1 When Interests Collide

Social and Political Reactions

For it is a matter which concerns every author personally . . . that his own proper labors, the hard-earned acquisitions of his own industry and intellect, should not be pilfered from him, and be represented by another as his own creation and property.

—*The Lancet*[1]

It's sad if people consider them high standards. They should be ordinary standards.

—Margot O'Toole[2]

Truth swings along a wide arc in the late twentieth century. In politics and popular entertainment, people accept deception as commonplace—campaign image making, video synthesis, and the trompe-l'oeil of fantasy parks seem normal parts of life. The trust that society places in science, however, traditionally assumes different standards, including assurances of authenticity and accuracy in all that science does or recommends.

The strength of this faith in science's intrinsic truthfulness helps to explain the roots of the current political controversy in the United States over "scientific fraud." Forgery, fakery, and plagiarism contradict every natural expectation for how scientists act; they challenge every positive image of science that society holds.

The sequence of events that led to federal regulations (and the heated political debate surrounding them) initially attracted little attention, from journalists or even from the scientific community. In the 1960s and 1970s, the most outrageous examples of faked data or plagiarism were dismissed as aberrations, as unrepresentative of the integrity of scientists overall.[3] Then, in the 1980s, cases began to surface at Harvard, at Yale, at state universities, and in several different fields. Scientists employed at the best places, who had attracted hundreds of thousands of dollars in federal research funding, and who had published in top-ranked journals were accused of forging, faking, or plagiarizing their way to success. Reputable laboratories and schools were accused of not just indifference but coverup. Soon, the congressional committees charged with overseeing U. S. research and development (R&D) programs began to investigate why the management and evaluation systems created to monitor research and research communication had apparently failed to detect or prevent misconduct. These monitoring systems had served science well for decades; but the basic concept of peer review—the reliance on experts to review journal manuscripts before publication—rested on tacit assumptions about the honesty and truthfulness of all participants in the process, and these were exactly the old-fashioned virtues that deception mocked.

Now the klieg lights are on. What began as private discussions among professionals stands unshielded in the spotlight of national politics. Moreover, controversy has spread beyond the relatively few institutions that have been directly implicated and has spilled from the hearing rooms of Capitol Hill to the pages of morning newspapers.

No other science policy issue of our time contains such potential for altering the who, what, how, and whether of the production and dissemination of scientific knowledge and for affecting the rights of individual authors and publishers. This book focuses on one segment of that enormous issue—how the controversy affects communication practices and policies in the journals that disseminate the results of scientific research—using it as a window on the larger debate. What does the discovery of unethical conduct say about current systems for evaluating and disseminating research-based knowledge? And how may those systems be changed by efforts to detect, investigate, mitigate, and prevent wrongdoing?

The issue has a number of unusual characteristics worth noting at the outset. One is the level of emotion present, explicitly and implicitly, in almost every discussion. One can point to similar psychological reactions to forgery or plagiarism in other fields of scholarship and creativity. In reacting to literary plagiarism, for example, Edgar Allen Poe once wrote that he was "horror-stricken to find existing in the same bosom the soul-uplifting thirst for fame and the debasing propensity to pilfer. It is the anomaly, the discord, which so gravely offends."[4] Identical disbelief greets the news that scientists have seemingly faked data for the sake of a few more publications. Deception seems anomalous. Time and time again, shocked co-workers have been heard to mutter such phrases as "all of us thought very highly of this young man. . . ," and then they shake their heads and turn away in disgust.[5]

Another significant aspect of the issue is that the discovery of unethical or illegal conduct contradicts scientists' self-images, their beliefs about how "real" scientists act. As a consequence, the community often engages in wholesale denial of the problem's significance. There is deep-seated disagreement over elementary procedural points, leaving investigators uncertain about whose standards to apply in assessing guilt or innocence. Because congressional interest in research fraud coincides with other prominent and highly controversial science policy debates, such as those over resource allocation, priority setting, and university overhead, and with the perception that standards are lax in all professions, scientists who are accustomed to deafening applause suddenly feel beleaguered by unwanted criticism.

