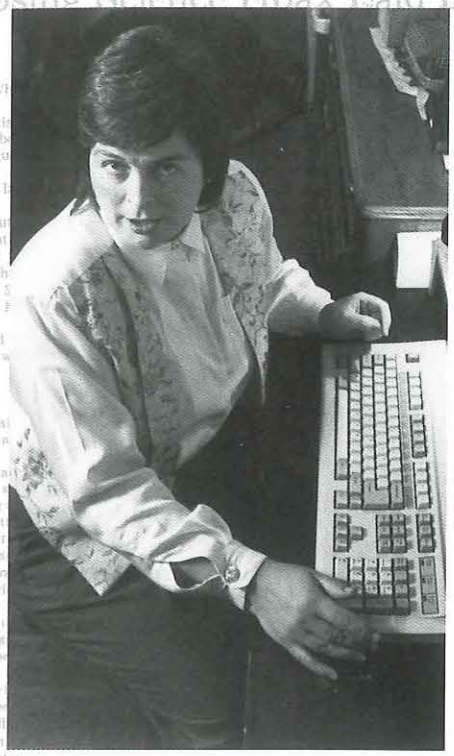
Toward the end of October, OSI revised its plans for handling the allegations against Imanishi-Kari. They would be presented to OSI and its scientific panel, not to the coauthors. Both would evaluate the evidence she provided and then conduct an investigation, talking with the other principals in the dispute. The new dispensation meant that Imanishi-Kari would be given only indirect access to the allegations and no access at all to the material evidence or testimony against her.

Imanishi-Kari was, to all intents and purposes, prevented from mounting a genuine defense. The OSI combined the duties of investigator, prosecutor, judge, and jury and pursued them in the manner of the Star Chamber. During the investiga-

tion, it did not provide the accused a list of specific charges. The charges "are revealed to you as time goes on," a lawyer familiar with the agency's ways remarked. "It is as if there is an indictment you can't see." Both during and after the investigation, Imanishi-Kari was denied the right to see the evidence, the right to learn what witnesses said, and the right to cross-examine them. The burden of proof was on the accused rather than on the accuser. The NIH's Office of Scientific Integrity was as Orwellian in procedure as it was in name.

In early 1991, OSI put its tentative conclusion that Imanishi-Kari was guilty of fraud into a draft report. The report, though strictly confidential, was leaked to many newspapers, provoking a flood of media response. Analysis of it, along with the pre-leak representation and commentary on the case, reveals just how integral belief in scientific fraud and misconduct had become to the post-Watergate political culture of the 1980s. The trope of Watergate itself ran through what was said by influential representatives of the national press, such as *The New York Times*, which savaged Baltimore for "stonewalling" and, likening the long affair to a huge cover-up, declared that "the Baltimore case started with apparent fraud by a single scientist and soon led to a widespread denial of wrongdoing by almost everyone in a position to right the wrong." The report also happened to be leaked at the same time that the university overhead scandals were breaking, particularly the alleged ripping off of the government by Stanford University. A number of observers melded the Baltimore case together with misuse of public funds. Like a reporter for *Chemical and Engineering News*, they held that "a major problem is that universities today are not only knowledge centers but also financial conglomerates," with all the incentives to corner-cutting behavior that mark such enterprises.

But no single person did more to tie the case to the features of post-Watergate political culture than Margot O'Toole—not by design but simply by refracting it through the language and suspicions of the culture of Watergate, which came naturally to her, and by having been portrayed as a victim of powerful scientific and institutional interests by Congressman Dingell, his staff, and the press. "Here was someone chewed up by the system," Bruce Chafin says. "Here was a perfect object lesson." O'Toole herself indicted Tufts, MIT, and the *Cell* paper's coauthors for responding to her complaints with "the knee-jerk reaction of cover-up" and with displaying "contempt for the labor of people trying to repeat the work" of the experiment. In an article in the *Chronicle of Higher Education*, she claimed that the Tufts and MIT investigators "told me the corrections I proposed [to the paper] were 'out of the question' because they could adversely affect the authors' careers and financial support."



Hero In Exposing Science Hoax Paid Dearly

By PHILIP J. HILTS
Special To the New York Times

WASHINGTON, March 31 — Wh
Dr. Margot O'Toole, a junior
searcher in molecular biology, rais
uncomfortable questions in 1986 ab
the validity of a senior colleagu
work, she felt alone.

Dr. David A. Baltimore, a Nobel l
reate who was a co-author of a
search paper that used the disput
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band's job was in jeopardy as well. S
took work answering phones at F
brother's moving company.

"It was very difficult," she said
day. "There were times when I w
really frantic."

