Essays of an Information Scientist, Vol:10, p.93, 1987 Current Contents, #15, p.3, April 13, 1987

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Current Contents, #49, p.3-5, December 7 1987

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What Do We Know About Fraud and Other Forms of Intellectual Dishonesty in Science? Part 2. Why Does Fraud Happen and What Are Its Effects?

Number 15 April 13, 1987

In part one of this essay on misconduct in science, we discussed various definitions that have been given of scientific fraud and the disparate estimates of its frequency.[1] In this essay we focus on speculations concerning the causes of fraudulent behavior, the implications of fraud for the scientific community, and the actions being discussed—or already taken—to deal with it.

The Roots of Fraud

The causes of fraud are a matter of debate among scientists. In the vacuum created by a lack of rigorous studies, the late Philip Handler, former president of the National Academy of Sciences, expressed the belief that fraud was due to individual aberrations. As reported by Patricia Woolf, Princeton University, New Jersey, Handler said that "one can only judge the rare acts [of fraud] that have come to light as psychopathic behavior originating in minds that made very bad judgments,...minds which...may be considered deranged."[2] This is an appealing argument since, as Daniel E. Koshland, editor, *Science*, points out in an editorial, "there is little percentage in falsifying science.... An oversimplified admonition might be, 'You may escape detection by falsifying an insignificant finding, but there will be no reward. You may falsify an important finding, but then it will surely form the basis for subsequent experiments and become exposed.' "[3]

In 1957 Robert K. Merton, Department of Sociology, Columbia University, New York, gave an interpretation of deviant behavior in science in terms of the race for priority. He observed that "competition in the realm of science, intensified by the great emphasis on original and significant discoveries, may occasionally generate incentives for eclipsing rivals by illicit or dubious means."[4] This idea derived from the theory of anomie, a term referring both to societal instability resulting from a breakdown in values and to individual or personal uncertainty and alienation. Anomie theory was first promulgated by Émile Durkheim (1858-1917)[5] and developed by Merton in 1938 and later.[6,7]

According to Merton's theories, aberrant behavior (such as fraud on the part of scientists) results when accepted avenues of attaining societally prescribed goals are unavailable or perceived to be unavailable. Building on Merton's work, sociologist Jerry Gaston, Texas A&M University, College Station, claims that failure to reach a goal according to the "rules of the game" may cause a scientist "to adopt a different mode of operation to get an edge or advantage over...competitors."[8] And in a reprise of his anomie work, Merton said, "If there is a lesson to be learned from some of the consequences of a belief in the absolute importance of originality in science, it is that absolute beliefs have their dangers too. They can give rise to the kind of zeal in which anything goes."[9] I plan to discuss anomie in the future.

Others offer explanations that blame misconduct more on "the system" than on individuals. For instance, Robert G. Martin, National Institute of Diabetes and Digestive and Kidney Diseases, National Institutes of Health (NIH), Bethesda, Maryland, writes in a letter to the editors of *Science* that journal editorial policies and "the insidious rise in publication costs" may contribute to creating a hospitable environment for fraud.[10] Martin says that some journals accept manuscripts that are admittedly incomplete if the results are considered "scientifically exciting." He also contends that authors faced with the necessity of adding more data and controls (to satisfy reviewers) in less space (to satisfy editors) may "opt for cutting the text and assuring the editor that the requested controls have been performed.... But the data are not shown. The reviewer is then presented with the unenviable task of accepting the revised manuscript or imputing the integrity of the author."[10]

The competitiveness of science is regarded by many as a significant contributor to scientific misconduct. Merton observes that "the culture of science has long put a premium on originality, on being the first to make a scientific discovery. Being second, let alone a subsequent n^{th}, hardly counts at all. Moreover, scientists know that much the same discovery is often made independently by two or more investigators at about the same time.... This often brings about a rush for priority. So it is that the culture of science and its reward system combine with the fact of multiple discoveries to produce intense competition among scientists.... That same premium on originality which has reinforced intrinsic motives for advancing the frontiers of scientific knowledge also contains pathogenic components."[9] Along with many others, Lawrence Altman, medical correspondent, Science News Department, *New York Times*, and Laurie Melcher, research associate, Cornell Medical College, New York, have observed that long lists of publications are often critical in securing for scientists grants, promotions, and tenured positions, so there is an emphasis on getting results and publishing them quickly.[11]