Scientists also tend to perceive themselves as defenders of objectivity in an irrational world, struggling to rebuff superstition and pseudoscience. Popular culture reinforces those self-images when it assigns scientists the role of seekers, determiners, and guardians of truth.[6] Deception and fraud shatter the glistening surface of these images.

The discovery of fraud among a nation's intelligentsia also provokes reconsideration of its core values, revealing where those values may be self-contradictory. Entrepreneurship, independence, and ambition, for example, are important traits in American culture. As a result, the nation's scientists have been encouraged to stretch their imaginations and energies to advance the frontiers of knowledge, encouraged to engage in intense competition as a matter of course. Ironically, Americans also advocate fair play, and they expect winners to be humble and generous, to share credit where credit is due, and not to steal (or award) credit falsely. The greedy overtones of fraud and plagiarism seem intrinsically out of character for scientists, seem unacceptable behavior for society's heroes.

In addition to these emotional and cultural components, the political controversy has been distinguished by unrelenting disagreement over basic definitions. Is "scientific misconduct" an ordinary social crime committed by deviant individuals, does it represent an indicator of slipshod research management, or is it a symptom of widespread social immorality? Whose standards should be applied and what specifically should be permitted or prohibited? The failure to develop widely accepted definitions of expected research conduct has resulted in years of unproductive debate and unworkable *ad hoc* policies.

The political dispute over research fraud taps into larger issues of national policy. The political framework adopted for post-1945 organization of U.S. science assumed that scientists were trustworthy and that their technical expertise was always reliable. American researchers have enjoyed considerable autonomy in directing their own research agendas and procedures compared to scientists elsewhere in the world. Each report of suspected research fraud tests the soundness of this approach; each proof of wrongdoing further erodes political support for unilateral autonomy and raises more calls for accountability. The combined attention of congressional committees, federal science agencies, and the press has thus slowly shoved the focus from initial concern about personal conduct (and a preoccupation with remedial moral counseling) to foundational questions of who sets the policies, public and private, for scientific research and communication. And that debate will have implications for the international systems of scientific communication, including commercial and nonprofit periodical and book publishers, secondary indexing and abstracting services, libraries, and similar information processing organizations around the world.

The recent suggestions that journals afford ideal locations for detecting, investigating, and rooting out fraud must therefore be carefully considered. One of the glories of U.S. science (and perhaps one of the keys to its success) is the extent of freedom of communication within the research system. In theory, any person is free to publish and speak about science, no matter how innovative, radical, or ridiculous the idea because, although peer review is widely regarded as a means of *authenticating* published science, it was never intended as a method of scientific censorship. Mistakes or errors within the system may simply represent a price paid for a free market in scientific information and, if the risk is unacceptable, then the alternatives should be carefully debated. Locating an appropriate balance between the need to prevent and punish the unethical practices described in this book and the need to preserve individual freedom of expression will require cooperation among the scientific community, the publishing community, and government policy makers. This book recounts how their various interests continually intertwine—and sometimes collide head on.

SETTING THE SCENE

Even under the best of circumstances, allegations of illegal or unethical professional conduct stimulate a maelstrom of conflicting reactions. Colleagues and friends alike react emotionally—sometimes with denial, sometimes outrage. Everyone has opinions. No one stays quiet.

To illustrate the types of issues and the miasma of conflicting interests and attitudes that typically surround misconduct allegations in the context of scientific publishing, I begin with descriptions of two episodes from the nineteenth century and two from more recent times. Research procedures and protocols may have changed over the course of the years, but human failings remain constant.

