PROFESSIONAL RELATIONS BULLETIN

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FROM THE EDITOR . . .

Report from New York

As usual, there were a variety of things to keep your Councilors and officers busy at the ACS national meeting. One important area involved discussion of a staff generated report on reorganization of the Society's governance structure. As I understand it, this was initiated last year by then president Paul Gassman instructing Executive Director Crum to come up with a plan for "streamlining" the ACS. While the impulse may have been legitimate (e.g., concern that there are too many committees operating in the ACS), much was left to be desired about the process.

First, it was kept secret for a long time. Even the Council Committee on Committees (ConC) was kept in the dark. Second, restructuring governance is a *policy* issue, and it is a misuse of the staff to assign this to them; staff should have been made available to *assist* membership representatives in this important task, but it was wrong to ask staff to be the decision makers. And finally, for a variety of reasons, the staff recommendations were not attractive.

For example, *all* professionalism related committees (CPR, CES, PROPPACC, and the Board committee on professional and member relations) would be combined into one Society committee, thus reducing the independence of Council. Another problem was the recommendation that Budget and Finance, currently a committee with membership from both Board and Council, would become a committee of the Board only, eliminating direct Council oversight in this important area. And so on.

ConC chairman Maurice Bursey held an open forum for discussion of the staff plan, and a standing room crowd participated. Discussion was frank, and the result, somewhat to my surprise (I am a victim of some pretty heavy steamrollers in the past), was general agreement that the staff plan should be abandoned, and a new approach should be taken to discuss possible restructurings.

So that there be no misunderstanding, I would like to add two comments. First, the staff should be commended for their efforts; much time was put in to try to accommodate the needs of the leadership, even though the results were not accepted. Second, Dr. Bursey should also be commended for the leadership and good judgement he exercised in directing the open review of the plan when it fell upon him (he was not ConC chairman when this project was initiated).

In a related area, Council considered a petition to change the status of the Committee on Economic Status (CES) from a joint Board-Council committee to a standing committee of the Council. This was a move that the members of the committee regarded as important, in that it would eliminate delays caused by the necessity to get all appointments approved by both the Chairman of the Board and the President; it would enhance their visibility in the Council, with guaranteed time for the committee chairman to address that body; and standing committee status would assure greater long-term stability. This last was of particular importance to the petitioners, I would guess, in light of the restructuring ideas floating around. As it happened, a majority of the Council voted in favor of the change, but it did not achieve the required two-thirds vote. Apparently, the committee has a lot of support, and it will continue in its present role. Stay tuned for further developments.

Henry Hill

As you know, the Division's major award for service to chemists and the furthering of professional relations issues is the Henry Hill Award. Nominations are being sought now, and should be sent to: Dr. Atilla E. Pavlath, USDA Western Regional Research Center, 800 Buchanan Street, Albany, California 94710-1100.

Commercial

Thanks to the efforts of many of you, we have maintained a large enough membership to retain two Councilors. We still need to grow, to emphasize the importance of professional relations issues in the ACS, as well as to pick up another Councilor position or two. Copy the application form in this issue and join up some friends. Check out some chemists you don't like, too. They may be educable.

Dennis Chamot

TOWARD SOME SCIENTIFIC OBJECTIVITY IN THE INVESTIGATION OF SCIENTIFIC MISCONDUCT

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Science is often characterized as the search for truth. Possibly because truthfulness in reporting is a prerequisite for accomplishing this central goal, many people have assumed that scientists are honest. However, this is a logical fallacy. Although it is not possible to achieve the ideals of science with dishonest reporting of research methods or results, it appears to be quite possible to be dishonest in these and many other aspects of one's professional and personal behavior and still have an outstanding career in science. This is evidenced by the increasing number of highly successful scientists who have been accused of lying, cheating, and/or stealing in their scientific work (see Altman, 1988; Anderson, 1991; Committee on Energy and Commerce; 1988, 1989, 1990; Committee on Government Operations, 1988; 1990; Crewdson, 1989; 1991a; 1991b; 1991c; Davis, 1989; Dorfman, 1978; Epstein, 1991; Gordon, 1990; 1991a; 1991b; Greenberg, 1987; 1990; 1991a; 1991b; Hilts, 1991a; 1991b; Kamin, 1974; Roberts, 1990; Sprague, 1991; Stewart & Feder, 1987; Valentine, 1988; Werth, 1991; Wheeler, 1991; Wiernik, 1991).