Some think that this atmosphere contributes to the temptation to stoop to fraud. Science journalist Stephen Budiansky quotes Robert Petersdorf, vice-chancellor for health sciences, University of California, San Diego (UCSD), as saying that science has become "too big, too competitive, too entrepreneurial, too bent on 'winning,'... [thus] eroding the moral fiber" of some scientists, who resort to cheating to keep up with what they perceive as the pace of their colleagues' accomplishments.[12]

A case in point is that of William Summerlin, who, in 1973, at the Sloan-Kettering Institute in New York, claimed to have solved the problem of transplant rejection by culturing tissue to be grafted for prolonged periods before actually performing the procedure.[13] His "proof," however, consisted of white mice whose dark-colored skin grafts had resulted from Summerlin's felt-tip pen, rather than from transplants from unrelated black mice, as he had claimed. Summerlin, notes T.J. Hamblin, Royal Victoria Hospital, Bournemouth, UK, "was under enormous pressure at the time, with a heavy clinical load, 25 research projects, and a boss who demanded more publications and bigger breakthroughs."[14] Indeed, "several of the recent cases of fraud," according to science journalist Nicholas Wade, "have occurred in laboratories with a heavy emphasis on paper production.... It is scarcely surprising that in such an environment, younger researchers should be tempted to shade their results, to tidy up the data so as to give the chief what he wants, and eventually to invent data out of whole cloth.... Fraud may well be a sign of the stresses in the contemporary scientific enterprise."[15]

But Eugene Braunwald, professor of the theory and practice of medicine, Harvard Medical School and Brigham and Women's Hospital, Boston, Massachusetts, doesn't accept the idea that competition turns good apples into bad. "In awarding promotions and grants, undue weight is sometimes given to the quantity, as opposed to the quality, of a scientist's contributions," he writes. "This unfortunate practice may lower the quality of science, but I do not believe it is the root cause of fraud.... It is usually not possible to explain the deeper motivation for the commission of serious scientific fraud within the framework of normal behavior."[16]

As suggested by anomie theory, at least some acts of scientific dishonesty seem to be committed in order to acquire or maintain a certain level of recognition or prestige. Merton said that "the pressure to demonstrate the truth of a theory or to produce a sensational discovery has occasionally led to the faking of scientific evidence."[4] Other deceits, however, seem to have been perpetrated out of the sheer conviction that the conclusion is right and the facts must be made to fit it. Charles Dawson's discovery of the human skull and apelike jawbone of a "dawn man" at Piltdown Common near Sussex, UK,[17] in 1912 is one such example. Piltdown Man was exposed as a hoax in the 1950s, when modern archaeological techniques showed conclusively that the "fossil" had been stained and its molars crudely filed to give the appearance of age. But for a time, England had a claim to being the cradle of civilization that matched those of France and Germany, where Paleolithic cave art and ancient human remains had been discovered at the turn of the century.[18] (p. 119-22) Another, more recent example of a fraud committed to preserve an idea is that of psychologist Sir Cyril Burt (1883-1971), who fabricated research on the IQs of schoolchildren to support his theories of the heritability of intelligence.[19]

The Consequences of Fraud

It seems likely to many people that each instance of fraud that comes to light is, at the very least, damaging to the public credibility of science. For decades, Altman and Melcher claim, "scientists have insisted that science was honest and virtually fraud-proof."[11] The basis for this belief, according to Woolf, has been the twin safeguards of refereeing and reproducibility.[2] "Now," Altman and Melcher continue, "those past denials have created the impression that fraud is a major new problem, even if the percentage of cases is really no larger than the true hidden proportion in the past."[11]

