The Persistence of Fraud in the Literature: The Darsee Case

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The incidence of fraud, or scientific misconduct, has been much publicized of late. While not new, the case of John Darsee has played an important part in the literature of biomedical research for a number of years. Darsee was discovered to have fabricated the data which formed the bases for many articles and abstracts he published through 1981. The present study shows that, although a considerable amount of time has passed and some of his papers have been retracted, Darsee's work continues to be cited, and cited positively, in the literature on cardiology through 1990. The implications of this phenomenon are discussed. © 1992 John Wiley & Sons, Inc.

Introduction

"Fraud" may be defined as the fabrication of data and findings, plagiarism, the deliberate distortion of actual data, or other acts which fall under the rubric of scientific misconduct, as applied by such bodies as the National Science Foundation and the National Institutes of Health. The behavior under scrutiny here is of the first type—fabrication. Fraud in science has been the focus of a great deal of attention in recent years. In 1987, a 450-item bibliography was compiled under the auspices of the National Library of Medicine (Van de Kamp & Cummings, 1987). Five conferences on the subject were scheduled from February through April 1991 (Hamilton, 1991). It is difficult, even with the publicity and scrutiny fraud has received, to determine the extensiveness of misconduct in research.

The reports of fraud are frequently prominently featured; witness the recent case of David Baltimore and Thereza Imanishi-Kari (Wheeler, 1991). Part of the notoriety of this case centers on the fact that Baltimore is a Nobel laureate; part on Baltimore's insistence that no misconduct existed. His own integrity was never in question, but allegations directed at his coauthor led to retraction of the paper. The problem has been around and has attracted attention since the mid-1970s. Altman and Melcher report that, in 1981, "Over a third of the members of the audience at a session [at a meeting of the Council of Biology Editors] on the subject raised their hands in answer to a question of whether they knew of a recent unpublished example of fraud" (Altman & Melcher, 1983, p. 2004). Two books have been written in the last decade with the express intention of alerting both scientists and nonscientists to incidents of fraud in scientific research (Broad & Wade, 1982; Kohn, 1986).

The seriousness of the matter has prompted government investigation in some instances. Some of the cases reported by Broad and Wade, and by Kohn, resulted in hearings in 1981 on fraud by the U.S. House Committee on Science and Technology (U.S. House of Representatives, 1981). More recently, further extensive attention has been paid to the problem by the National Institute of Mental Health (1987). Several writers have attributed at least some of the problem to pressures to publish and to attract outside funding and to stiff competition faced by young researchers in the academic and professional worlds (Petersdorf, 1986; Weinstein, 1979; Woolf, 1986). This is not the sole cause of misconduct, by any means. As with any human endeavor, reasons for behavior are multifarious, but the pressures evident in the research environment have been the focus of attention by noted analysts of scientific misconduct. Goodstein offers an assessment of the reasons for fraud:

> My own judgment is that scientists are most vulnerable to the temptation to fake data when (1) they are under career pressure to produce something; (2) they think

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Received September 11, 1991; revised February 10, 1992; accepted March 23, 1992.

> they know what the answer is and feel that going to the trouble of taking the data is superfluous; and (3) they think they are somewhat protected because experiments are not expected to be precisely reproducible. This last point applies more to the biomedical sciences than to the physical sciences. (Goodstein, 1991, p. 513)

Whatever the cause or motivation, some fraud and misconduct is occasionally uncovered or admitted to. Between 1980 and 1982, the National Institutes of Health reported 45 cases of apparent misconduct (Chubin, 1985, p. 178). More recently, the National Institutes of Health have established the Office of Scientific Integrity. From March 1989, the date of the Office's founding, through June 1991, 174 allegations of misconduct were investigated. In 86 cases, no misconduct was found; 56 were still underway in 1991; and, in 19 instances, misconduct was apparent (Wheeler, 1991). Broad and Wade estimate that each detected case of major fraud is accompanied by 100 undetected cases and, furthermore, by 1000 cases of minor fraud. This is an impressionistic assessment, however; no firm evidence supports their figures. Furthermore, not all are as quick to state that fraud is rampant in the research environment. Szilagyi (1984) and Braunwald (1987), for instance, assert that the act of fraud is heinous, but believe that it is not a fundamental and inherent part of science. Braunwald's statement was offered in response to a paper by Stewart and Feder (see below) analyzing articles by scientists who coauthored works with Darsee. A related problem is that the extent of fraud is not as ominous as its potential for disruption. Woolf (1981) notes:

> The damage caused by falsification is not related merely to its frequency. Even if cases of fraud are infrequent, fraud has an impact on the research of other working scientists, on the reliability of the published literature, and on public attitudes that are vital to the future support of research. Greater understanding of these effects would allow the scientific community to take steps to minimize the adverse consequences within science and to reassure the public that its trust is warranted. (pp. 12–13)

Many of the cases of admitted or discovered fraud and misconduct were slow in being resolved. One possible explanation for the difficulty of discovery is the lack of replication of studies. It may be that actual or perceived rewards and funding do not work in favor of replication. The advancement of work in science does sometimes lead to the rooting out of invalid work through the process of building, or attempting to build, upon previous work. It may also be that an over-reaching assumption of honesty governs actions. Zurer (1987) writes:

> One contributing factor may be that chemists [and other research scientists] very seldom talk explicitly about what standards are expected in the research lab. Students don't attend seminars or hear lectures on the necessity to watch out for self-deception or to avoid the temptation to "improve" their results. Scientists just assume the proper standards are self evident. (p. 14)

A third factor is that the weight of authority carries substantial import, even if there is evidence of fraud, misconduct, or nonmalicious error. As Weinstein (1979) observes:

> Although the norm of universalism is violated when evidence is accepted on the basis of authority, when research is backed by the name of an eminent investigator, originates from a prestigious institution, is well funded by knowledgeable granting agencies or finds its way into print in a respected refereed journal, scientists generally accept it at face value. (p. 644)

The above section places the problem of fraud and misconduct in a context of scientific investigation as it has been conducted for nearly two decades. The review of the literature is not intended to be exhaustive, but presents some opinions on causes and pervasiveness of fraud and misconduct. The writings cited indicate a sensitivity to the issue which has been articulated in the popular and scholarly literatures since the 1970s. It also provides an indication that the case of John Darsee is not an isolated incident. While Darsee's case is a notorious one, and one about which many details are public, it exists in an era of concern about the integrity of scientific research. The present study examines the subsequent trail of Darsee's work throughout the scientific literature and the acceptance or rejection meeting his work.

Background on the Darsee Case

John Darsee earned his undergraduate degree from Notre Dame University and received his medical degree from Indiana University in 1974 (Kohn, 1986). He continued at Emory doing clinical work and research until 1979, when he moved to Boston. He worked in the laboratory of Eugene Braunwald, an noted physician and researcher and head of medicine at both the Brigham and Women's and Beth Israel Hospitals (Altman & Melcher, 1983). In May of 1981, some colleagues made known their suspicions regarding some work Darsee had been doing (Anonymous, 1982). Braunwald immediately launched an investigation, in which further irregularities in the suspected work came to light.

Darsee had been offered a position as Assistant Professor of Medicine at the Brigham and Women's Hospital, Braunwald withdrew the offer and the NIH fellowship Darsee held, and Darsee's appointments with Brigham and Women's Hospital were terminated. Darsee continued to do research in the laboratory, however, though under close supervision. He also submitted further papers and abstracts for publication, some coauthored with Braunwald and others in the lab. The assumption of Darsee's colleagues was apparently that the one piece of suspect work was an isolated aberration.

Darsee was involved, at the time of the termination of his Harvard and Brigham and Women's appointments, in a multi-institutional project funded by the National Institutes of Health. Data gathered by Darsee prior to May 21, 1981 was substantially at variance from

that gathered at Duke and Johns Hopkins Universities and the VA Medical Center. He was unable to account for the variances, so a committee of eight faculty members from Harvard and other institutions was established by the Dean of the Harvard Medical School. At approximately the same time, NIH convened a committee to investigate the incident. The Harvard committee found substantial evidence that data had been fabricated in the course of the study. The NIH group delved further into Darsee's other publications and discovered systematic falsification of data in five other studies. As a result of the committee's work, several of Darsee's publication were retracted and he was barred from receiving NIH funds for a period of ten years. (Fig. 1 presents a chronology highlighting Darsee's career and activities.)

