NIH Sees Plagiarism in Vision Paper

Panel says researcher took data from paper he peer-reviewed and used it for his own work; the accused says it is just not true, but NIH recommends debarment proceedings

THE RETINA OF THE EYE has been compared to a piece of film. Light causes film to bleach as an image is recorded. But unlike film, the pigment in the retina regenerates as we record image after image.

In a paper in the 26 June 1987 issue of *Science*, C. David Bridges of Baylor College of Medicine explained how that regeneration takes place. It was something Bridges and his colleagues in vision research worldwide had been trying to explain for 30 years. Though the data were preliminary, Bridges' discovery was one of major importance.

NIH Director Wyngaarden has terminated Bridges' federal funding.

But there is alleged to have been one problem with his publication. The discovery that the retina regenerates when one form of vitamin A (all-*trans*) is "isomerized" or converted back to its complimentary form (11-*cis*) was made not by Bridges but by Robert R. Rando and his research team at Harvard and published 2 months earlier in the *Proceedings of the National Academy of Sciences* (*PNAS*). According to a report just completed by the National Institutes of Health and released to News and Comment under Freedom of Information, Bridges stole Rando's idea, then claimed it as his own.

An investigating panel of vision scientists and NIH lawyers has found that Bridges plagiarized Rando's work, which he saw prior to publication as a referee for the *PNAS*. Katherine Bick, deputy director for extramural research at NIH, told *Science* that she was "horrified" by Bridges' apparent violation of trust that is supposed to lie at the heart of the peer-review system. In the past, there have been rumors that the system is not inviolate. People say that so-and-so lifted an idea from a competitor whose paper he had for review. But, says Bick, this is the first actual case of theft through peer review that NIH has handled.

NIH director James B. Wyngaarden has recommended that the Department of Health and Human Services initiate formal debarment proceedings to declare Bridges ineligible for future federal funding. In addition, he has banned him from all NIH review bodies for 10 years and has terminated his current NIH funding.

Bridges will appeal the NIH finding at his debarment hearing. He calls the NIH findings "outrageous." In a telephone interview with *Science*, he declared "I am totally innocent of this accusation." In a lengthy point-by-point challenge to the NIH's findings, Bridges claims that the research that led to his *Science* paper began a couple of months *before* he ever received Rando's manuscript. Furthermore, he filed a rebuttal, challenging some of the NIH findings of fact.

But NIH was not swayed. Indeed, the NIH investigating panelists responded to each of Bridges' points in what seems a more thorough report than many that have emerged from the NIH fraud office in recent times. The panel's conclusion: Bridges lacks the evidence to support his contentions.

One issue that is likely to come up during Bridges' appeal is the matter of primary data for the *Science* experiment. The NIH report states that Bridges told a review committee at Baylor that he had destroyed them just prior to his move to Purdue University, where he now holds a position in biochemistry. The absence of primary data "shocked" the NIH panel, which simply found it hard to believe that Bridges would destroy the data for what it regards as "arguably the most important" discovery of his career.

But Bridges told *Science* that he never said any such thing to the Baylor committee and, further, that he considers his computerized data (not missing lab books) to be "primary." "The data were entered directly into the computer. The idea that I destroyed my primary data is laughable."

Like many cases of alleged misconduct in science, this case is messy, full of conflicting arguments about details, and long running. It began in July 1986 when Harvard professor John Dowling, a member of the National Academy of Sciences, received a vision research manuscript from his Harvard colleague Robert Rando. Rando hoped Dowling, in his capacity as an NAS member, would sponsor its publication in *PNAS*.

Bridges, like Dowling, had been in the vision business some 30 years. "David was the obvious person for me to send Rando's paper to," Dowling told *Science*. The manuscript reached Bridges' lab in July, but it was not until September that he returned it to Dowling with a handwritten note saying he could not be an expert reviewer because he was working on a nearly identical experiment. Dowling then found another reviewer who gave Rando his blessing, and with that, Dowling "communicated" it to *PNAS*, which published it in April 1987.

"The idea that I destroyed my primary data is laughable."

—C. David Bridges

Prior to that April publication, vision researchers were getting ready for the annual meeting of the Association for Research in Vision and Ophthalmology (ARVO). Rando submitted an abstract based on his *PNAS* paper. Bridges submitted an abstract that appeared strikingly similar, based on work submitted first to *Nature*, which turned it down, and later to *Science*, where a revised version was subsequently published.