Sociologist Deena Weinstein, DePaul University, Chicago, Illinois, also feels that fraud may shake society's faith in science, and she says one result might be that funding agencies will be less forthcoming in the future. But she also thinks that fraud has profound effects on the scientific community: "If scientists must be concerned about the validity of the conclusions upon which their own research rests, they may need to replicate prior work (a great inefficiency) or... live with an endemic doubt and anxiety."[20] Moreover, she warns, "knowledge that others have committed fraud and that some have 'gotten away with it' may lead to...the feeling that one does not know what...is right."[20]

Yet science does progress, in spite of error and bias and misconduct and inefficiency. Thus it would seem that, even if some unknown number of papers are unreliable, science as a whole continues to function. In an incident such as the one involving John R. Darsee,[21] who fabricated data at Harvard, at Emory University, Atlanta, Georgia, and as an undergraduate, individuals may have been hurt, and time, money, and equipment may have been wasted. Yet the very fact that Darsee's duplicity was discovered is another instance of science as self-correcting. As Benjamin Lewin, editor, *Cell*, notes in a comment similar to Koshland's, "if the [fraudulent] data are important, another laboratory will in any case repeat them, and any discrepancies will be evident."[22]

But whatever the actual incidence and impact of serious fraud in contemporary science, it seems fair to ask why the known cases progressed so far: why did the safeguards in which Handler placed such faith fail? Braunwald notes that although "scientific fraud is a crime and must be investigated like any other crime...scientists are not trained to be criminal investigators and few are good at it."[16] And in my recent essays on refereeing and the peer-review process,[23-26] I noted how rare it is for referees of scientific articles to notify editors that a given paper arouses their suspicion, and I listed some of the conjectured reasons for this.[23,24] Scientists may well be aware of problems with a colleague's work, but, for a variety of reasons, they seldom voice their doubts in print or in public. Harriet Zuckerman,[27] Department of Sociology, Columbia University, gave a classic example of this in connection with the Summerlin affair, quoting the remarks of Nobelist Peter B. Medawar:

> I found myself lacking in moral courage. Summerlin once demonstrated to our assembled board a rabbit which he said had received from a human being...a corneal graft.... Through a perfectly transparent eye this rabbit looked at the board with a candid and unwavering gaze of which only a rabbit with an absolutely clear conscience is capable. I could not believe that this rabbit had received a graft of any kind...because the pattern of blood vessels in the ring around the cornea was in no way disturbed. Nevertheless, I simply lacked the moral courage to say at the time that I thought we were the victims of a hoax or confidence trick.[28]

Part of the reason for this reticence may lie in a remark reported in *Chemical & Engineering News* by writer Jeffrey L. Fox and attributed to a Cornell scientist who wished to remain anonymous. The source told Fox that scientists "can't live...without attributing honesty to coworkers. You can be skeptical, but you can't [look] at everything as being made up."[29] Lewin voices a similar opinion: "The presumption of innocence holds powerful sway in the world of science. When a paper is reviewed, the assumption is implicit that the work is described accurately, that the authors actually did what they say they did, and that the data presented are representative. What other way exists to consider a manuscript without tearing a hole of suspicion in the fabric of science? Yet this leaves us immensely vulnerable to any sort of deceit...."[22] Science journalist William J. Broad and Wade claim that "simple human factors that often shape scientists' attitudes and motivations [may push] out of mind the basic methodological safeguard of replication.... Pride, ambition, excitement at a new theory, reluctance to listen to bad news, unwillingness to distrust a colleague"[30] are all factors that contribute to allowing fraud to seemingly slip through science's safety net.

In fact, they report that several scientists tried and failed to replicate the work of Cornell graduate student Mark Spector, who promulgated a now-suspect theory explaining the origins of cancer;[30,31] however, none "took their failures seriously enough to make them public."[30] And Bernard Dixon, European editor of *The Scientist*™, writes of similar behind-the-scenes skepticism in connection with Michael Briggs, Deakin University, Geelong, Australia, who fabricated data on the relationship between cardiovascular disease and the use of contraceptive pills by women. Experts had been questioning his work for years—but "in the conference corridors,...not in public or in print," according to Dixon.[32]