To Read, to Translate, to Steal

It is routine practice in research and scholarship to award special prizes for particular writings and then, as part of the reward, to publish the essay. Such a process took place in 1852, when the British Royal College of Surgeons awarded its Jacksonian Prize to an essay by Henry Thompson and published his essay in London in 1854. Two years later, in Paris, scientist José Pro presented an "original" essay on the same subject to the Societé de Chirurgie, which awarded him their highest honor. Twenty-three of the twenty-six quarto pages of Pro's essay was later proved to be a "literal translation" of Thompson's prize essay.

British scientific reaction was characteristically understated in its outrage. *Lancet*, a medical journal published in London, printed extracts from the two works side by side as indisputable evidence of the plagiarism; the editors prefaced the columns with sarcastic praise for "M. Pro's undoubted merits as a translator."[7] In biting criticism of the French medical establishment, *Lancet* wrote: "M. Pro, in perpetrating one of the most shameless and extensive plagiarisms which has been brought to light for many years, has paid a great compliment to their real author, and . . . the Societé de Chirurgie, by conferring their highest distinction upon the supposed original observer, have quite unwittingly confirmed it."[8]

Lancet recognized that the case had implications far beyond simple plagiarism, especially in the world of research where scientists must trust other scientists to give them due credit. The editorial pointed out that the wrong was not just Thompson's concern alone; it was shared by the entire community. Every author, the editors wrote, desires "that his own proper labors, the hard-earned acquisitions of his own industry and intellect, should not be pilfered from him, and particularly that those acquisitions should not, in some more or less distant parts of the world, be represented by another as his own creation and property."[9]

Like many similar episodes, this one was later used as a sort of Aesop's fable of scientific morality, to introduce an account of similar plagiarism in the United States.[10] During the winter of 1865–1866, George C. Blackman, a physician in Cincinnati, Ohio, received a copy of a prize-winning medical essay that had just been published in France. In 1862, the Academy of Medicine of Paris had proposed a history of research on *Ataxia Locomotor Progressive* as the subject of a prize essay, and Paul Topinard, a member of the Societé Medicale d'Observation, had entered and won the competition by adding original clinical observations on the disease to his analysis of data on 252 previously reported cases. Like countless scientists before and since, Blackman loaned his copy of the Topinard essay to a colleague, Roberts Bartholow, who had "professed a desire to study the subject."[11]

A few months later, an article by Bartholow, titled "The Progressive Locomotor Ataxia (*Ataxia Locomotor Progressive*)," appeared in an issue of the *Journal of Medicine*, published in Cincinnati. In a note printed at the bottom of the article, Bartholow stated that he had made "liberal use" of Topinard's essay. In fact, his debt to Topinard was more than token. After giving the details of an Ohio case that he had studied, Bartholow devoted the remainder of the article to what was actually a *literal* translation of the French essay.

When the publisher of the Cincinnati journal discovered the apparent plagiarism, he asked Bartholow to address the accusation in part two of the series, which was scheduled to appear in the next issue. Bartholow complied, stating:

> It having come to my knowledge that some persons consider my reference insufficient to the work of Dr. Paul Topinard (*De L'Ataxie Locomotor Progressive* etc.) I beg to inform the readers of the Journal that my intention was, by the term, "liberal use of the Essay" to express the idea of a synopsis of such parts as suited my purpose in the preparation of my article. As the work is a voluminous one, it would be impracticable to present a translation in the number of pages allotted to me. The present, as the former article, is a synopsis chiefly of the views of Topinard.
>
> ——R. B.

George Blackman was apparently unsatisfied with Bartholow's explanation and decided to publicize further what he regarded as his colleague's wrongdoing.[12] Like the *Lancet* editors a decade earlier, Blackman became a "nemesis," attempting singlehandedly to punish unethical behavior through dogged publicity. In 1868, he privately published a pamphlet in which he outlined the allegations, drawing on a description of the Thompson/Pro story for comparison. "As preliminary to the main object of this paper," Blackman wrote, "we will briefly refer to a literary transcription of 'extraordinary peculiarity,' which occurred in the great metropolis of France, and then proceed to show how the said transcription has even been eclipsed by a writer of the Queen City."[13]