Incidence of Misconduct. Daniel Koshland (1987), the editor of Science, has claimed that "99.9999% of reports are accurate and truthful (p.41)." Although there is little data on the incidence of scientific misconduct, the available studies suggest that Koshland's estimate may be overly optimistic. For example, the Food and Drug Administration conducted 1,521 random audits in an 11 year period and found what appeared to be misconduct in approximately 11% of the studies. As a result, 62 clinical investigators were decertified to do research (Lisook, 1986, as described by Sprague, 1991). However, as Sprague (1991) has pointed out, this sample may not be representative of the scientific community in general. Davis (1989) found that approximately 39% of men and 57% of women who responded to a large national survey of scientists, reported that their work had been stolen or plagiarized. His findings must be interpreted with some caution because Davis received a low response rate (approximately 33%), and he did not provide subjects with a specific definition of plagiarism. Thus there may be sample biases and variation in the way subjects defined terms. The National Institute of Health's Office of Scientific Integrity has received approximately 180 allegations of scientific misconduct since it opened in 1989, and has thus far convicted about 20% of those charged (Wiernik, 1991).

Some scientists who have committed scientific misconduct may have contributed numerous erroneous findings to the scientific literature. After Dr. John Darsee confessed to fabricating the data for one paper, investigators found that he had forged much of the data that formed the basis of more than 100 publications (Stewart and Feder, 1987). Stewart and Feder attempted to understand how so many fraudulent publications could have entered the scientific literature, undetected by the coauthors, reviewers, and editors, who had been charged with careful examination of the papers. They examined Darsee's 18 published research articles to determine whether there were errors or discrepancies that could have been detected just from inspecting the published reports. Of the 18 papers, only two had no apparent errors. Twelve papers had ten or more errors each, ten papers had 14 or more errors apiece. The two most error-ridden papers had 28 and 39 errors that could be discerned from the published text alone. Stewart and Feder's analyses seriously call into question how well the checks and balances in science actually work and hence the integrity of the scientific literature.

Although it is impossible to calculate a misconduct rate from the data described above, these data suggest that misconduct is more than a rare problem. In addition to the limited empirical evidence, there are also several theoretical reasons to hypothesize that the perceived incidence of scientific misconduct is lower than the real incidence. These include: failures to detect, report, prosecute, and/or convict wrongdoers and the lack of visible punishment or publicity when those committing scientific misconduct are found guilty.

Based on a presentation at the DPR Symposium, "Whistleblowers, Advocates and the Law", held at the national ACS meeting in New York City, August 27, 1991.

Detection Rate. The detection rate for scientific misconduct may be considerably lower than the real incidence rate. First, those individuals who are outside of a lab have little access to the raw data, lab notebooks, data analyses, and other records which might provide evidence of misconduct. Although it is often claimed that there are checks and balances in science such as editorial review, replication of research, and the cumulative nature of research which would cause misconduct to be detected, Stewart and Feder's (1987) data suggest that these checks may not catch misconduct. Editors and reviewers may or may not recognize plagiarism, note that data are too good to be true, notice that statistics and data do not fully correspond, or be able to distinguish between fraud and error, anomalous findings and fabricated data.

Another check is supposedly provided by replication. However, exact replication of a study is often considered to be relatively uncreative research and thus is often not published by editors. Likewise failures to replicate are often not accepted for publication (e.g., Funder & Block, unpublished manuscript). In addition, failures to replicate can be attributed to real experimental differences or error, which are often difficult to distinguish from fraud.