Another, more recent factor for this silence and for the lack of research on fraud may be scientists' fear of lawsuits. In at least one instance, the study by Walter W. Stewart and Ned Feder, NIH, of the research practices of the 47 coauthors of Darsee,[21] the threat of lawsuits was sufficient to significantly delay the publication of the original manuscript. According to an article by Stewart and Feder in the *Boston Sunday Globe*, their paper was praised by reviewers and editors alike, yet went unpublished for three years amid formal threats of libel suits.[33] The study was finally published in *Nature* early this year after extensive revision.[21] The issue also contains a rebuttal of the Stewart-Feder charges by Braunwald, who was one of Darsee's coauthors and the head of the Cardiac Research Laboratory, Harvard, at the time Darsee's frauds were discovered.[16] An anonymous editorial accompanying the article and Braunwald's commentary explains the editors' interest in the Stewart-Feder study and their role in putting it into print. It cautions that, although the study is "not itself above reproach," it nevertheless raises "important questions about the reliability of the scientific literature and the role of the process of publication in the practice of science."[34]

But this still leaves the matter of the reproducibility of results. Supposedly, if inferior or even fraudulent work makes it past the refereeing process, scientists who read it will immediately spot any errors when they try to reproduce the paper's results. However, in a letter to the editors of *Science*, Arthur H. Neufeld, Eye Research Institute of Retina Foundation, Boston, Massachusetts, charges that "reproducing the experiments of other investigators is no longer of primary concern" to today's busy, competitive scientists.[35]

Neufeld claims that this is a big change from the practice of "several decades ago," when he says reproducing results was common and the "methods section of a journal article was important." Now, though, the "exponential growth in new and interesting paths to follow has outpaced the growth in the number of scientists and in the funding for research. Who has the time, interest, money, or need to reproduce another scientist's results?"[35] Neufeld argues that "the implications of not reproducing results are severe. Much of what is published goes unchallenged, may be untrue, and probably nobody knows.... The foundation on which we based our research was other scientists' methods and results. Now the foundation is trust."[35] While this may be a valid observation, considering the progress made in some fields, we have every reason to believe that our trust has not been misplaced.

Fraud may adversely affect the work of individual scientists, who may have built a body of literature on the basis of fraudulent work. In the case of Spector, for instance, senior scientists and coauthors at Cornell are working intensively to sift the wheat from the chaff and establish what can be confirmed of Spector's elegant theory.[29] And in connection with the Darsee affair, Bruce B. Dan, senior editor, *JAMA—The Journal of the American Medical Association*, reports that the list of papers citing Darsee's work takes up "more than 29 feet of computer printout from the *SciSearch*® database." Dan states that "the ultimate validity of these 241 papers...is now open to question."[36] However, neither he nor anyone else that we know of has attempted to determine the validity of this assertion.

In fact, we found evidence to suggest that Dan's statement is much too strongly worded. We read 19 of the 20 papers that cited a 1981 work in the *New England Journal of Medicine* (*NEJM*) by Darsee and Steven B. Heymsfield, both at Emory University at the time.[37] An Emory University investigatory committee found "no evidence that any of the work described in this paper is valid" and that, in fact, "the entire paper may be fictitious."[38] The authors of 15 of the 19 papers cited the *NEJM* work as background or to put their own findings in perspective. The authors of the other four papers described the *NEJM* work as unsupported, said it was in contrast with previous work in the field, or stated they could not confirm it. This analysis provides little direct evidence of harm or damage done to these investigators. Unless fraudulent data from previous papers are incorporated as the basis for subsequent work, it is difficult indeed to assess whether any damage has been done to the work of honest investigators.