Blackman facetiously praised his colleague's abilities as a translator and then accused Bartholow of having attempted to dispel suspicion by acknowledging a "liberal" use of "a celebrated French Prize Essay on the same subject," when in truth the use was *literal*. Blackman juxtaposed columns from the two Bartholow articles and the original prize essay, demonstrating that not only the texts, but also their openings and subheads, were identical in many places. Although Blackman acknowledged that the third part of Bartholow's article (published in June 1866) bore "less evidence of being 'a debasing *translation* and unscrupulous assumption' of knowledge . . . ," he remained convinced that Bartholow's original intention was to deceive. Blackman's condemnation of the character of the accused was scathing; his desire to expose him, intense; and his method, publicity.

To Republish

Such attitudes toward theft of ideas and such actions to publicize it are not simply quaint artifacts of the nineteenth century. They typify attitudes within science that are expressed vigorously today, sometimes behind the scenes and sometimes in "corrective action" taken to publicize unethical behavior. A typical episode occurred in the 1980s, when news reports began to describe a "rash of what appears to be piracy . . . in the scientific literature" involving a Jordanian biologist, E. A. K. Alsabti, and several journal articles he had published.[14]

Alsabti apparently took articles published in one country (usually in small-circulation journals) and published them under his own name in journals published elsewhere. He also allegedly retyped previously published articles and then submitted them to other journals. In one instance, he reportedly obtained a preliminary version of a manuscript which he successfully published under his name before the original author could get the article into print.[15]

Alsabti did not have a permanent faculty appointment in the United States and, during the period in which he engaged in these activities, he moved frequently. Because none of the universities or laboratories in which he had worked was willing to conduct a formal investigation, several scientific journals, most notably *Nature* and the *British Medical Journal*, took up the banner. They independently addressed the problem by publicizing the wrongdoing—printing paragraphs from the original texts side by side with Alsabti's articles, along with editorial comment on his "piracy."[16]

The journals' actions represent the only direct confrontation of Alsabti's behavior by any institution at the time. Alsabti neither admitted to plagiarism nor retracted the articles in question, although (after a legal battle that stretched for years), the Commonwealth of Massachusetts was able to revoke his license to practice medicine, citing the alleged plagiarisms.

Nature and the *British Medical Journal* aggressively expressed community outrage in one of the few ways possible, given the lack of available mechanisms or procedures for bringing charges on plagiarism and also given Alsabti's steadfast assertion of innocence. The journals pilloried Alsabti just as the *Lancet* and George Blackman had done in similar cases almost a century earlier. By publishing comparative texts, the editors led readers to act as a "jury of peers" and evaluate the actions for themselves. They also forced action when the normal institutions for doing so either could not or would not respond.

To Publicize, to Analyze

Scholarly and scientific journals, then, may unwittingly aid in disseminating plagiarisms or descriptions of faked data or artifacts; they also attempt to correct these lapses by giving publicity to wrongdoers and by serving as the forums in which scientists can discuss or debate issues of professional ethics. One of the most troubling examples of the latter functions took place in the 1980s. The resulting controversy drew mass media attention, provoked congressional hearings, and emphasized dramatically that editors and publishers could not stand back and wait for universities or government agencies to sort out the disagreements on their own. The publishing community not only had a stake in the debate but, one way or another, it was being pulled into the political controversy, whether it wanted to be there or not.