It also has been argued that the cumulative nature of research is a check because anomalous results are judged to be questionable. However, many scientific findings do not interrelate sufficiently that aberrant results would stand out as truly improbable or impossible. This is particularly true in new areas of research. In addition, when results do seem unreasonable, the person who detects the anomaly may prefer to give their colleague the benefit of the doubt and hence to attribute the results to error, rather than misconduct. Finally, even if someone suspects misconduct, they often have no power to obtain the evidence needed for proof and hence do not pursue their suspicions.

Reporting Rate. In the event that misconduct actually is suspected or detected, it is often not in the best interests of the person detecting misconduct to report it. Those who are most likely to detect misconduct are individuals who are most closely associated with the wrongdoer, and possibly even working in the lab where the misconduct occurs. These individuals are generally either students, friends, mentors, or collaborators of the person committing the misconduct, who want to avoid getting their colleague in trouble. Not only may they risk losing the allegiance, help, funding, letters of recommendation, and other resources and support from the perpetrator, but also that of the perpetrator's friends and professional allies.

The people who expose misconduct may find their careers to be severely harmed if they report it. If they are associated with the person who committed the misconduct, their own reputation may be sullied. In addition, retaliation against whistleblowers is commonplace in the scientific community, irrespective of whether the whistleblower's allegations are correct (e.g.,Committee on Energy and Commerce House of Representatives, 1990; Committee on Government Operations, 1990; Hilts, 1991b; Sprague, 1991, Wiernik, 1991). Whistleblowers have seen their funding cut, reputations damaged, jobs terminated, promotions denied, careers derailed, and found themselves to be the objects of investigations and lawsuits and their colleagues turning against them, for living up to the scientific ethic that requires anyone who knows of scientific misconduct to report it (see Hilts, 1991b; Gordon, 1990; 1991; Greenberg, 1991a; 1991b; Sprague, 1991; Wiernik, 1991). Hence fear may prevent many scientists from reporting misconduct.

Even if it were not professionally dangerous, it can be extremely time and energy consuming to be involved in an investigation of misconduct because whistleblowers are often made to carry the burden of proof for their allegations. And even if they do prove that misconduct has occurred, little may be done about it (this will be elaborated below). In sum, there are few, if any, rewards for reporting scientific misconduct, and the potential risks and costs of whistleblowing are great. Hence it seems likely that the incidence of reported misconduct is considerably less than that which is suspected or detected.

Prosecution and Conviction Rate. In the event that suspicions of misconduct are reported, they may not be fully investigated. Investigations are generally handled by the institution that supports the research of the person alleged to have committed misconduct. The interests of the alleged wrongdoer and the institution where he or she does his/her research are often in alignment such that the institution stands to lose benefits and incur costs (i.e., to be punished) if the misconduct is exposed. For instance, the institution may lose grant money, reputation, good students, status, and other important resources if one of its researchers is convicted of scientific misconduct. Because of these conflicts of interest, institutions often ignore the complaint, do a sloppy investigation, or even intentionally cover-up (e.g., Committee on Energy and Commerce, House of Representatives, 1988; 1989; 1990; Committee on Government Operations, 1988; 1990).

At a more personal level, the individuals who are asked to carry out the investigation are often colleagues or even friends of the alleged perpetrator (e.g., O'Toole, 1991; Gordon, 1990; 1991). The biases of such investigators may motivate them to avoid careful examination of the evidence of misconduct and too readily accept the defendant's explanation. It appears to be a common strategy of such investigators to deflect attention away from the actions of alleged wrongdoer by focusing blame on the alleged motives and actions of the whistleblower. Often the whistleblower becomes the object of accusations and investigation (Committee on Government Operations, 1990; Gordon, 1990; 1991a; 1991b; Greenberg, 1991; Hilts, 1991b; Sprague, 1991; Werth, 1991).

Punishment and Publicity for Convictions. In the event that wrongdoing is detected and reported and the wrongdoer is prosecuted and convicted, there may be little or no punishment for, or publicity of, the wrongdoing (e.g., Wheeler, 1991). Institutions hide behind the cloak of "confidentiality", producing secret reports which withhold the evidence and findings from public inspection. Few, if any, sanctions may be dealt out for the guilty (e.g., Greenberg 1990; 1991a; Hilts, 1991a, Wiernik, 1991), possibly because sanctions are often determined by those whose interests are aligned with the wrongdoer's.

