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A FRAUD THAT SHOOK THE WORLD OF SCIENCE

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A Junior Scientist Accuses a Senior Scientist

On a bright, cold morning in early February 1980, Jeffrey Flier, a tall, mustachioed young physician, boarded a train in Boston on his way to New Haven to carry out a distinctly disagreeable professional task. He was going to conduct an "audit" at the Yale University School of Medicine: He would spend some hours interrogating and examining the laboratory records of an associate professor there, one of whose published research papers on insulin metabolism had been called fraudulent by another researcher.

Dr. Flier - he pronounces it "flyer" - was well qualified for the assignment. He had trained in diabetes research at the National Institutes of Health (N.I.H.) in Bethesda, and now, though only 31, he was chief of the diabetes metabolism unit of Beth Israel Hospital in Boston and an assistant professor at the Harvard Medical School. He was intimately acquainted with the special technique of studying insulin metabolism used in the challenged paper and had been accepted by both sides as an impartial auditor.

Flier, recounting the events of that day not long ago, recalled feeling somewhat uneasy about the coming encounter; he had never taken part in an audit and had no clear idea of how to comport himself or what to expect. (Scientific audits are very uncommon, and there are no standard rules by which to conduct them.) But he knew that, no matter how he acted, the meeting was bound to be stressful and humiliating for his interviewee, Dr. Vijay R. Soman. Still, Flier was not deeply anxious, since he felt confident he would find no evidence of overt fraud. On the basis of having met Soman and read some of his research papers, Flier took him to be a serious and solid, if not especially innovative, scientist. At the medical schools and hospitals where Soman had been trained in his native India and in this country, he had made an impressive enough record to win a fellowship in endocrinology at the Yale School of Medicine in 1975 and had been appointed to its faculty the next year. Now, at 37, he held the rank of associate professor; primarily a laboratory researcher, he was currently studying the conditions under which human blood cells "bind" - seize and hold onto - molecules of insulin, the hormone that enables the body to utilize sugar. Papers that Soman had written with various Yale colleagues reporting these studies had recently appeared in such prestigious publications as Nature, The New England Journal of Medicine and The Journal of Clinical Investigation.

Soman's senior collaborator on the paper under attack, moreover, was Dr. Philip Felig, a highly respected medical researcher, the occupant of an endowed chair and chief of endocrinology research at the Yale School of Medicine. Felig, a prodigious worker, was known in his department to expect and to get a high output of publishable research from his subordinates. He himself, at 43, already listed in his bibliography some 200 published research papers - more than most scientists produce in a lifetime - the fruits in part of his own energy and in part of co-authorship, a widely practiced system by which the senior partner supervises the work of a number of junior colleagues, co-signs their papers and shares credit for their work. He had won more than a dozen academic prizes and awards and held substantial research grants from the National Institutes of Health, the American Diabetes Association and the March of Dimes Foundation. Flier considered it most unlikely that such a man would have taken part in even a marginal scientific impropriety, let alone so heinous an offense as the outright faking of research data.

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Yet that was what Felig and Soman had been accused of. A year earlier (as Science magazine would eventually reveal), Dr. Helena Wachslicht-Rodbard, a young endocrinology researcher at the National Institutes of Health, had bitterly complained to her supervisor and other persons that Felig and Soman, in one of their papers, had plagiarized some of the words and ideas of a paper of hers that they had seen that had not yet been published. Moreover, she had written to Robert W. Berliner, dean of the Yale School of Medicine as well as Soman's and Felig's superior, questioning the "authenticity" of the data in their paper and suggesting that they had never actually done the research reported in it.

In that letter, she found no fault with the conclusions of the Soman-Felig paper, for, like her own, theirs reported a discovery of some minor scientific value - namely, that the blood cells of patients with anorexia nervosa (a morbid aversion to food) tended to bind more insulin molecules per cell than is normal, but that when the patients were treated with behavior therapy and could eat again, their blood cells behaved in more usual fashion. What provoked Dr. Wachslicht-Rodbard's attack were, rather, certain trivial omissions and improbabilities in the Soman-Felig paper. For one thing, neither the psychotherapists nor the hospital where the studies had been done were named. For another, the six patients in the study - all female - had had no menses while anorexic but had all resumed menses upon gaining weight, a neat but clinically improbable outcome.

Finally, as Dr. Wachslicht-Rodbard has often told associates, she couldn't help feeling that certain other data were suspiciously accurate. To find out how many insulin molecules are bound by each blood cell, a researcher mixes batches of the cells and plasma with radioactive insulin, then separates the cells from the plasma and measures the radioactivity of each; this reveals how much insulin was seized by the cells and how much remains free in the plasma. Such measurements, when plotted on a graph, normally lie close to - but rarely dead on - an ideal curve of some sort. The Soman-Felig data, however, were very nearly perfect.