This episode began in the laboratories of one of the most prestigious medical schools in the world. John Darsee was a promising young researcher at the Harvard Medical School, working under the mentorship of a top cardiologist, Eugene Braunwald. During his previous graduate work at Emory University, and later at Harvard, Darsee had appeared to his professors, mentors, supervisors, and most co-workers as a typical, bright, energetic, and ambitious post-doc. His research, part of an important multi-institutional project, was funded by the National Institutes of Health (NIH); when the larger project concluded, several years after the Darsee controversy died down, its final results were, in fact, widely praised. Scandal erupted in 1981 when Darsee was accused of manipulating, inventing, or otherwise compromising the integrity of data reported in over a dozen coauthored papers and over fifty abstracts based on his Harvard cardiology research.[17]

Darsee's dismissal and the sensational details of the allegations attracted considerable press coverage. The event seemed to imply that fraud could occur even at the "best" universities, where integrity and truth were emphasized in school mottos and inscribed on the very buildings in which the scientists worked. As the official conclusions of Harvard University, Emory University, and NIH investigating committees were released in

1982 and 1983, one issue remained unanswered, however: exactly what role had been played by Darsee's coauthors? Darsee had published the faked data in many articles that also bore the names of senior faculty and junior postdoctoral researchers, both at Emory and at Harvard, yet these scientists had apparently failed to detect the fabrication and had willingly cooperated as coauthors.

In retrospect, it may seem remarkable that no one questioned Darsee's phenomenal rate of production or asked to see his data, but such a level of trust among co-workers is normal and, indeed, encouraged among scientists. As it became known that many of Darsee's coauthors were, in fact, "honorary"—that is, they had not actually participated in the research or in the writing of the questionable articles, yet had allowed their names to be listed on the papers—embarrassing criticism of them began to appear in the press. In editorials and in letters to scientific journals, outraged scientists questioned the wisdom of prevailing standards for authorship, the quality of editorial review at major journals, and the growing practice of "nonparticipating" coauthors. The Darsee episode began to spark intense debate about who should receive credit for collaborative research, and why.

The practice of honorary coauthorship particularly disturbed two biologists, Walter W. Stewart and Ned Feder, who worked in the National Institute of Arthritis, Diabetes, and Digestive and Kidney Disease (NIADDKD), part of the NIH. To Stewart and Feder, any coauthor who accepted credit for an article should accept responsibility for its accuracy (and therefore accept blame if the article was later found to be flawed). As a sideline to their own research, the two biologists began to analyze "the Darsee coauthors" and to write an exposé of the practice of honorary coauthorship in general.

The early scientific work of Stewart and Feder is highly regarded, but they are biologists, not social scientists; they lack training in the theories and methodologies of sociology, and the early drafts of their manuscript, couched in the language of the social sciences, reflected their unfamiliarity with its pivotal literature and interpretations.[18] Had they phrased their remarks as general editorial comments by two biologists on the issue of coauthorship, without purporting to be presenting data collected as part of social science research, their manuscript might well have received a normal review and appeared, with little fanfare, in any number of journals. Instead (and perhaps because it was the world they knew best), they attempted to cast their article as objective "science." They focused on a specific group of John Darsee's coauthors, identifying them directly by name or indirectly by citation to the articles in question. They used blunt language in condemning the coauthors' actions. And they doggedly attempted to publish their article in the most eminent scientific journals in the world.

Their efforts to publish quickly became a *cause célèbre.*[19] Stewart and Feder first gave a draft to John Maddox, editor of *Nature*, in 1983; by early in the next year, the manuscript had been revised repeatedly. Some time after Maddox gave a version to one of the "Darsee coauthors" named in the manuscript, a lawyer for Eugene Braunwald (the most prominent of the scientists named, and the head of the lab in which Darsee had worked) wrote to Stewart and Feder, claiming that the draft contained "defamatory statements." The attorney had similar discussions with representatives of *Nature*.

Over the next four years, while they continued to seek publication, Stewart and Feder were threatened with lawsuits claiming that their manuscript contained libelous statements.[20] The coauthors who threatened to sue did not condone or excuse Darsee's behavior, but they were convinced that publication of the article would damage their professional reputations. One attorney began to warn journal editors that even circulating the manuscript to peer reviewers, much less publishing it, would draw legal complaints.[21] Such threats raised anew the prospect that private disputes between scientists might be propelled into the courts and they pushed the issues to a new stage.[22] Heretofore, most science editors, authors, and referees had—with good reason—tended to regard editorial decision making and the peer review system as closed, protected activities, more or less like professional conversations among colleagues.

