

Watching the Watchers

Elias A.K. Alsabti came to this country from Jordan in 1977 and passed through research laboratories at Temple University, Jefferson Medical College and the M. D. Anderson Hospital and Tumor Institute before acquiring an M.D. degree from the American University of the Caribbean in Montserrat and gaining entrance to two internal medicine residency training programs. When plagiarism charges involving at least seven research papers caught up with Alsabti, he was asked to resign from a program affiliated with the University of Virginia School of Medicine and, about two months later, from a program affiliated with Boston University School of Medicine.

Alsabti's alleged activities first came to light last year when E. Frederick Wheelock of Jefferson Medical College complained in a letter to the editor in the April 12, 1980 LANCET that a paper published by Alsabti included "an almost verbatim copy of the background section of my research grant application... the remainder of the article came from early drafts of my manuscripts." Another of Alsabti's papers was virtually identical to one published by Daniel Wierda and Thomas L. Pazdernik in the EUROPEAN JOURNAL OF CANCER. Alsabti had apparently intercepted the paper when the journal sent it out for review. Alsabti's version made it into print before that of Wierda and Pazdernik and left Wierda with the task of trying to make sure the record does not suggest that his work was derived from Alsabti's.

One month after SCIENCE (Vol. 209, No. 4453) reported on Alsabti, a front-page New York Times story (Aug. 9, 1980) recounted a bitter wrangle between officials of Columbia University College of Physicians and Surgeons, Columbia's new chairman of medicine Philip M. Felig and Yale University School of Medicine's dean Robert W. Berliner. A Columbia P&S committee had found Felig lacking good judgment over his handling of accusations of plagiarism and data falsification involving Vijay Soman, a junior colleague and coauthor of Felig when he was at Yale University. The charges stemmed from a curious coincidence: Felig had passed a paper on insulin binding in anorexia nervosa sent to him for review by the NEW ENGLAND JOURNAL OF MEDICINE to Soman for comments, a common practice among reviewers. A few months later, Soman and Felig submitted a paper on similar research to the AMERICAN JOURNAL OF MEDICINE. Their paper was sent for review to the authors of the NEJM paper, and an author

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Recent scandals in the scientific community have led to public inquiries, a congressional subcommittee hearing and soul-searching within the research community

of that paper, Helena Wachslicht-Rodbard, complained that the Soman/Felig paper included plagiarisms and asked for an investigation. She also questioned whether the work reported by Soman and Felig had actually been carried out.

Eventually, Soman admitted to having used the NEJM paper as a crutch for preparing his own and to having doctored reported data. In addition, Soman could not provide reviewers with data to support at least eight other published papers. He resigned from Yale early in 1980, about a year after the charges were first made.

In the meantime, Felig had been selected for the coveted P&S chairmanship. In January 1980 he had proposed Soman for a position at Columbia but withdrew his name in late February. Questions about the handling of the Soman situation subsequently led to a Columbia faculty committee recommendation that Felig resign. After issuing a rebuttal, Felig returned to Yale, where he is a professor of medicine.

Another case of cheating was reported in the March 6 SCIENCE. It involved data falsification by John C. Long, a young researcher at the prestigious Massachusetts General Hospital. (Long now works as a surgical pathologist in a Michigan hospital.) The falsification involved fabrication of results of a series of experiments published in the JOURNAL OF THE NATIONAL CANCER INSTITUTE. An inquiry was triggered by a colleague's suspicions of those data — coupled with the discovery that Long had withheld information suggesting that the cell culture lines he worked with were contaminated — and eventually led to the demise of the entire project and

Long's resignation, as well as challenges of other papers he has published.

Yet another charge of data falsification has generated what some think is a tardy National Institutes of Health investigation. As recounted by Alexander Capron at a recent hearing of the House Committee on Science and Technology's subcommittee on investigations and oversight, the case involves complaints of data faking in a cancer research project conducted at Boston University Medical Center in 1978. The data reported by a B.U. research unit headed by Marc Straus were expunged from the records of the Eastern Cooperative Oncology Group. Straus and other unit members were asked to leave B.U. Straus later moved to the New York Medical College at Valhalla and received another National Cancer Institute research grant "approved by the Valhalla IRB [institutional review board] and by the several review committees at NCI, none of whom had been notified of the charges against Dr. Straus," said Capron, director of the President's Commission for the Study of Ethical Problems in Medicine and Biomedical and Behavioral Research.

The Straus situation will be discussed again at a June 5 hearing of the President's commission on ethics in Boston, where Straus hopes to clear the record, he told SCIENCE NEWS. "I asked to testify [because]... the allegations are not true, and I can prove it," he says. Not surprisingly, Straus believes existing procedures for handling such allegations to be inadequate. "I asked from the beginning for an independent peer review, and I just couldn't believe that I couldn't get the agencies involved to conduct one. ... Due process just didn't exist," he says, adding that the new NIH debarment regulations are ominous in their implications because researchers could lose grants "without any proof of wrongdoing."