'Maintained Her Commitment'

But Wednesday, in language rist
above the scientific and bureaucra
jargon common in Government
ports, the National Institutes of Heal
called her a hero. "Dr. O'Toole s
fered substantially for the simple ac
raising questions about a scient
paper," the agency said in a report
the case. "Notwithstanding the los
and costs she incurred, Dr. O'To
maintained her commitment to sci
tific integrity."

In a draft report, the health n
tutes' Office of Scientific Integrity a
in effect that Dr. O'Toole had be
right all along: crucial data in
paper based on work by her super
Dr. Thereza Imanishi-Kari, had be
faked. The scientific paper describ
findings suggesting that transplan
genes could stimulate a recipient's im
mune system. The finding has not been
confirmed by other researchers.

"One of the most surprising things to
me is the way so many members of the
scientific community and the scientific

Continued on Page B6, Column 1

Margot O'Toole

O'Toole said several times that she deserved no special credit for her courageous whistle-blowing, that she did only what an honest scientist should do. But at other times she engaged in a kind of self-fashioning that resonated with the sort of heroes that many people hungered for in the 1980s. In an interview with a sympathetic reporter for *Mirabella*, a prominent women's magazine of fashion and chic, she contrasted herself to David Weaver, who was another of the coauthors on the *Cell* paper when he was a postdoc with Baltimore: "We both could have made the choice to keep quiet for the sake of our own careers. He did and I didn't. . . . The science was more important to me than the career and . . . the career was more important to him than the science."

Parts of the media, including some with enormous influence in scientific affairs, were willing

and enthusiastic collaborators in the representation of the case as exemplary of a scientific Watergate. Some could almost be termed collaborators with Congressman Dingell. Dingell's subcommittee shrewdly leaked documents, many of them confidential, to favored reporters, who published the committee's version of events, and published them uncritically. Donald Kennedy, the former president of Stanford who was uncritically brutalized by the press during the overhead controversies at his university, recently observed, "Press accounts frequently refer to unnamed subcommittee members and quote from documents obtained before they were made public. When such material comes regularly from one side of a controversy, it amounts to news management—and no responsible reporter should be captured in that way."

The capture is partly explained by the overwhelming enthusiasm of many journalists since Watergate for investigative reporting. Since the 1960s, that trend has come to mark some science reporting, and it has created a genre of science reporting that displays the strengths and weaknesses, including the often unfairly injurious weaknesses, that are characteristic of investigative reporting in general. I don't mean to disparage investigative reporting. The public interest has been advanced by it. But it would seem that investigative reporting in science, like investigative reporting in general, demands getting the facts right. Equally important in this special genre, it demands getting the science right, too.

"When such material comes regularly from one side of a controversy, it amounts to news management—and no responsible reporter should be captured in that way."

Even though the leaked draft report was just that—a draft—Baltimore and Imanishi-Kari were tried and convicted in the press. In this affair—as likely in other high-profile cases of scientific misconduct—influential reporters often got both the nonscientific and the scientific facts wrong. I will cite just one example, Philip Hilts of *The New York Times*, but it is a salient one, because the *Times* is arguably the most influential paper in the country, and its science reporting is excellent on the whole. Donald Kennedy has indicted Hilts for bias in the Baltimore case, pointing to an attack article that Hilts published in *The New Republic* in 1992 and alluding to his important coverage of the case in the newspaper. Hilts has responded that no one has pointed to any specific bias in his coverage of the case. I don't know anything about Philip Hilts's motives, but I do know that his article in *The New Republic* was riddled with errors about how the case was investigated at MIT, all of them mistakes that reinforced one of the main points of his piece, which was that the investigation was designed to save the reputation of David Baltimore at the expense of Margot O'Toole.

Baltimore could take care of himself. Imanishi-Kari was far less able to defend herself against a fundamental point that Hilts reported about the *Cell* paper in a profile of her in the *Times* on June 4, 1991. To quote from his article: "The central claim of the paper depends on how many mice showed the unexpected antibody properties. But the statement in the paper that said this work was done, she has admitted, was false. 'We did not do it,' she said." Hilts's story continued: "But in explanation she said a similar characterization was done on other mouse samples."

The fact of the matter is that an enormous amount of work was done by Imanishi-Kari that demonstrated that the mice had abnormal antibody properties. What was *not* done was an additional test—the isotyping of the antibodies generated by one set of mouse cells. That data would have added weight to the paper's central claim, but it was not decisive for it, a crucial point of the science that Hilts missed entirely. He also did not report that the statement about the isotyping had gotten into the paper inadvertently—as the result of a miscommunication between Baltimore and Imanishi-Kari in the drafting of it to the effect that the test had been done on that set of cells when, in fact, it had been done, but on another set. The bottom line is that, by not paying attention to the science, Hilts's story misrepresented Imanishi-Kari as having declared that the test was crucial to the experiment's central claim, which she had not and which it was not. His story also gave the impression that in the published paper she had asserted knowingly and deliberately that she had performed the test. Imanishi-Kari protested Hilts's misrepresentation in a letter to the science editor of the *Times*, but she got neither an apology nor a correction.