The NIH committee, in its report, stated that:

> The panel is of the opinion that the circumstances prevailing throughout Dr. Darsee's period in the laboratory, while not responsible for or in any way condoning his misdeeds, helped to create an environment that may have inhibited their being uncovered . . . there just wasn't enough direct contact with Braunwald. (Culliton, 1983, p. 936)

Braunwald maintained that he kept in close touch with operations of his laboratory. In an effort to excuse himself, Darsee wrote to Braunwald:

> This was an extremely difficult period for me. I had too much to do, too little time to do it in, and was greatly fatigued mentally and almost childlike emotionally. I had not taken a vacation, sick day, or even a day off from work for six years. I had put myself on a track that I hoped would allow me to have a wonderful academic job and I knew I had to work very hard for it. (Knox, 1983, p. 1806)

Woolf (1986) observes two phenomena associated with fraud, both of which are evident in the Darsee case. Some extensively reported frauds have occurred in laboratories where publications per year clearly exceed the norm and pressure is frequently mentioned as a contributing factor. Both of these observations, as well as the facts of the Darsee case, point to the reality of academic research: institutional funding is perceived as inadequate for the conduct of extensive research, and rewards are based largely on published output and the attraction of external funding.

Subsequent to the Harvard and NIH investigation, Darsee's career received further scrutiny from a committee at Emory (Moran, 1985). This panel examined ten papers and 45 abstracts by Darsee and coauthors. It concluded that one paper was valid, one was reported to be valid by the coauthors, and eight were invalid or fraudulent. Only five of the abstracts were seen to be valid, while the remaining forty were questionable at least. The committee stated in its conclusion:

> The results of the evaluation of Darsee's work by this committee provide overwhelming direct and circumstantial evidence of flagrant and extensive fraud in his research at Emory University and of fabrication of data published in the name of the university even after he was at Harvard. (Moran, 1985, p. 303)

1974	Earned M. D. degree from Indiana University School of Medicine
1974-1977	Intern, junior resident, then senior resident in medicine at the Emory University Hospitals
1977-1979	Clinical fellow in cardiology at the Emory University Hospitals
1978-1979	Also served as chief resident in medicine, Grady Memorial Hospital and instructor in medicine, Emory University School of Medicine
1979	Went to Harvard Medical School as research fellow in cardiology
1980	Appointed instructor in the department of physiology and research associate in medicine at Brigham and Women's Hospital
July 1981	To be effective date of appointment as assistant professor in medicine at Harvard Medical School (offer withdrawn and positions as fellow at Harvard Medical School and research associate at Brigham and Women's Hospital terminated before this date)
Nov. 1981	Dr. Eugene Braunwald told Drs. J. Willis Hurst and Robert C. Schlant of Emory University that Darsee had been accused of fabricating data while at Harvard
3 Dec. 1981	Braunwald urged Hurst and Schlant to conduct an audit of Darsee's work at Emory
17 Feb. 1982	Dean James F. Glenn of the Emory University School of Medicine appointed an ad hoc committee to review Darsee's work while he was at Emory
29 June 1982	Committee's report completed
Feb. 1983	Committee's report released, with a memorandum from the associate director of NIH for extramural research and training to the director of NIH (which recommended that Darsee not receive financial assistance and contract service from NIH for ten years, that Darsee not serve on any peer review advisory committee for ten years, and that NIH investigate Darsee's work at Emory)

FIG. 1. Chronology of events in Darsee's career.

Citations to Darsee

Most of Darsee's publications are dated through 1981. It may be expected that these publications, appearing as they do in some prominent journals, will be cited in subsequent literature shortly after the publication of the original paper. A question asked here relates to the number of citations received after 1981 by all of Darsee's articles and abstracts. For that reason, the time period studied is 1982–1990. These data can be gathered searching *Science Citation Index*. This investigation revealed that Darsee's papers were cited a total of 328 times during the time period in question. Some of those citations are in non-English-language publications; this examination focuses on only those in English-language journals. This means that 298 citations are analyzed.