These incidents have generated a mixed response within and outside the research community. Although virtually all scientists agree that data falsification and plagiarism are cardinal sins in research, opinions differ on how common they are and on what their causes may be. Furthermore, no one seems quite sure how they can be detected or prevented without damaging an already stressed research system.

The detection of these incidents, says Princeton University's Patricia Woolf, is basically a healthy sign reflecting "a relatively normal professional housekeeping

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activity. . . . To me there is no doubt that people are going to be more scrupulous and engage more in anticipatory skepticism. What we're going to see is more attention to standards and prompter reactions" to violations of those standards, as well as "some new mechanisms" for handling such allegations and insuring due process within the scientific community."

Others, however, are not so sure that the recent scandals are isolated aberrations. In Boston, at a recent meeting of the Council of Biology Editors, University of Georgia ecologist Frank Golley gave a gloomy account of science at present: "We have reached the point where it is very expensive and very difficult to assure the quality of publication. An author who manufactures data or who plagiarizes another paper or a grant application is not likely to be detected. SCIENCE magazine reports the sensational cases of unethical performance but these cases are, I suspect, the tip of an iceberg. In biology, where there may be 4.5 million species and tens of thousands of habitats, scientists could work for years with no one wanting or able to repeat their experiments and observations. I personally feel that the system is reaching a crisis point where the whole process of responsible communication is in jeopardy."

The scheduling of a session on ethics in science and publication at the CBE meeting was not surprising, given the recent cases of falsification and plagiarism in published scientific papers. Journal editors, already beleaguered by rising publication costs and proliferation of journals and research, are not overjoyed by the prospect of having to tighten review procedures and check the credentials of those who submit papers for publication. Although larger journals have fulltime professional staff, many operate, as one editor observed during the meeting, "with half a secretary and some stamps." And many have felt increasingly sharp twinges of alarm as last year's crop of scandals emerged in the press.

John C. Bailar III, who was editor of the JOURNAL OF THE NATIONAL CANCER INSTITUTE when Alsabti's activities were reported, put it this way: "I read about Alsabti in SCIENCE on a Sunday night, and I can assure you that I was in the office checking our files at the crack of dawn Monday. Luckily, we'd rejected the three papers he'd sent us."

Still, many noted at that time that Alsabti's papers appeared primarily in foreign or obscure journals and doubted that such a

problem could arise except on the fringes of biomedical research.

Thus, the Felig/Soman uproar came as a shock. And although some witnesses at the recent congressional hearing assured the subcommittee that existing safeguards within science provide adequate protection against abuse, Felig testified that "the recent revelation of a number of instances of data falsification leads to the conclusion that in scientific research there are those who are prone to unethical behavior. This new awareness precludes a 'business as usual' attitude. We must learn from such unfortunate experiences and strive to implement mechanisms to minimize the likelihood of such recurrences." Senior

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scientists, Felig suggested, should limit their delegation of research to junior colleagues to areas in which they have "intimate familiarity with the techniques employed" or exercise particular care in reviewing the original data reported. Furthermore, institutions should develop review procedures for situations in which data are challenged and take the matter "out of the hands of the scientists whose work is in question"—a step that has been taken at Yale, Felig said.

He also recommended that journals "recognize their responsibility to publish retractions when data has been found to be unreliable," a step editors at the Boston meeting had no quarrel with.

Other suggestions, however, such as having researchers submit raw data with their manuscripts, meet with little enthusiasm among researchers or journal editors. The logistics of handling such data would be difficult, for one thing. Besides, as BRITISH MEDICAL JOURNAL editor Stephen Locke observed in Boston, a researcher intent on falsifying results could also falsify the raw data submitted.

Locke and other editors expect that computerization will help journals keep pace with the "information explosion" and develop more sophisticated ways of selecting reviewers. More widespread use of computerized literature searching might also detect at least some plagiarisms and perhaps stem the spread of a phenomenon called by ANNALS OF INTERNAL MEDICINE editor Edward J. Huth "salami science"—the publication of the thinnest possible slices of a research project in the maximal number of journals. Detecting outright frauds, however, is a prospect that editors think nearly impossible as well as outside their domain.

A case in point is the falsification perpetrated by Long. Asked whether it could have been detected, Bailar said no. "When this first came to my attention, I went back and reread the paper. . . . That falsification was undetectable at the time it was made."

According to the SCIENCE account, a data book submitted by Long to one of his colleagues provided "reason to suppose that the pertinent section of the notebook had been forged," but that illustrates Locke's point about the impracticability of requiring raw data to be filed with journals. The BRITISH MEDICAL JOURNAL will probably require, however, that authors agree to make raw data available for several years as a condition of publication, Locke told SCIENCE NEWS.

To some researchers, the notion of "playing policeman" is abhorrent and would destroy the trust they consider essential to the scientific enterprise. Other pressures are already placing stresses on that system; as one administrator expressed it, "I'm lucky. I'm not trying to support a wife and a family on a year-to-year research grant. If you're doing that, you *have* to have good data, and the pressures are enormous."

