COMMENTS

The Evolution of the "Scientific Misconduct" Issue: An Historical Overview

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Scientific misconduct became a controversial public policy issue in the United States in the 1970s and 1980s when several cases of faked and fabricated research were discovered in prestigious academic institutions and resulted in coverage in the general as well as scientific press. This publicity drew Congressional and federal agency attention to what, until then, had been treated primarily as a matter of institutional or laboratory policy. No scientist had ever condoned such behavior, but most preferred to handle the investigation or resolution internally and quietly, regardless of the source of funding or the prestige or standing of the accused.

Once the issue drew public attention, it became quickly clouded by emotion, personality, power-brokering, and politics. There were reiterative debates over what action(s) should be considered "wrong" (and if so, whose rules had been broken and who should investigate) and whether even objective analysis of misconduct might somehow damage the reputation of the research system overall. Scientists, policymakers, philosophers, and lawyers argued over whether "the problem" was that of "a few bad apples" or "a rotten barrel," and some even questioned whether the scientific community should voluntarily cooperate with federal investigations. Fortunately, more objective, measured discussion has replaced the volatile atmosphere of the 1980s and early 1990s. However, the initial reactions of many scientists who purported to speak for all of science, coupled with delays in university investigations and the development of ethics codes, not only resulted in further expansion of the federal regulatory presence on university campuses but also helped to create a situation in which an accusation of misconduct, whether warranted or not, can now trigger years of expensive and time-consuming litigation.

Given that virtually no one approves of egregious fakery, fabrication, or plagiarism in science, how did we arrive at this point? To understand, we must look first to the cultural and political environment in which the issue arose.

Science's Cultural and Political Context

Americans have high expectations of science (1). The American public holds scientists in high esteem, second only to physicians in social prestige. Scientists are generally perceived as well-intentioned seekers of truth; universities, as cathedrals of learning and as producers of knowledge vital to the health and welfare of society. A vigorous scientific research enterprise is considered essential to the U.S. domestic economy and to American competition in foreign markets. Integrity on the part of scientists has simply been assumed as part of the formula for research support. The reliance on academic institutions to manage billions of dollars of research support is testimony to that political trust.

The discovery of fraud and deception threatened to tarnish science's pristine image. In addition, in the 1980s and 1990s, the United States experienced a social climate that, while not necessarily more moral, became decidedly more moralistic. Politicians seemed eager to investigate allegations of all sorts, and the news media were inclined to publicize allegations even before they had been investigated thoroughly. Citizen frustration with reports of widespread fraud and abuse in government contracting further heightened the level of skepticism toward all who received government support.

Science itself had also begun to change. Scientists were clinging to a self-image and norms forged in an era of single-investigator projects, but the drive for economic competitiveness encouraged a multidisciplinary, multi-institutional, multinational research model with multiple collaborators and therefore multiple authors. The average number of co-authors per publication began to grow in all

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fields, as did disputes among co-authors over credit and intellectual property rights (2). All this was taking place in a profession in which credit (not necessarily money, patents, or even fame) had always been considered the most essential outcome of one's research. Without our ideas, as expressed in our communications, we are nothing. We do not make products, we make ideas. Steal my words, and you steal my authorship. Steal my idea, and you steal my identity as a scientist.

Acts of faked or misrepresented scientific and technical data, falsified professional credentials, misidentification of authorship, and plagiarism are neither 20th-century inventions, nor limited to any one research field or national research system (3). When problems have been uncovered, scientists around the world have initially tended to act much the same. They have characterized the offender as aberrant, argued that the episode is isolated, or attempted to explain it as caused by stress, bad judgment, or moral corruption (or all three). Such reluctance to acknowledge the painful reality of deception by a colleague has usually been followed by conscientious investigation of the situation and a renewed sensitivity to the importance of integrity in science.

What distinguishes the U.S. context has been the extent of government involvement. Not all countries respond to abuse, error, or accident with government regulation, but that is a standard reaction in the United States. From the 1850s regulations governing the manufacture of steamship boilers (which had a tendency to explode and kill their operators) to modern legislation requiring motorcycle riders to wear helmets, the American political system routinely uses the law and regulatory standard-setting—rather than, say, social pressure alone—to effect change in private, institutional, or commercial behavior.