When Rando learned about the two abstracts, he called Dowling. Had Dowling sent Bridges his paper to referee? Dowling, in what the NIH panel calls a "departure from accepted procedure," honestly said that he had.

Shortly after that, the NIH panel reports, Bridges sent Dowling a draft of his own manuscript, by that time accepted by *Science*. "What bothered me was the way he wrote it," Dowling says. "There was no credit to Rando." Dowling called Bridges and advised him to modify his paper. He also brokered a meeting between Bridges, Rando, and one of Rando's coauthors at the ARVO conference.

Bridges and Rando reportedly reached an agreement whereby Bridges was to change his *Science* paper to clearly acknowledge Rando's priority. At that point, no one, including Rando, was accusing Bridges of

plagiarism—just of claiming priority. Indeed, as far as Rando knew, Bridges had begun the experiments he described in his paper before reading Rando's paper.

Still, Rando claims not to have been entirely comfortable with the brokered agreement. In an interview with *Science*, Rando said, "I felt uneasy that no one at *Science* would know" about our agreement and how important it was. Thus, on 12 May 1987, Rando wrote to *Science* editor Daniel E. Koshland, Jr., enclosing a copy of his *PNAS* paper. Rando says he and Bridges "reached a verbal agreement that certain changes should be made in the *Science* article to indicate that our paper had been sent to Prof. Bridges for review in July 1986. The changes we agreed on are enclosed," he said in a letter News and Comment obtained under Freedom of Information.

When the Bridges manuscript appeared in the 26 June issue, the agreed-upon changes were not there. The NIH report reflects some confusion over what happened.

Now, Bridges has acknowledged to News and Comment that the changes were his. Rando "really infuriated me by writing Koshland," Bridges says, and he blunted the language meant to give Rando priority.

In that, he was successful. " . . . recent data by the same authors that appeared since the present work was submitted demonstrate a retinol-specific isomerase in pigment epithelium (*5*)," Bridges' paper says, in what is not exactly a paragon of clarity. The footnote does say that "a version of the latter manuscript [referring to Rando] was shown to the present authors by J. E. Dowling."

It was right after Bridges' publication in *Science* that the priority dispute escalated.

By all accounts, there was tension in Bridges' laboratory at Baylor at the time he was making plans to leave for Purdue. One of his co-workers—indeed, Bridges' key technician—who was not to accompany Bridges to Indiana, told Rando that there was evidence Bridges had used Rando's data in designing the experiments that formed the basis of the subsequent *Science* paper—the very experiments that Bridges now claims were under way before he read Rando's paper. Baylor officials had also been notified of this allegation and the president appointed a three-person committee to investigate.

That committee, which met for the first time in September 1987, focused on the allegation that Bridges tried to claim priority from Rando. Purdue was aware of the investigation, but did not consider it sufficient grounds for withdrawing its offer to Bridges.

Ultimately, the Baylor committee, which had notified NIH of its investigation from the start, concluded that Bridges was guilty of claiming priority that rightly belonged to Rando. But Baylor only hinted that the Rando manuscript might have been plagiarized. Baylor sent its report to NIH. Bridges sent a sharp rebuttal.

NIH then decided to take a look for itself, appointing a panel headed by Ralph A. Bradshaw, a vision researcher at the University of California at Irvine. It is that panel's report that forms the grounds for the NIH recommendation that Bridges be debarred from receiving federal funds.

The NIH panel investigated a number of angles to the dispute; one of the most important, perhaps, the timing of Bridges' alleged experiments.

Bridges contends that he began the crucial vitamin A isomerase experiments, using frogs, on 14 May 1986. His lab technician and coauthor, Richard A. Alvarez, says the experiments were not begun until the 6th of August—after Bridges had seen the Rando manuscript. (Alvarez, it should be noted, did not see the Rando manuscript and has not been implicated in any of Bridges' alleged improprieties.)