Felig expressed a similar view during the congressional hearing when he said one lesson he had learned from the Soman episode was that "senior investigators must recognize that junior colleagues may misconstrue their behavior as 'pressure' and may not share the senior individual's ethical principles. Consequently, the senior scientist must be vigilant to the possibility that a seemingly trustworthy colleague may be manipulating data to 'please his boss.'"

Or to get a grant renewed, or, perhaps, from "some sickness of heart and mind that lies outside science." The question now being probed within and outside science is not why scientists occasionally cheat, however — but what the scientific process can do to prevent it and whether existing mechanisms work well enough to minimize the deleterious effects of what National Institutes of Health director Donald Fredrickson terms "rogue scientists."

Lawrence K. Altman, the New York Times reporter who broke the Felig/Soman story, told CBE members in Boston that more must be done. "Research once was private, but because the taxpayers are now paying for it, the research community is now subject to the same scrutiny as the Defense Department and the Congress. The researcher is a public servant," he said.

The congressional hearings sounded the same note. Assurances from witnesses such as Philip Handler, outgoing president of the National Academy of Sciences, that "the matter of falsification of data... need not be a matter of general societal concern," however, did not mollify congressmen concerned about wasting "the taxpayers' money."

Explanations from William A. Raub of the National Institutes of Health about procedures established last October enabling NIH to suspend or debar researchers from receiving federal funds in "cases in which proof of wrongdoing exists" were not well received by the subcommittee either.

Although Raub also described the institution of a "tracking system" under which NIH officials can be alerted if applicants for grants are under investigation for charges of malfeasance, his emphasis on the agency's "presumption of innocence" drew fire from several committee members, particularly when he invoked it in connection with the Straus situation. "Presumption of innocence is strictly a legal concept in a legal context," said Rep. Robert Shamansky (D-Ohio) — inappropriate in making decisions to award federal grants. All in all, the NIH procedures for dealing with research fakery were deemed inadequate by Shamansky and his colleagues. Subcommittee chairman Albert Gore Jr. (D-Tenn.) did, however, assure his audience that the hearing

was not intended as a prelude to the development of "any regulatory scheme" to deal with scientific fraud.

Such an effort would no doubt generate howls of protest from researchers who already feel overburdened by governmental red tape and threatened by possible cuts in funding, and who wonder whether fierce competition and dependence on year-to-year grants may be contributing to "ethical corner-cutting" among young scientists.

Ironically enough, the one point virtually everyone discussing the issue agrees on is that there are no data on the incidence of fakery in scientific research. That lack of data makes it difficult for the scientific world to offer an effective response to challengers like Altman and members of the congressional subcommittee, who wonder if research falsification may be more common than scientists would like to believe and wonder how much of it may go undetected.

Woolf, Handler, Fredrickson and others have explained in detail the aspects of science that operate to deter investigators from such scientific crimes as plagiarism and data falsification, not the least of which are the at times draconian consequences of detection — loss of one's livelihood, reputation and professional standing. The penalties are severe enough so that comments like Shamansky's evoke fears of "blacklisting" and "witch-hunting" within the research world. As NCI's Saul Schepartz commented in connection with the Straus investigation, "The question of who did what to whom is very much in the air at this point... Expunging falsified data is one thing, but pinning blame is another." No legal charges have been brought, he emphasized. Schepartz and others have observed that investigators deprived of research grants pending any formal determination of wrongdoing might well seek recourse in court themselves.

It is a prospect few within science like to contemplate, and it appears that most would like to believe Fredrickson's assertion that "the current production of useful new knowledge is nothing short of spectacular, and testifies to a vigorous state of health in the life sciences." But on the other, darker side, are the views expressed by Golley. "Publication has become the primary basis for establishing status and success. As the number of publications

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has grown it is more difficult to judge the accuracy and honesty of offered papers. The ingenious and dishonest cheat can publish and republish the same piece of data or can manufacture data without detection. The larger society uses and misuses scientific information with little regard for scientific ethics.... The use of scientific publications as a medium for communication between scientists is compromised and the entire scientific enterprise suffers and is diminished.

"... we are obliged to carry on a rear guard action by slowing down the process, using more reviewers, setting up computer files of reviewers and authors, checking and cross-checking papers.... All of these suggestions are palliatives, I'm afraid. But they may retard the deterioration of science through compromise of published communication," Golley concluded.

Some journals are taking those steps, and the Council of Biology Editors is even considering the feasibility of a study of the efficacy of peer review in biomedical journals being suggested by Bailar, now a consultant to the NEW ENGLAND JOURNAL OF MEDICINE, Drummond Rennie, of NEJM, and Alfred Yankauer, editor of the AMERICAN JOURNAL OF PUBLIC HEALTH.

Unfortunately, without data, the significance of the recent science scandals and the answers to the many questions about the nature and health of the country's scientific research enterprise that have emerged in their wake remain, uncomfortably, a matter of opinion. □

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