The 298 citing papers were examined in order to determine the contextual purpose for citing Darsee's articles. Each paper was read and the citations were placed in one of three categories. Some of the papers dealt directly with the subject of fraud or the retraction of one or more papers written or cowritten by Darsee. This category contained the fewest citations. The second category is that of negative citations. Negative citation was inferred when the citing paper overtly or implicitly acknowledged problems with methodology or findings, but without explicit mention of fraud. The remaining category, positive citations, is the largest. A positive ci-

tation is defined as any citation that overtly or implicitly accepts the methodology and/or findings of Darsee's work. For instance, an implied positive citation would be a citing author's mentioning without comment a procedural method or a finding noted by Darsee in his paper. From such a citation, acceptance is inferred for the purposes of this study. In other words, the absence of negative comment is interpreted as a positive citation. Any indication of approval, such as the use of reinforcing language ("good," "well-conceived," etc.) is taken to be an overtly positive citation.

The task of examining the citing works was shared by the authors of this article, with the principal author taking on the bulk of the work. However, 29, or approximately 10% of the articles, were examined by both researchers to determine if there is agreement in the application of the categories. In only one instance was there any degree of disagreement. This resulted from a paper which was critical of Darsee's methodology, but which accepted the findings nonetheless. This is the only case of a paper incorporating both positive and negative evaluation of Darsee's work. The level of agreement in the independent assessments (96.6%) implies substantial confidence in the study's findings.

Table 1 presents a breakdown of the citations by category for the years 1982–1990. As can be seen from the table, the vast majority of citations to Darsee's work is positive in nature. Positive citations comprise 85.9% of the total number of citations. Negative citations are 8.4% and citations acknowledging fraud are 5.7% of the total. In 1990, all 14 citations were determined to be positive. Three of the papers cited in 1990 have been shown to be fraudulent or questionable by the committee convened at Emory University in 1982. Analysis shows that 136 of the positive citations are to works deemed fraudulent or questionable by the committee. (One of those citing papers recognizes that Darsee's paper included some shoddy methodology, but agreed with the findings, so it was included in the positive category.)

Some interpretation of these data is conjectural. For instance, it may be assumed that total numbers of citations by year dropped off because many knowledgeable scientists simply stopped citing Darsee at all. These researchers may have even made conscious decisions to keep Darsee's name out of the literature by not citing his work. Darsee's papers, however, have remained alive in the literature, but explanation for this phenomenon is elusive. Darsee was cited in the journal *Circulation* as late as 1986, despite the fact that two of his papers had appeared and were retracted in that journal. On the other hand, the *New England Journal of Medicine* includes no citations to his work after 1983 (the year in which the only two citations were a retraction and an editorial on fraud). Through 1990, though, Darsee is still cited, and cited positively, in the literature of cardiology.

TABLE 1. Number and category of citations to Darsee's work by year.

Year	Positive	Negative	Fraud
1982	71	14	2
1983	47	4	12
1984	38	1	2
1985	26	3	0
1986	18	1	0
1987	14	1	0
1988	14	1	0
1989	14	0	1
1990	14	0	0
Totals	256	25	17

Garfield and Welljams-Dorof (1990) found something of a similar, but far less dramatic, pattern of continuing citation in the case of Stephen Breuning. Breuning's works continued to be cited, though in quite small quantities, after he was convicted of scientific fraud in 1988 for fabricating data included in studies published primarily during the 1980–1984 period. Garfield and Welljams-Dorof's (1990) paper noted a larger number of negative citations that is indicated in this study, and they concluded that Breuning's influence on the literature was not great. They wrote:

> The most interesting observation to emerge from this single case study is that the scientific literature seems to purge itself of articles that are known or even suspected to be fraudulent. The annual distribution of non-self-citations indicates that authors shun falsified research once it is publicly exposed. (p. 1426)

It is obvious that Garfield and Welljams-Dorof's findings cannot be extrapolated to all cases of fraud. Pfeifer and Snodgrass (1990) looked at citations to retracted papers. They found that these papers continued to be cited, even after retraction. They also discovered that while citation by authors in the United States dropped after retraction, this group was still in the majority of citing authors. Some of the possible reasons for the continuation of citation of retracted observed were the lack of plentiful information of retractions generally and the absence of retraction in the self-indexing of some journals.