Forged research may have tangible effects beyond the scientific community. As the Briggs affair shows, fraudulent papers may have serious health implications for society at large. A small sampling of articles[39-42] suggests that the public may be exposed to products that have been "inadequately tested," according to an anonymous, former US Environmental Protection Agency official quoted in the *Los Angeles Times*.[40] And as suggested by many, including Daryl E. Chubin, former director, Technology and Science Policy Program, School of Social Sciences, Georgia Institute of Technology, Atlanta, and now at the Office of Technology Assessment, Washington, DC, "the misconduct of a few scientists threatens to discredit the profession in the eyes of the public, if the public believes that misconduct in research has been discovered and has then been ignored."[43]

The public perception of science and scientific misconduct through the eyes of the media has been a matter of some concern to scientists, who fear that the publicity recent cases of fraud have attracted give science an unnecessary "black eye." There seems to be a general feeling that the press is blowing the subject of misconduct in science out of proportion. Yet such opinions don't take into account the nature and function of the media. As Merton has observed:

> [Society has] the strong moral expectation that scientists in pursuit of reliable knowledge will live up to the highest standards of probity. When those expectations are seen to have been violated in a few cases, these statistically rare reports attract great public notice and become especially newsworthy. We all still remember the observation, made half-a-century ago by John Bogart, then the city editor of *The* [New York] *Sun*: 'When a dog bites a man, that's not news. But if a man bites a dog, *that's* news!' And Bogart might have added: even more startling and therefore more likely to make headlines would be the episode where not only does man bite dog, but dog doesn't even bite back, as would be the case were the scientific community to take the fabrication of data as a mere quirk or misdemeanor rather than virtually a capital crime.[44]

Moreover, it may be that the way journalists report on fraud "further idealizes science as a pure, dispassionate profession," according to Dorothy Nelkin, Cornell University, Ithaca, New York.[45] She suggests that journalists either describe individual acts of fraud and the investigations that revealed them or discuss the causes and extent of fraud. The first style treats fraud as a "scandal" or a "sin against science," while the second paints fraud as a problem endemic to today's highly competitive scientific enterprise; yet both styles, Nelkin says, "project a coherent image of scientific ideals.... Fraudulent acts in other fields... [are] reported, often cynically,...as one more example of corruption."[45]

Dealing with Fraud

Even if Nelkin is correct and the damage to science's credibility is less than scientists fear, it seems reasonable to assume that this may change if there is a real or perceived increase in the instances of scientific fraud over the next few years. Scientists may then face a choice between developing procedures for detecting or preventing fraud or accepting an increased level of institutional or government involvement in their work.[46,47] Many scholarly organizations and institutions have issued guidelines for the ethical conduct and reporting of research.[48-53] The NIH recently issued such guidelines concerning work funded by grants and awards made by agencies in the US Public Health Service;[54] and according to William F. Raub, deputy director, NIH, the NIH now requires that "institutions [that] get grants and contracts must have a misconduct policy in place."[55]

UCSD, one of the institutions victimized by the case involving Robert Slutsky, has instituted a tough-minded approach to the problem of finding out exactly what happened. In the wake of the discovery that Slutsky, a junior scientist at the UCSD medical school, had published false data, an *ad hoc* investigative committee found at least 68 papers of "questionable validity"—and has put "all coauthors on notice that they would have to defend their papers."[56] And the NIH, which funded both Slutsky and Darsee, barred Darsee from receiving federal funds for 10 years and required Brigham and Women's Hospital to repay the $122,371 they received in contract funds from the NIH.[57]

Most of the authors who committed the highly publicized acts of fraud over the last few years are no longer practicing researchers. But according to Lewin, many cases of professional misconduct are handled privately and are kept between the perpetrators and their immediate superiors.[22] What happens to these authors, who have admitted to committing fraud or have been caught doing so, and who continue to publish? "To refuse to consider further work from [such scientists] is to establish a kangaroo court on the basis of the word of one senior scientist. Yet to consider [their] papers like any others is perhaps naive."[22]

Whether or not new instances of fraud surface, scientists must always examine the work and data of colleagues with a healthy skepticism. Clearly, the government and the public have a right to expect that scientists will continue to cultivate an ethical atmosphere that will encourage young scientists to uphold the tradition of trust that has worked so well. However, a heightened awareness on the part of scientists that scientific fraud does indeed occur and a willingness to voice reasonable suspicions during the refereeing process will help reduce the number of instances.