It may be impossible to ever resolve the battle of the dates beyond a shadow of a doubt, but the NIH panel believes that the bulk of the evidence supports Alvarez and the 6 August date. According to the panel, there is no good evidence that Bridges was doing studies that would be logical precursors to the isomerase experiments. They

Case Highlights Sensitive Issues

Rando *v.* Bridges comes at a time when Congress and the press have considerable interest in the workings of science. At congressional hearings during recent years, scientists have repeatedly assured the public that fraud and misconduct in research are rare; an aberration. The enterprise is vast; the number of cases of misconduct is minuscule by comparison. And yet, significant cases seem to keep cropping up.

Is the Bridges case of alleged theft of intellectual property really unusual, or if NIH's indictment proves out, is it only the fact that Bridges got caught that stands out? What if Bridges is innocent, as he claims to be? Could his now-damaged reputation be restored? How did this all come about anyway? The case highlights several politically sensitive issues in addition to the sanctity of peer review. Among them are these:

■ The role of scientific journals in weeding out suspect papers. *Science* knew that the Bridges paper was in dispute (though not accused of plagiarism) before publishing it. Should *Science* have held off? Editor Daniel E. Koshland, Jr., says no. "We hear complaints about priority all the time," Koshland says. "Shortly before we published [the paper], we became aware of the controversy about priority but we did not have sufficient evidence to take action and decided it would be unfair to delay publication." Rando did not allege plagiarism.

■ The retention and ownership of primary data. The NIH panel was highly critical of the Baylor review committee for not pursuing the issue of the missing primary data further. While the Baylor committee "drew negative inferences" from its belief that Bridges destroyed the original laboratory notes, the NIH panel declared that Baylor was wrong to conclude that the principal investigator owns his primary data and has authority to destroy them. Baylor, the panel said, should review its requirements for retaining data.

■ The quality of institutional review of allegations of misconduct. The NIH panel also criticized Baylor's review, saying it "lacked depth and rigor," in part for placing so much emphasis on the question of priority while skimming over the matter of possible plagiarism. Baylor insists that its investigation was appropriate, given the allegations at the time.

■ The quality of peer review. The NIH panel is harsh in its criticism of the merits of the Bridges paper, going out of its way to take issue with the anonymous reviewers who recommended its publication in *Science*. After its own review of the paper, the panel concluded it contains "internal inconsistencies, incomplete data, and misrepresentation" quite apart from any issue of plagiarism. However, the editors of *Science* have told News and Comment that they stand by the quality of the initial scientific review.

■ The pressures inherent in science that might lead people to cheat. As in many walks of life, there is a great premium in science on being first. One observer noted wryly that had Bridges been generous in granting Rando priority in his *Science* paper, the allegation that he had stolen Rando's actual experimental idea might never have been made. ■ **B.J.C.**

seem to have come out of the blue. Further, with the exception of the Bridges *v.* Alvarez accounts, there are no eye witnesses to any frog experiments in May and June. Nor, NIH claims, is there evidence that Bridges intended to follow up the discovery.

But perhaps most important to the NIH panel's thinking is evidence that Bridges lacked the necessary laboratory chemicals to do in May what he said he did. The frog experiments claimed by Bridges require the use of tritiated retinol or vitamin A. Records show that Bridges received a shipment in January 1986 which, the NIH experts believe, would not have been sufficient for the early experiments because some of the tritiated material is used up during purification procedures that are needed as the substance ages. The next shipments, according to records obtained from the manufacturer, arrived in August. Furthermore, the August shipment of tritiated retinol was less radioactive than the earlier one. The level of radioactivity reported for the frog experiments in the *Science* paper is consistent with the August shipment.

This is another point with which Bridges takes issue. He refers to a statement in the second of his two rebuttals, claiming successful research has been done with tritiated vitamin A that was 2 years old.

But NIH's panel was not persuaded.

And so, the NIH conclusion was reached—Bridges not only had the opportunity to plagiarize Rando's experimental protocol but that opportunity was "realized."

For now, Bridges' career is very much up in the air. Purdue University dean Kenneth Kliewer told *Science* that he "knew that there had been trouble at Baylor before David came here, but we thought it was just a priority dispute. Since Rando published first, it seemed like an exercise in futility. Since he's been at Purdue, David has been a first-class person, good with graduate students, good on faculty committees."

Kliewer has empaneled a special committee of senior Purdue faculty to advise him. Meanwhile, he is waiting.

