

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

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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 academics³ 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 others⁴—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 de-

scribed 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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