There are good theoretical reasons to suggest that the incidence of scientific misconduct may be much greater than the number of cases that have been detected, reported, successfully prosecuted, and publicly documented. The incidence of scientific misconduct may be great enough to seriously threaten public health, safety, and welfare (e.g., Roberts, 1990), as well as to harm the careers of scientists who's intellectual properties are misappropriated (e.g., Greenberg, 1990, 1991a; Hilts, 1991b; Roberts, 1990; Wiernik, 1991) and to slow scientific progress in important research areas (e.g., Crewdson, 1989; 1991c; Roberts, 1990). In addition, it is possible that millions of dollars of limited public funds are wasted that could be better spent elsewhere (e.g., Committee Energy and Commerce, 1988; 1989; 1990; Committee on Government Operations House of Representatives, 1988; 1990). Until we have data to refute these hypotheses, they can not be dismissed.

Reactions from the Scientific Community. The reactions of many of the most outspoken members of the scientific community to these challenges have been anything but scientific. Some scientists have readily generalized from their personal observations and beliefs to make factual claims about scientific misconduct (e.g., Koshland, 1987), while other scientists have actually attempted to suppress the free speech of those who have proposed alternative hypotheses and brought to light evidence supporting these hypotheses (see Greenberg, 1991b). Reports about the treatment of whistleblowers in science (e.g., Gordon, 1990; 1991; Greenberg, 1987; 1990; 1991a; 1991b; Hilts, 1991b; O'Toole, 1991; Sprague, 1991; Werth, 1991; Wiernik, 1991) also reveal a scientific environment in which evidence is suppressed and truth may not be valued as much as maintaining the status quo. Many scientists appear to be unwilling to objectively examine evidence of misconduct (see Greenberg, 1987; 1990; 1991a; 1991b; Hilts, 1991a; 1991b; O'Toole, 1991; Sprague, 1991). The scientific community has been even slower to objectively pursue allegations that the universities and institutes, which were charged with the responsibility to fairly investigate misconduct, have instead intentionally covered up the wrongdoing of their researchers.

Prominent scientists predicted that Congressional involvement in investigating fraud and misconduct would stifle intellectual and scientific freedom (see Foreman, 1988). However little evidence has been brought forward to support this hypothesis. There may be more evidence to support the conclusion that the scientific community's alarmist reactions, prejudices, fears, intolerance of hypotheses that contradict what scientists want to or do believe, and unwillingness to objectively evaluate evidence may be posing a greater threat to scientific freedom (see Greenberg, 1991b). Scientists who want to do something constructive about scientific misconduct can begin by treating this topic like any other in science -- with an open mind, setting aside personal biases; encouraging free discussion of alternative theories and hypotheses; carefully gathering data; and impartially evaluating the available evidence.

References

Altman, L. K (1988). Eminent Harvard professor quits over plagiarism, university says. New York Times, November 29, 1 & 8.

Anderson, C. (1991). NIH: Imanishi-Kari guilty. Nature, 280, 262-263.

Foreman, J. (1988). Baltimore speaks out on disputed study. The Boston Globe, May 23, 31-34.

Committee on Energy and Commerce House of Representatives, Subcommittee on Oversight and Investigations. (1988). Fraud in NIH Grant Programs, Washington, D.C.: Government Printing Office, April 12, No. 100-189.

Committee on Energy and Commerce House of Representatives, Subcommittee on Oversight and Investigations. (1989). Scientific Fraud, Washington, D.C.: Government Printing Office, May 4 & 9, No. 101-64.

Committee on Energy and Commerce House of Representatives. (1990). Scientific Fraud, Washington, D.C.: Government Printing Office, May 14, 94-817.

Committee on Government Operations House of Representatives (1988). Scientific fraud and misconduct and the federal response. U.S. Government Printing Office, April 11.