Such regulation may not be triggered necessarily by proof of increased fraud and abuse. More likely, it responds to heightened sensitivity or awareness of a problem, or to the perception of change. We cannot, for example, measure whether there is more (or less) fraud in science today than in previous eras because of both insufficient quantitative data and inherent inconsistencies in how misconduct has been defined. Only a few sensational episodes of scientific fraud are well documented; many, such as the Piltdown skull forgery (4, 5), remain a mystery. Allegations often survive only in participants' memories or departmental gossip; formal records may not exist or may be locked in confidential personnel files. Few of the people caught up in these cases, whether accused, accuser, or investigator, have been willing or able to be interviewed by social scientists. The most comprehensive details on recent cases have been contained in news reports (which vary widely in accuracy and credibility) or the sanitized announcements from government investigatory agencies.

The increased media attention (6) also happened to coincide with attention to ethical conflicts in all walks of life, from the tribulations of television evangelists and star athletes to the foibles of legislators. Journalistic attention to scientific misconduct helped to spotlight science as yet another area of concern, and it drew politicians like moths to a flame.

In the history of U.S. science after World War II, the first contemporary instance of faked scientific data to receive significant public attention, the case of William T. Summerlin (7, 8), was covered only briefly in the news. In 1974, Summerlin's research at the Sloan-Kettering Cancer Center in New York focused on skin cancer; he was investigating the use of tissue culture to facilitate genetically incompatible skin transplants. Before a meeting with the center's director, Summerlin reportedly darkened the grafts on two white mice to imply success with the experiment. When confronted with the deception, Summerlin blamed mental and physical exhaustion, institutional pressure to publicize the research, and an "unbearable clinical and experimental load" which had "numbed" his judgment (7).

The Summerlin case contained many elements that would be found in subsequent cases: high-profile research with accompanying pressure to succeed; a popular scientist and powerful supporters; claims of momentary misjudgment or unusual stress; and an ambiguous resolution. Accused scientists sometimes maintained their jobs (and salaries) for years while the institutional investigations dragged on. The eventual penalties imposed tended to be linked to loss of status (e.g., loss of appointment as director of a project or laboratory) or temporary restrictions on the accused individual's ability to participate in the life of science (e.g., prohibiting service on grant review committees or submission of proposals to federal agencies) rather than to monetary fines, imprisonment, or even loss of credentials, such as those imposed when other types of professionals were found guilty of ethical violations. No scientist went to jail. Few of the cases found their way outside the realm of institutional investigation or administrative law cases.

To observers outside the community of science, such resolutions were (perhaps understandably) perceived as coverups, especially when scientists whose careers had received collateral benefit from the fraud (e.g., those who had allowed their names to be listed as co-authors on papers later found to be based on fabricated data) were also not called to accept responsibility and were excused from censure. This phenomenon of phantom or honorary co-authorship had become routine practice in some fields or laboratories, and those who continued to defend the practice found themselves arguing that a scientist's willingness to accept credit should not always imply an equal responsibility to accept blame. It was an untenable position, and it quickly drew criticism from all sides.

Congressional Attention to Detail

Most of the time, Americans tend to criticize their legislators for insufficient action. In the case of scientific fraud, some scientists found themselves complaining about the opposite. When delays in the Harvard and NIH investigations of the John Darsee case at Harvard Medical School (9)

began to draw allegations of coverup in the press, political action followed quickly. The first formal Congressional attention to scientific misconduct took place in 1981, in two House subcommittee hearings (31 March and 1 April) (10, 11). The representatives' questions centered primarily on whose rules should be applied to conduct such as Darsee's, whether scientists could demonstrate that such misbehavior was an aberration and unrepresentative of the integrity of science overall, and whether the scientific community was investigating the problem with sufficient vigor. The Chairman of the House subcommittee, Albert Gore, Jr., observed,

"We need to discover whether recent incidents are merely episodes that will drift into the history of science as footnotes, or whether we are creating situations and incentives in the biomedical sciences, and in all of Big Science, that make such cases as these the tip of the iceberg" (10).

This litany would be echoed in other Congressional hearings throughout the next decade.

In retrospect, it appears obvious that many scientific leaders misread the legislators' intentions and goals. Congressional oversight, in fact, routinely focuses on ethical issues in an effort to crystallize political opinion about an issue or to urge change within a government agency, but does not always seek to prompt new legislation (12). In the early misconduct hearings, the scientific community was urged to take charge of policing its own ethical behavior, and to articulate its standards more formally. By establishing ethics codes and by investigating allegations promptly and thoroughly, scientists might be able to forestall the imposition of new federal regulations.