The accusation of libel further delayed publication; by December 1984, after more revisions, Stewart and Feder had received NIH approval on a "final" version, but Braunwald's attorney continued to claim that the text contained libel.[23] Perhaps out of pique, perhaps just frustrated at the delays, Stewart and Feder began to circulate throughout the scientific and science policy communities a packet containing the draft manuscript and copies of what was already a voluminous correspondence on the matter. Maddox requested another round of revisions, and the biologists withdrew the article. The second phase began.

Stewart and Feder passionately clung to the hope that their manuscript would be published in a science journal because they believed that discussions of this ethical issue should be confined to scientists. So they next formally submitted the manuscript to *Cell*, beginning an unsuccessful review process that lasted about eight months. Hedging their bets, they also began to contact editors in the science studies community.

The biologists' tireless efforts and the reactions they stimulated seem even more extraordinary in retrospect. Journalist Daniel S. Greenberg, a long-term observer of science and politics, later called the manuscript "a scientific Flying Dutchman, unable to find a place of publication, despite praise from reviewers and expressions of regret from editors."[24]

Stewart and Feder first called me in 1985, in my role as editor of *Science, Technology, & Human Values* (*STHV*). That experience, although mild by comparison to that of those directly caught in the legal controversy, nevertheless provides a firsthand example of how even ordinary journal publishing can contain multiple conflicts of interest.[25] Nothing is straightforward. Where no formal rules govern publishing policies or procedures, ambiguity reigns, for better or worse.

As a formal policy, *STHV* discouraged "double submission" of articles, that is, the practice of submitting an article simultaneously to two or more journals without the editor's knowledge or permission; potential authors were asked to attest, upon submission of an article, that it was not under review or had not been submitted elsewhere. Stewart was convinced, however, that *Cell* would soon reject the manuscript and he implied that, if *Cell* did reject the wandering manuscript, it might next land at *STHV*.

Like most academic journals, *STHV* relied on delicate political coalitions for its survival. In those years, the journal was cosponsored by academic programs at Harvard University and the Massachusetts Institute of Technology (MIT). It was co-owned by the two universities and published by the commercial firm John Wiley & Sons. This coalition created a delicately balanced situation. The universities and the publisher supported the editor's freedom to make all decisions on content; in return, they expected the editor to make prudent choices that would not violate the law, would not jeopardize the reputation of the journal or its sponsors, and would not impose undue financial burdens on the sponsors or the publisher. The *STHV* editorial advisory board, like those at most small journals, was consulted about matters of long-term policy, but not about the daily editorial operations or particular manuscripts (unless a board member was an expert on the topic). This was clearly no ordinary manuscript, however. Even to circulate it to peer reviewers meant attracting controversy and, possibly, legal action. "Wise counsel" was in order. So, I consulted the publisher, the university attorneys, and a few trusted members of the board. Their reactions and recommendations represented typical opinions about these matters—appropriate, honorable, divided.

The publisher, for example, reacted with strong support for moving forward with review and publication and informally encouraged me to do so. Any controversy over this type of ethical issues would have attracted favorable publicity and probably increased circulation, of course; but John Wiley & Sons is also one of the oldest, most respected scientific and engineering publishers in the United States. Its executives apparently bristled at the idea that the threat of a libel suit should ever be allowed to influence expert peer review or to hobble editorial freedom.[26]

According to the contract between the universities and the publisher, the academic sponsors warranted that all content transmitted to the publisher for typesetting was (among other things) free of libel. The universities, not Wiley, therefore, would have had to bear the legal and financial responsibility for responding to a lawsuit. Yet, in this case, the Office of the General Counsel at Harvard, which normally provided legal advice to the journal, was also advising at least two of the Harvard faculty members involved in the Darsee case. A clear conflict of interest prevented involvement of the counsel's office. Had *STHV* actually become embroiled in a legal controversy, lawyers for MIT and Harvard would have had to sort out these obligations and agree on how to protect their joint as well as their individual interests. Fortunately, that complicated process was never tested.