Conclusion

This study illustrates clearly the dangers that fraudulent or questionable research hold for the research process generally. The works of John Darsee were held up to close scrutiny a number of years ago and many of his publications were found wanting. Despite the publicity his case received, and the extensive reviews to which his papers were subjected, his work remains alive in the research literature of cardiology. Several years after the reviews of his works, Darsee's articles are cited positively (even if the approval is implicit) in subsequent papers. In fact, the age of Darsee's papers may be a factor in his continued citation. The retraction of some of his articles occurred before existing mechanisms

were in place for alerting researchers about retraction. There is the possibility that a scientist publishing new research may be unaware that the cited paper was ever retracted.

While not everything Darsee contributed to the literature was fraudulent or questionable, it remains that much of the fraudulent work of Darsee is currently accepted as though it were valid. One question that remains from this study is: What more could be done to alert researchers regarding erroneous or fraudulent research of the past? There is a substantial amount written about dealing with the problem in the present, about penalties for fraud, and about the responsibilities of academic institutions and government agencies. If there is fraud, however, and if there is some sloppiness due to the rush to get things into print, then there is likely to be continued carelessness with regard to verification of the literature by authors.

An implication for the literature of science and for scholarly literatures in general is that researchers need to take care how they retrieve information. The literature that has been cited in the past cannot be accepted uncritically. It must be treated with the same scrutiny that is accorded to new research; the fact that it is older and can be traced through the citations of former works is not necessarily evidence of its accuracy, validity, or honesty. Garfield and Welljams-Dorof mention the paper by Thomasson and Stanley (1955) which is as valid now as it was then:

> The uncritical citation of disputed data by a writer, whether it be deliberate or not, is a serious matter. Of course, knowingly propagandizing unsubstantiated claims is particularly abhorrent, but just as many naive students may be swayed by unfounded assertions presented by a writer who is unaware of the criticisms. Buried in scholarly journals, critical notes are increasingly likely to be overlooked with the passage of time, while the studies to which they pertain, having been reported more widely, are apt to be rediscovered. (pp. 610–611)

Much of what is written here applies to error as well as to fraud. The symptoms described above relating to pressure to publish and to win grants also manifest themselves as careless errors. The thrust of some of the work of Stewart and Feder (1987) centers on error as a pernicious hindrance to the growth of knowledge. They describe ten classes of lapses from accepted standards for scientific research. These classes include nonmalicious incidents, such as inaccuracies in numerical presentation, to the failure to take action after work has been questioned. Since science depends heavily on its literature in order to build the research base of the future on the work of the past, it is essential that the integrity of the literature by preserved. As is shown here, there is reason to be concerned with the literature of science and the research based on the findings of that literature.

The implications for biomedical research are obvious. Implications for information policy also seem clear in light of the continued positive citation to work that has been shown fraudulent, negligent, or, at best, questionable. A letter to the editor appearing recently in this Journal addresses this issue. Stein acknowledges a concern regarding use of spurious material, and suggests that indexing and abstracting services create a link between retracted or withdrawn papers by adding a note to that effect to the record for the original paper. This is the case with retracted publications indexed in MEDLINE. Kotzin and Schuyler (1989) describe the process employed in handling the retrieval issues surrounding retracted papers. In addition to notification of retraction appearing as part of the retracted citation itself, MEDLINE includes such retrieval mechanisms as the MeSH subject heading "Retraction of Publication" in order to assist with the identification of documents that can present a genuine danger if taken as accurate. Of course, if a paper is never formally retracted, this mechanism is not applicable. It becomes clear that this is both a biomedical research and an information retrieval problem, and that both aspects of the problem require further investigation. Future work should build on that already undertaken to determine the extent of the problem and the possible influences of such a lack of critical scrutiny applied to the literature.

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