Politicians are rarely shy about expressing disappointment, and by 1988 many legislators had lost patience with the scientific community's excuses and with the failures in various investigations of alleged misconduct. A concurrence of events combined to propel the issue onto the front page. On April 11 and 12, hearings of two separate House subcommittees featured the testimony of harassed whistleblowers and tales of coverup (13, 14). One of these hearings opened the door on what became the most high-profile case of all, the MIT investigation of allegations against Thereza Imanishi-Kari, a colleague and collaborator of Nobel laureate David Baltimore (15, 16). Three days later, another scientist, psychologist Stephen J. Breuning, was indicted in federal court and charged with falsifying research project reports to the National Institute of Mental Health (17–19).

The Breuning case in particular demonstrated that science did not always self-correct. Breuning's research data had been widely reported and widely used: From 1980 to 1983, his published papers represented at least one-third of all scientific articles on the topic, and later citation analysis showed that, from 1981 to 1985, his work had "meaningful" impact on the literature (20). Moreover, his conclusions had been used to justify patient treatments. Even though no harm occurred, this case seemed to speak clearly to many nonscientists, and it helped to draw further press and political attention to the issue.

Nevertheless, resistance to Congressional suggestions of reform persisted in the biomedical research community. An editorial in the *New England Journal of Medicine*, for example, argued against establishment of *any* federal ethics oversight system and asserted that Congress was simply "responding to false impressions" and should therefore back off: "The biomedical-research community is willing and able to police itself and is taking steps to do so more effectively" (21). Unfortunately, many universities, laboratories, and scientific associations continued with business as usual, moving sluggishly (if at all) to write ethics codes or tighten procedures for research supervision and training. Investigations by both universities and government agencies seemed inordinately slow and unresponsive to calls for change (22).

Another arena in which positive change might have occurred more quickly was the scientific publishing industry (6). Many prominent scientific journals had refused to withdraw tainted articles even after they were proven to be based on fabricated data (23). In most cases, the publishers were concerned that if even one co-author objected to retraction or correction, the journal might be sued for libel. After Stephen Breuning pled guilty in federal court, more than 50 of his articles remained in dispute for months; Breuning refused to discuss the matter publicly, and the journals would not retract the articles unless all authors agreed (24).

Because misconduct had so often come to light after publication in a journal, questions also began to be raised about the reliability of peer review, accuracy of editorial scrutiny, and integrity of the scientific literature overall (6). Despite the importance of such issues, the publishing community, too, seemed reluctant to address them head on or to develop general guidelines for authorship, reviewing practices, and the like. In the United States, the historic separation of press and government and the traditional autonomy of the scientific communication system from government support or subsidy, reinforced this sense of independence from such political concerns. A few well-known editors engaged in public debate or testified in Congress, but most publishers, journals, and editors watched from the sidelines. Journals that had published problematic articles were portrayed as victims; the editors and referees characterized as duped by devious authors.

By 1990, political attention finally began to shift away from attaching blame and to focus on improving the investigatory process and developing strategies for prevention or deterrence (25–27). The difference in how the National Institutes of Health (NIH) and the National Science Foundation (NSF) responded to political pressure is instructive. Although the NIH began in 1981 to reformulate its policies for grantees, and in 1985 the Health Research Extension Act (Public Law 99–158) directed the Department of Health and Human Services (HHS) to develop regulations for all *applicants* for grants, contracts, and cooperative agreements, and require institutions to report allegations and investigations to the government, there was little political satisfaction

with the actual pace of development of regulations. Legislation in 1993 established an Office of Scientific Integrity, which was eventually moved from NIH to HHS and renamed the Office of *Research* Integrity (ORI).

Looking back at the history of ORI, one cannot help but conclude that several factors inhibited a smooth start from the outset: the hot lights of media and Congressional attention, the tendency of Congress to micromanage agencies (particularly this one), and the fact that, from the beginning, ORI was involved in investigating one of the most sensational misconduct cases in the history of American science, the Baltimore case (15, 16). Every action and decision of ORI was dissected by the interested parties at every step. It was not an auspicious beginning, and it gave ammunition to the critics of federal misconduct regulation. Fortunately, more careful attention to procedure, tighter management, and an expanding list of closed cases have now put the NIH effort on a steadier course.