Conversations with the journal's advisory board members brought mixed recommendations. Some urged me to proceed with peer review. Others urged just as strongly that the matter be dropped for fear that "bad publicity" might endanger *STHV*'s survival or my professional career. Even rejecting the manuscript on the basis of inadequate scholarship would have attracted criticism. Almost everyone suggested that I retain an attorney, an expensive prospect for a junior faculty member who was then serving as the journal's editor without compensation.

Thankfully, no journal had to take the legal risks in order to publish the article. On 26 February 1986, Stewart and Feder testified at a hearing before the House Committee on the Judiciary. Its Subcommittee on Civil and Constitutional Rights was then investigating "the 'chilling effect' of libel threats on editors' willingness to publish controversial material."[27] In April, the *New York Times* reported extensively on Stewart and Feder and their findings.[28] And on 14 May 1986, the House Committee on Science and Technology held a hearing on the controversy surrounding the manuscript and the article was published in the *Congressional Record*, thereby effectively eliminating the threat of libel.[29] Here, the link between the professional conduct of scientists, the discussion of ethical issues in journals, and the broader issues of national science policy were joined. The House Committee's staff recognized that publication practices were linked to a changing climate for R&D management, to an atmosphere of ever more frantic competition among young researchers, and to the intellectual integrity of the knowledge base.

Nature published the paper in 1987, accompanied by commentary from

Eugene Braunwald, seemingly ending the saga.[30] One headline declared "Stewart and Feder (Finally) in Print," but, as I shall discuss in chapter 6, this episode was not the last controversy to engage the interest of Walter Stewart and Ned Feder.

Courage is easy to summon in retrospect. I cannot truthfully say what I would have done if I had been forced to decide whether to proceed with formal peer review and to risk damaging a journal I had worked so hard to build. The episode left a sour taste in many mouths. And it threw an embarrassing spotlight on the weaknesses in editorial policies, revealed how difficult the choices could be, and showed how inadequately prepared the journals were to meet them. We were, at the least, unaware of the common ground. The same laws that protect or punish mass-market newspapers and magazines apply to scholarly journals. And the same shifting political environment that influences research funding and practice also affects its communication.

MULTIPLE EXPLANATIONS

Anecdotes can show how roles play out in particular episodes, but understanding the broader issues at stake requires a look at the historical, social, and political context in which the episodes take place. For the political controversy over scientific misconduct, a significant aspect of that context is the persistence of certain explanations for what is happening. Foremost among these is the unwillingness to accept that any scientist, any *real* scientist, could deliberately fabricate or misrepresent data and then describe that data in scientific articles as if it were real. Framed as questions repeatedly posed by many people in many forums, these rationalizations form an intellectual grid on which to impose the history of the political controversy.

How Much Is There?

Like a dog nipping at a runner's heels, this question has nagged the analysis and discussion of research fraud from at least the 1970s. Is there increased incidence of wrongdoing, are the standards for behavior getting higher, or is there just better detection? Is the potential for unethical conduct inherent in science for all time or inherent in how research is conducted today?

Deceitful appropriation of credit and ideas is not a new problem, of course, either for scientists or any other creative professionals. A cursory glance at history tells us that. Social norms and expectations (and, hence, the definitions of what constitute violations of standards) may change over time within individual professions or research fields, but outright fakery and unattributed theft of another's ideas have never been considered appropriate behavior.