But to me, the more important issue is a perverse and pervasive anti-science attitude among some members of the press on matters scientific, not just on fraud in science. Among other notions they promulgate is the idea that scientists are the new generation of fat cats. In relation to fraud, isn't it only "natural," they ask, to expect scientists, like so many other groups in society, to be dishonest? In my opinion, it is not a coincidence that scientists, many of whom may have been attracted to their profession by a desire to help humanity, should also be less inclined to be criminals—and to be less susceptible to fraud in its various forms.

As we have seen, fraud is variously estimated to be anything from a minor phenomenon hardly worthy of discussion to a major threat to the integrity and conduct of science; it has been blamed on everything from the failings of individuals to the failings of modern science (and, indeed, of modern society) itself. But ironically, what is missing from all these speculations on misconduct in science is *science*—that is, a sociological inquiry into the exact causes and incidence of fraud. I believe that such an inquiry would demonstrate that scientists have a great deal to be proud of, in spite of occasional instances of aberrant behavior. Nevertheless, caution should be exercised; as Stewart and Feder warn near the end of their paper:

> Examination of scientific practices could cause unwarranted harm to individual scientists. Systematic examination of scientific practices might even weaken the fabric of trust that is essential to the functioning of science. Of all human endeavors, science is one of the most successful—prodigious in benefits, low in cost. But science, vulnerable to abuse from within by its practitioners, is perhaps even more vulnerable to harm by regulation, and at some point, the cost of further regulation will outweigh the benefits. Scientists have, to an unusual degree, been entrusted with the regulation of their own professional activities. Self-regulation is a privilege that must be exercised vigorously and wisely, or it may be lost.[21]

* * * * *

My thanks to Stephen A. Bonaduce and C.J. Fiscus for their help in the preparation of this essay.

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Essays of an Information Scientist, Vol:10, p.370, 1987 Current Contents, #49, p.3, December 7, 1987

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Some Deviant Behavior in Science Has Nothing At All to Do with Fraud

Number 49 December 7, 1987

In my recent two-part essay on fraud and intellectual dishonesty in science,[1,2] I tried to demonstrate that the amount of outright fraud in the scientific community is, by comparison with other professions, minuscule. I tried to remind readers that the borderline between outright fraud, disreputable error, or other kinds of unwitting errors is often thin. That is why I used the neutral phrase "deviant behavior" in the title of Part 1. In the broad spectrum of the latter, there are many kinds of misconduct and behavior, including those that bypass accepted norms. For example, pork barreling by academics[3] may not be illegal, but many scholars regard it as unethical. It is certainly not traditional since it attempts to bypass peer review.

A kind of behavior that some might describe alternatively as charisma or chutzpah is that which has certain scientists seeking publicity in ways perceived to violate the norm. In some national science cultures, as in the UK, it is considered gauche even to talk about one's accomplishments to the public. An eminent British scientist once wrote me that it is his policy never to comment on his own work. This was in response to an invitation to write a commentary on one of his many classic papers.

On the other hand there are the "visible scientists"—like Carl Sagan, among others[4]—who gain a certain kind of publicity by being continuously public figures. The publication of James D. Watson's *Double Helix*[5] aroused discomfort among many in the scientific community.[6]

In my attempt to illustrate one of the many types of "deviant behavior," in the sociological sense, I referred first to a case of alleged disreputable, or careless, error. The point I was trying to make was that "divergent classifications of the misbehavior of scientists contribute to difficulty in arriving at a consensus definition of fraud in individual cases. Works that contain some irregularities but have not actually been fabricated can cause heated debate, with some scholars arguing that fraud has been committed, while others argue against such a conclusion."[1] (p. 4) The documentation for the discussion of this case of alleged disreputable error was provided in my essay and does not warrant repetition. Nevertheless, a few readers felt that the researchers involved had been badly treated in my report. They are free to publish their concerns. But I believe that we made a balanced and fair journalistic report on the debate concerning methodological irregularities, even though such irregularities were unintentional.

In extending my review of the spectrum of deviant behavior, I also referred to the work and style of Stanley Prusiner, University of California, San Francisco (UCSF). Having read an extensive article by G. Taubes in *Discover*[7] magazine that included numerous photographs of Dr. Prusiner (presumably taken for the accompanying article), I had the impression of a man who had mastered the art of public relations.