* * *

Recall that it was the legacy of the war over recombinant DNA that helped prompt the emergence of fraud as an issue in American political culture. Before that, people had tended to think that science was perhaps the least corruptible institution in American life, certainly less corruptible than certain branches of the evangelical churches. One of the things that prompted Al Gore to call his hearings in 1981 was the evidence, mounting as he saw it, that science, in fact, might be corruptible, and that the potential for fraud raised serious questions about the ability of scientists to deal reliably with the fraught ethical questions that the advance of molecular biology was raising. Philip Handler claimed that there was "absolutely no relationship" between fraud, even as a minor problem, and what Gore called "the other questions involving ethical judgments which now

Scientific fraud is a kind of late-20th-century morals charge, in a branch of morality that large parts of the public perceive as central to human health and human fate.

confront science in great magnitude." But Gore asked: "If the scientists who have been found in this recently reported outbreak of seemingly unconnected events to be making these kinds of judgments with respect to arguably small matters, shouldn't we be concerned about the decisions they are making elsewhere?" Handler said, no we should not, whereupon Gore retorted, "Well, I disagree strongly."

My guess is that, at least tacitly, many people continue to disagree strongly. Scientists have long resembled a secular priesthood in American culture—originally as mediators between man and God's universe, but now as explorers, interpreters, and commanders of nature's powers. It should not be surprising that many people display high trust and confidence in science, which they do. But it should not be surprising either that many develop comparably high suspicion of it if evidence—or a claim of evidence—emerges that its celebrated concern for truth is violated, particularly among biomedical scientists.

To be sure, much of the concern with scientific fraud stems from the fact that biomedical science is lavishly supported with public money. Nevertheless, the exposure of fraud in the biomedical sciences is tantamount to revelations of pederasty among priests perhaps more than of embezzling among bankers. Scientific fraud is a kind of late-20th-century morals charge, in a branch of morality that large parts of the public perceive as central to human health and human fate. The fact that it's freighted with all that such charges connote—violation of a moral code and exploitation of innocents—helps explain why scientific fraud and misconduct has become a powerful issue in the United States, a country that more than any other expects its public shamans to behave consistently with their declared moral commitments. The public perception of both the behavior and powers of biomedical scientists may be exaggerated, but it is a fact of life for the fortunes of science in the American political culture of the late 20th century. As such, it is something that the scientific community cannot afford to ignore, no matter how rarely fraud may occur. □

Dan Kevles first wrote about this case for The New Yorker *in May 1996, arguing that Thereza Imanishi-Kari was not guilty and that David Baltimore had been right to defend her. The following month she was exonerated on all counts. He then expanded that article into a book,* The Baltimore Case: A Trial of Politics, Science, and Character, *which was published by W. W. Norton early this fall. The book made the bestseller list of the* Los Angeles Times, *which, in a review, praised the book as a "brilliant, unsparing and meticulously researched account of a controversy that helped reshape how scientific misconduct is handled in the United States."* The New York Times *called it "a splendid study of a major contemporary scientific scandal."*

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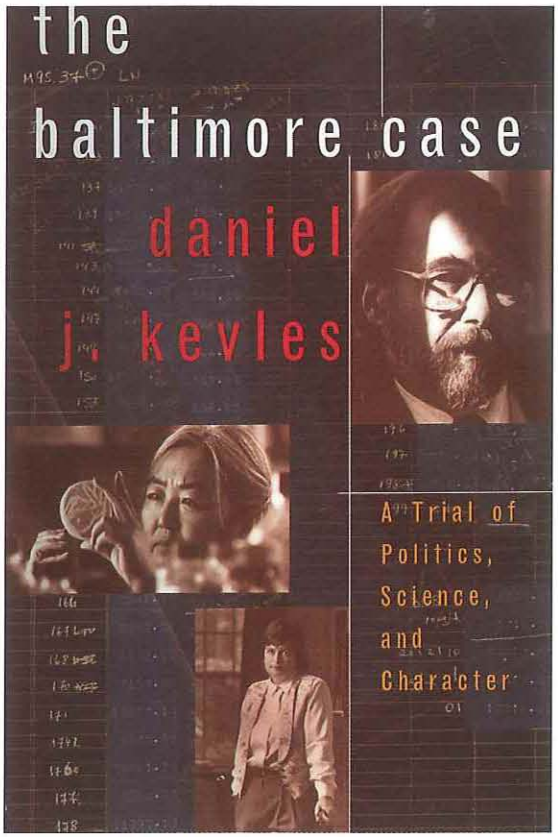

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