NSF was more adroit in establishing its policies and procedures. The agency defined the limits of its inquiries early and incorporated its investigatory unit within the NSF Office of the Inspector General (IG). By placing the misconduct unit within the IG's office, NSF avoided the stigma associated with a separate ethics police force, and the agency also made a strong statement about the relationship of ethical behavior to the expectation of accountability in how institutions manage their federal funding (28).

Retrospective Assessment

The issue of scientific misconduct thus became a controversial policy issue in part because of failures in communication and a lack of a spirit of cooperation between the scientific community and the federal government that supported its work. Contrary to what some scientists claimed, Congress was not out to destroy science; most of the House members concerned with this issue were the same ones who fought vigorously for increased research appropriations. The record clearly shows that agencies and their grantees were initially urged to investigate the allegations themselves, to resolve the inquiries swiftly and fairly, and to do whatever possible to prevent future occurrences. They were frequently warned that regulations should be perceived as a last step, to be taken only if the problems continued. Unfortunately, a few outspoken scientific leaders greeted these congressional suggestions as unreasonable and unwarranted attempts to restrict scientific freedom. Their attitudes discouraged the type of response that might have convinced Congress that formal regulation was unnecessary.

Looking back, we can also see that the scientific associations and organizations failed to respond swiftly enough to the calls for development of ethics codes and comprehensive ethics training programs, despite a history of attention to other ethical issues (29). Had there been a concerted effort by all the major societies, similar to that which the Society for Neuroscience has undertaken, many of the harshest provisions of the regulatory apparatus could have been avoided. Moreover, formal codes provide investigating committees with a more objective statement of the norms by which to judge actions, and they enhance the likelihood that investigations will be consistent, fair, and sensitive to the differences among research field practices.

U.S. scientists today must cope with a regulatory apparatus that no one believes works all that well. The shift from print-based to electronic publishing is raising new issues and potential opportunities for deceptive presentation of data, plagiarism of text and graphics, and fraudulent appropriation of ideas (30). Fortunately, the publishing community has begun finally to respond to such challenges. The International Committee of Medical Journal Editors, for example, has taken the lead in defining such things as responsible authorship (31, 32). More journals now require authors to attest to the originality of articles at the time of submission, or to state that all co-authors have read the text and have agreed to submit it. More and more journals require authors and reviewers to disclose potential conflicts of interest.

To peer into the future of the scientific misconduct issue is to see many of the lessons of the past in sharp relief. As economic competitiveness further dominates domestic and international political choices, both governments and institutions are focusing attention on "the whole set of processes . . . by which new knowledge gets converted to economic wealth" (33). As a result, administrators and managers have become more concerned with how well research programs work, and with eliminating any impediments to their success, including the potential for fraud and waste. Policymakers who had previously been unconcerned with the publication process or the ethics of science now eagerly inquire about how and to whom scientists communicate their ideas, and seek assurance of their integrity. The "logic of the situation," historian Bruce L. R. Smith (34) has observed, is that "science will most likely be conducted under more restrictive conditions . . Scientific activity will become more regulated even as resources decrease."

The scientific community's anticipatory concern about such public policy issues could help to avoid repeating the damaging controversies of the past, in which the political outcome did not alleviate the original problem and the regulatory burden on science only increased further. Discussions of ethical issues in research, rather than being a rehash of old grievances or relegated to seminar afterthoughts, can effectively use the history of science to point the way toward change, and thereby help to establish a more comfortable and healthy ethical climate for all scientists, including those in the next generation.

1. LaFollette MC. Making Science Our Own: Public Images of Science, 1910–1955. Chicago: University of Chicago Press, 1990.
2. McDonald KA. Too many co-authors? Chron Higher Ed 41:A35–A36, 1995.
3. Lock S, Wells F. Fraud and Misconduct in Medical Research (2nd ed). London: BMJ Publishing Group, 1996.