However, I received a letter from Professor T.O. Diener, Microbiology and Plant Pathology Laboratory, US Department of Agriculture, Beltsville, Maryland, in which he described the *Discover* article as "an attempt at character assassination of Dr. Prusiner." Diener wrote that "even the most superficial inquiry into the facts would have disclosed that the *Discover* article was far from objective and could best be described as pseudoscientific soap opera." Diener then discusses "only one of the many factual misrepresentations contained in the article." Diener concludes with the request that I publish, "in the name of intellectual honesty, a correction of the slur you perpetrated against a most productive and imaginative scientist."[8]

I may have inadvertently associated Prusiner's somewhat unconventional style and conduct with fraud. That, of course, was never said or intended. I am certainly unqualified to judge the validity of Prusiner's prion theory. Although it is still controversial, it may be one of those scientific controversies that inexorably leads to greater knowledge. That Prusiner's lab was awarded a $4 million Jacob Javits Center of Excellence in Neuroscience research grant in 1985 confirms the belief of qualified experts that his scientific ideas have great potential. And the citation record supports the impression that his papers have had considerable impact. But the merits of Prusiner's research have nothing to do with the issue of the methods used to obtain publicity for his lab.

However, Diener and another correspondent, Ivan Diamond, School of Medicine, UCSF, and director, Ernest Gallo Clinic and Research Center, San Francisco, feel that I have tainted Prusiner with guilt by association—that the mere mention of his name in an article about fraud and other forms of misbehavior in science was inappropriate.[9] It is unfortunate that the timing was such that I was unaware of the letter published in the February 1987 issue of *Discover* by Charles Weissmann, Institute of Molecular Biology, University of Zurich, Switzerland, who also interprets the Taubes article as an attempt to denigrate Prusiner. Weissmann expresses his conviction of Prusiner's intellectual honesty. He notes, however, that Prusiner "has an extraordinary and colorful personality" and that his "enthusiasm" has "also led him to espouse views prematurely." While agreeing that Dr. Prusiner's coining the term prion "unleashed much ill feeling in the scrapie community," Weissmann asks "on whom does that reflect badly—Stan or his critics?"[10]

As readers of *Current Contents*® throughout the world realize, we have always been meticulous in documenting our sources of information. We do this to protect cited individuals from inadvertent misrepresentation or, more often, to avoid scientific errors. I have always avoided personal attacks and will of course never tolerate yellow journalism in the pages of *Current Contents* or *THE SCIENTIST*™. If the juxtaposition of my comments about Prusiner in an essay covering a variety of deviant behaviors has caused him or anyone else undeserved public scorn, then I regret the failure to adequately clarify the intent of the discussion (see the selected Bibliography at the end of this essay for works discussing norms, mores, and ethics in science).

Upon rereading my comments about Prusiner, I found that I had not explicitly cited either those investigators mentioned in the *Discover* article who, at one time or another, were reported to have made critical remarks about Prusiner or his work, or the thrust of their criticisms. Those who simply disagree with his scientific conclusions need no mention here, but those who question his approach to public relations include Paul E. Bendheim, formerly a postdoc with Prusiner at UCSF, now at the Institute for Basic Research in Developmental Disabilities (IBR), Staten Island, New York, and Dave C. Bolton, another former Prusiner colleague, also now at IBR.[7] In addition, George G. Glenner, a research professor of pathology, University of California, San Diego, and a Prusiner coauthor,[11] strenuously disagrees with the conclusions expressed to the press by Prusiner.[12]

If he wasn't a public figure before receiving his $4 million grant, Dr. Prusiner is now. While it may be my prerogative as a journalist to criticize his PR style, it is essential to reiterate that he was never accused of fraud. I encountered the article in *Discover* just as my own article was in its last revision and inadvertently failed to send Dr. Prusiner a copy of my remarks. He has been sent a copy of these remarks, however, as well as my sincerest regrets for the confusion.

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