And Bridges is preparing yet another response to NIH. He and his lawyer will argue that a lack of due process precluded them from knowing all of the allegations as the NIH panel was moving along and responding completely to all of the evidence. "If I had gone out into the street and murdered someone in full view of 50 people, I would be accorded more safeguards than I got from NIH," Bridges contends.

NIH stands pat and is, in any case, now out of the loop. The next chapter in this sad tale will come when Bridges' appeal is heard during the debarment proceedings.

■ **BARBARA J. CULLITON**

Dahlem Conferences Face Ax

U.S. researchers, accustomed to a laissez-faire style of scientific meetings, often balk at first at the rigid format of the Dahlem conferences—week-long, interdisciplinary workshops held four times a year in West Berlin. But within a few days of each workshop's opening, even the most resistant participant is usually seduced by the unique character of these workshops. "As a result, there is an ever-swelling band worldwide of Dahlem loyalists, veterans of an extraordinarily successful approach to scientific communication and discussion," says Berkeley molecular biologist Gunther Stent.

But just when everyone was loving them, political wranglings in West Berlin are threatening to shut down the Dahlem Konferenzen. "The Stifterverband, a group of industrial donors that has supported Dahlem since the beginning, has given notice of termination to the entire staff, as of 31 December 1989," says conference director Silke Bernhard. "It might be possible eventually to arrange for another organization to sponsor Dahlem, but that will take time and continuity will be lost. My staff are already leaving or looking for new positions."

Conceived 15 years ago by Bernhard, the conferences have 48 attendees, and some are required to produce a discussion paper beforehand. The participants are split into four working groups, each of which produces a report on one aspect of the overall topic. This strict formula, paradoxically, produces an extremely free exchange and generation of ideas. The conferences have been supported by Stifterverband well beyond its usual pattern of 3-year support. "We did it because they were so good," says Stifterverband's chief executive, Hans-Hennig Pistor. But, he added, "we can't finance a project forever." Bernhard accepts this reasoning, but complains that in withdrawing its support, "Stifterverband was more concerned with politics than the future of the conferences."

The Stifterverband decided late in 1986 to end its support for Dahlem Konferenzen and the following year proposed that it be absorbed by the newly established Berlin Academy of Sciences. For 2 years Pistor and his colleagues at Stifterverband tried to get agreement of the transfer from Dahlem's advisory committee and its director, Bernhard. "I was concerned that Dahlem Konferenzen would lose its autonomy if it became part of the academy," says Bernhard.

In spite of threats that she would be fired if she did not sign a "letter of understanding" that was necessary for the transfer to be effected, Bernhard withheld her consent and explained her position at a meeting of Dahlem's advisory board on 6 December last year.

This recalcitrance prompted Pistor to comment later that "only the negative attitude of Dr. Bernhard stood in the way of a solution of the question of a takeover [by the academy]." Sir Gordon Wolstenholme of the Royal College of Physicians, London, and a member of Dahlem Konferenzen's advisory board, objected: "This was not a *negative* attitude but a correctly *positive* one on behalf of the integrity of the program of Dahlem Konferenzen."

The question of the takeover by the Berlin Academy became academic this spring: the newly elected West Berlin Senate, now dominated by a coalition of the Social Democratic and "Green" parties, disbanded the academy, declaring that it had been established undemocratically by the now ousted Christian Democrats. The new Senate has offered financial support for Dahlem, but Pistor told the advisory committee that he doubts it will "fulfill the pledges made concerning the financing of Dahlem Konferenzen." The Stifterverband is "compelled to give up the Dahlem Konferenzen as of 31 December 1989," added Pistor.

"The Stifterverband is deliberately not seeking a new sponsor as an act of political revenge," charges Bernhard. "First they tried to use me as a scapegoat, and now they are using the new Senate. The financial support is there, if they really wanted Dahlem to go on."

The new Senate may in fact come to Dahlem's rescue, as it recently asked the Free University of Berlin to explore the possibility of taking part in sponsorship. Wolf Singer, president of the Max Planck Institute for Brain Research, Frankfurt, told *Science* that the institutes might also become involved: "The law requires seven sponsoring organizations," says Singer. "I think it might be possible to achieve this, but it will take time. I hope it won't be too late." ■ **ROGER LEWIN**

Information for this article was provided by Don Kirk, a free-lance writer based in Bonn.