4. Spencer F. Piltdown: A scientific forgery. New York: Oxford University Press, 1990.
5. Weiner JS. The Piltdown Forgery. New York: Oxford University Press, 1955.
6. LaFollette MC. Stealing into Print: Fraud, Plagiarism, and Other Misconduct in Scientific Publishing. Berkeley: University of California Press, 1992.
7. Hixson J. The Patchwork Mouse. Garden City, NJ: Anchor Press/ Doubleday, p220, 1976.
8. McBride G. The Sloan-Kettering affair: Could it have happened anywhere? *JAMA* **229:**1391–1410, 1974.
9. Relman AS. Lessons from the Darsee affair. N Engl J Med **308:**1415–1417, 1983.
10. U.S. House of Representatives. Fraud in Biomedical Research, 1981: Hearings Before the Committee on Science and Technology, Subcommittee on Investigations and Oversight, 97th Cong., 1st Sess., 1981.
11. LaFollette MC. A foundation of trust: Scientific misconduct, Congressional oversight, and the regulatory response. In: Braxton JM, Ed. Perspectives on Scholarly Misconduct in the Sciences. Columbus, OH: Ohio State University Press, pp11–41, 1999.
12. Aberbach JD. Keeping a Watchful Eye: The Politics of Congressional Oversight. Washington, DC: Brookings Institution, 1990.
13. U.S. House of Representatives. Scientific fraud and misconduct and the federal response, 1988: Hearing of the Committee on Government Operations, Subcommittee on Human Resources and Intergovernmental Relations, 100th Cong., 1st Sess., 11 April, 1988.
14. U.S. House of Representatives. Scientific Fraud and Misconduct in the National Institutes of Health Biomedical Grant Programs, 1988: Hearing of the Committee on Energy and Commerce, Subcommittee on Oversight and Investigations, 100th Cong., 1st Sess., 12 April, 1988.
15. Kevles DJ. The Baltimore Affair: A Trial of Politics, Science, and Character. New York: WW Norton & Co, 1998.
16. Sarasohn J. Science On Trial: The Whistle Blower, the Accused, and the Nobel Laureate. New York: St. Martin's Press, 1993.
17. Valentine PW. Drug therapy researcher is indicted. Wash Post **111:**A1, A14, 1988.
18. Wheeler DL. Researcher is indicted for falsifying data and impeding investigation of his work. Chron Higher Ed **34:**A4, A12, 1988.
19. Zurer PS. Researcher criminally charged with fraud. Chem Eng News **66:**5, 1988.
20. Garfield E, Welljams-Dorof A. The impact of fraudulent research on the scientific literature: The Stephen E. Breuning case. In: Peer Review in Scientific Publishing: Papers from the First International Congress on Peer Review in Biomedical Publication, Chicago, IL, 10–12 May 1989. Chicago: Council of Biology Editors, p53, 1991.
21. Angell M, Relman AS. A time for congressional restraint. N Engl J Med **318:**1462–1463, 1988.
22. Steneck NH. Research universities and scientific misconduct: History, policies, and the future. J Higher Ed **65:**310–330, 1994.
23. Dalton R. Journals slow to retract Slutsky research errors. The Scientist **2:**1, 4, 1988.
24. Hostetler AJ. Fear of suits blocks retractions. The Scientist **1:**1–2, 1987.
25. U.S. House of Representatives. Scientific Fraud and Misconduct: The Institutional Response, 1989: Hearing of the Committee on Energy and Commerce, Subcommittee on Oversight and Investigations, 101st Cong., 1st Sess., 9 May, 1989.
26. U.S. House of Representatives. Scientific Fraud, 1990: Hearing of the Committee on Energy and Commerce, Subcommittee on Oversight and Investigations, 101st Cong., 2nd Sess., 14 May, 1990.
27. U.S. House of Representatives. Committee on Science, Space, and Technology, 1990: Maintaining the Integrity of Scientific Research, 101st Cong., 2nd Sess., 1990.
28. Buzzelli DE. The definition of misconduct in science: A view from NSF. Science **259:**584–585, 647–648, 1993.
29. Bulger RE, Heitman E, Reiser SJ. The Ethical Dimensions of the Biological Sciences. Cambridge, England: Cambridge University Press, 1993.
30. Peek RP, Newby GB. Scholarly Publishing: The Electronic Frontier. Cambridge, MA: The MIT Press, 1996.
31. International Committee of Medical Journal Editors. Retraction of research findings. BMJ **296:**400, 1988.
32. International Committee of Medical Journal Editors. Retraction of research findings. BMJ **302:**338–341, 1991.
33. Armstrong JA. Research and competitiveness: Problems of a new rationale. The Bridge **23:**3–10, 1993.
34. Smith BLR. The accountability of science. Minerva **34:**45–56, 1996.