The degree of formal condemnation, sanction, and punishment of particular actions *has* varied, however, over time and among professions. Once "authorship" began to carry the potential for profit, then plagiarism became a matter of concern to all authors and publishers; once "antiquities" began to be perceived as objects of value for commerce as well as scholarship, then forgery and purveying of fakes by dealers, archaeologists, and private collectors became activities worth the risk of detection.[31]

Through the years, science, like every other human occupation and profession, has attracted its share of crooks and criminals, as well as its heroes and individuals of integrity.[32] Nevertheless, even though the theft of scientific ideas may appear on the increase, no systematic assessment of the ratio of honest to dishonest people or activities in science has ever been made or indeed could be made because practices of attribution among disciplines, over time, even among contemporaneous institutions, are simply not comparable. In the Renaissance, for example, "authors and sources were cited only in case of refutation."[33] Even Leonardo da Vinci was accused of appropriating ideas and adopting engineering designs from his contemporaries. One likely target of such theft, Francesco di Giorgio Martini, a fifteenth-century Sienese artist and architect, published his inventions only reluctantly because he was convinced they would be stolen by "ignoramuses," as he called them, who sought to "adorn themselves with the labours of others and usurp the glory of an invention that is not theirs."[34]

For many critics of modern science, the fact that deception or misappropriation of ideas may be an age-old problem provides insufficient reason to dismiss current episodes as mere dots in a continuum. To them, today's ethical failings among scientists seem qualitatively different. Instead of blaming science, however, they blame its social context. Science is perceived as "subject to the forces of a given society" and its ethical failures as sad examples of corruption present everywhere—no more but no less reprehensible than insider stock trading or Senatorial shenanigans.[35] News reports frequently adopt this perspective when they refer to research fraud as another "white-collar crime." *Newsweek*, for example, included science in a feature on ethics scandals in the 1980s. Americans, the magazine wrote, have few illusions about their neighbors' morality and little confidence in social institutions. Over half of those surveyed by *Newsweek* believed that people were less honest than they had been ten years before, and fraud in science was cited as one more signal of that decline.[36]

Attention to scientists' standards also blends with emerging public concern about professional ethics issues overall, of the type that stimulates investigations of engineering ethics, medical ethics, and financial conflicts of interest among politicians and stockbrokers. Perhaps Americans are at a point of cultural reassessment, as they adjust their attitudes toward truth-telling and truth-tellers. It may be only natural that revisionism extends to those traditionally regarded as seekers and interpreters of truth. In museums, for example, as curators espouse new approaches to fakes and facsimiles, they attempt to fit them into historical understanding of the art rather than hide them in storerooms. Some museums, wary of trendy exhibition techniques that exploit reproductions and re-creations, now explicitly remind visitors that, whatever the smoke and mirrors employed in an exhibit, the "ultimate aim is to tell the truth."[37]

Another aspect of the measurement question involves controversial efforts to demonstrate how little "bad science" exists, compared to the vast amount of "good science." The editor of *Science*, Daniel E. Koshland, Jr., drew both applause and censure when he admitted that research fraud was "inevitable" and "unacceptable" but argued that it did not deserve much attention because it represented such a minute proportion of all research done in the United States: "We must recognize that 99.9999 percent of reports are accurate and truthful," he wrote.[38] When Koshland was rebuked for that statement, he fired back that he had not meant to imply that "scientists are ethically 99.9999% pure" but, given the millions of "bits of information" in all science journals, to note that the small amount of probable error was negligible.[39]

The question of "how much is there?" represents an inherently unanswerable question as long as it is posed in opposition to something labeled "good science." To the neighborhood that has never experienced a robbery, *one* robbery is too many. The setting is also crucial—like an embarrassing theft that takes place in a building surrounded by guards and expensive security equipment. Now that ethics offices have been established at the National Science Foundation (Office of Inspector General) and the National Institutes of Health (Office of Scientific Integrity and Office of Scientific Integrity Review), and now that these groups are not only investigating but compiling data on cases, there will eventually be comparative information on the *number* of allegations and convictions, if not also on the proportion of misconduct in research overall.[40] In the meantime, the question hangs in the air—unanswered, if not intrinsically unanswerable.

Why Pick on Scientists?

No one likes to be accused of wrongdoing, of course, and scientists are no exception. The leadership of the scientific community does not react cheer-