Committee on Government Operations House of Representatives (1990). Are scientific misconduct and conflicts or interest hazardous to our health? U.S. Government Printing Office, September 10, 101-688.

Crewdson, J. (1989). The great AIDS Quest: Science under the microscope. Chicago Tribune, November 19, Section 5, 1-16.

Crewdson, J. (1991a). 3 deaths in AIDS vaccine tests: Researchers failed to report fatalities; project continues. Chicago Tribune, April 14, Section 1, 16 & 17.

Crewdson, J. (1991b). NIH probe: Scientist made false statements: Brain research received agency grant. Chicago Tribune, July 28, Section 1, A4.

Crewdson, J. (1991c). Probe finds fraud in AIDS studies. Chicago Tribune, August 11, Section 1, 1 & 15.

Davis, M. S. (1989). The perceived seriousness and incidence of ethical misconduct in academic science. Unpublished doctoral dissertation, Ohio State University.

Dorfman, D. D. (1978). The Cyril Burt Question: New Findings. Science, 201, 1177-1186.

Epstein, K. C. (1991). Probers fault new NIH head in Cleveland Clinic inquiry. Cleveland Plain Dealer, August 1, 1 & 9.

Funder, D. & Block, J. (unpublished manuscript). Length of friendship and trait attribution: Failure to replicate.

Gordon, G. (1990). Lab aide sues U-M, boss over research. The Detroit News and Free Press, May 6, 1A & 9A.

Gordon, G. (1991a). "Whistleblower" backed in science inquiry. Detroit News, March 21, A3.

Gordon, G. (1991b). Academic fraud charged at U-M. Detroit News and Free Press, May 12, A3 & A5.

Greenberg, D. (1987). Publish or perish - or fake it. U.S. News & World Report, June 8, 72-73.

Greenberg, D. (1990). The politics of plagiarism: Plagiarist gets promoted; his victim loses her job. The Medical Post, September 25, 11.

Greenberg, D. (1991a). And now a "Policy to protect misconduct in research." Science and Government Report, June 15, 5.

Greenberg, D. (1991b). From high drama to pure farce: NIH's bungling goes on in the Baltimore case. Science and Government Report, 21, 2.

Hilts, P. J. (1991a). Crucial research data in report biologist signed are help fake: Nobelist to ask retraction of paper he defended. The New York Times, March 21, A1.

Hilts, P. J. (1991b). Hero in exposing science hoax paid dearly. New York Times, March 22, A1.

Kamin, L. (1974). The science and politics of I.Q. Potomac, MD: Erlbaum.

Koshland, D. E. Jr. (1987). Fraud in Science. Science, 235, 141.

O'Toole, M. (1991). Margo O'Toole's record of events. Nature, 351, 180-183.

Roberts, S. (1990). Lab Rat: What AIDS researcher Dr. Robert Gallo did in pursuit of the Nobel Prize, and what he didn't do in pursuit of a cure for AIDS. Spy, July, 70-79.

Sprague, R. (1991). From doubts to conviction of fraud: Tales of trauma. Presentation to the American Chemical Society, New York, NY.

Stewart, W. W. & Feder, N. (1987). The integrity of the scientific literature. Nature, 325, 207-214.

Valentine, P. W. (1988). Drug researcher pleads guilty to fraud: Psychologist used phone research results to obtain NIMH grants. The Washington Post, September 20, A8.

Werth, B. (1991). By AIDS Obsessed: Dr. Robert Gallo is determined to receive the recognition he feels is owed him for discovering the AIDS virus. Pulitzer Prize-winning reporter John Crewdson is even more determined to prove that Gallo is a fraud. Gentlemen's Quarterly, August, pps. 144-151, 205-208.

Wheeler, D. L. (1991). U.S. has barred grants to 6 scientists in past 2 years. The Chronicle of Higher Education, XXXVII, July 3, 1 & 6.

Wiernik, J. (1991). Suits charge plagiarism in research. The Ann Arbor News, September 16, B1 & B2.

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