In retrospect, Jeffrey Flier, too, realizes that he had noticed this oddity: "You'd shake your head and say, 'How did they ever get such beautiful data?' " Still, neither Flier nor those colleagues he talked to about it believed that the beautiful data were the result of deliberate fraud. For concocting fake data or even "fudging" real data - tinkering with the numbers to make them look better - is the worst possible sin in science, a violation of the sine qua non of the scientific ethos: absolute truthfulness. END PART ONE PICK UP PART TWO

Truthfulness in Science

In 1942, in a now-classic analysis of the ethos of science ("The Normative Structure of Science") the eminent sociologist Robert K. Merton listed among the moral norms by which scientists live "disinterestedness" (the willingness to work to extend knowledge, apart from personal benefit) and "communality" (the free sharing of one's discoveries with others). What makes such altruism possible is the "reward system of science," as Merton later called it: Honor, position, power and money go to those who make discoveries first - and who claim priority by promptly publishing their findings.

But while the system rewards priority of discovery, it penalizes with equal severity any effort to claim priority by means of fakery, since the rewarding of fraudulent discoveries would undermine the entire knowledge-sharing structure. Until recently, this has worked admirably: Deliberate fraud has been extremely rare.

In recent years, however, a series of important cases of scientific fraud has caused concern that there is an ominous weakening of the norm of scientific truthfulness. One much-publicized incident took place in 1974 at the Memorial Sloan-Kettering Cancer Center in New York. Dr. William Summerlin, who believed he had found a way to prepare mouse skin so that it could be grafted onto an unrelated recipient without being rejected, kept getting failures; one morning, expecting visitors to his laboratory, he used a marker pen to paint what looked like a black skin graft on a white mouse; he landed on the front pages, not as a discoverer but as a perpetrator of fraud. Another incident occurred in 1980 at Massachusetts General Hospital. There, Dr. John Long, who had been studying the characteristics of Hodgkin's disease in cell cultures from human patients, could not get certain measurements to turn out as he expected and, impatient to publish, he made

up numbers that looked right. Most of his uncooperative cells, other workers then discovered, were those of monkeys, not human patients (no one, including Long, knows how the contamination occurred); Long's years of work and his reputation were destroyed.

The significance of these and other such incidents lies less in their frequency, however, than in the shock and alarm they have engendered in the public and in the scientific community. Science administrators, testifying on scientific fraud this past spring before the Subcommittee on Investigations and Oversight (headed by Representative Albert Gore Jr.) of the House Committee on Science and Technology, said they doubted there was any major increase in such deviant behavior. What had increased, they agreed, was the public awareness of, and attention being paid to, such episodes.

This greater awareness and interest, however, strongly suggest a growing distrust of science and scientists by the public and the Congress, and such distrust could have grave consequences for a society as dependent on science as ours. Equally grave might be the consequences for science itself of endemic doubt and suspicion within its ranks, since mutual trust is essential to the system; where episodes of overt fraud have come to light, there seems to be, as I have heard from a number of scientists, a subtle but unmistakable loss of faith.

Grounds for Suspicion

The files of The New England Journal of Medicine show that in November 1978, more than a year before Dr. Flier's trip to New Haven, Helena Wachslicht-Rodbard had sent to that publication the article "Insulin Receptors in Anorexia Nervosa," reporting work that she and a couple of colleagues had recently completed. The editors, as usual, sent it out for "peer review"; each paper submitted is appraised by specialists in the appropriate field as to its scientific soundness, one of the purposes of which is to prevent fraudulent research from being published.

Two and a half months later, Dr. Wachslicht-Rodbard received her manuscript back; in a covering letter she was told that if she would revise it in accordance with three enclosed critiques by anonymous reviewers (one of whom had recommended rejection rather than revision), it would be considered. She immediately set about rewriting the paper. Some days later, Dr. Jesse Roth, the head of the diabetes laboratory in which she worked and the senior co-author of her paper, asked her to review a manuscript sent him by The American Journal of Medicine. She was upset, she later told a reporter for Science, when Roth mentioned that it was on exactly the same subject as her own paper and had been written by Vijay Soman and his distinguished superior, Philip Felig, of Yale. She read the manuscript and was dumbfounded to see that a number of brief passages, totaling about 60 words, were identical to those in her article, as was a formula she had devised for working out the number of receptor sites per cell. Yet other than her colleagues and the editors of The New England Journal of Medicine, no one had seen her paper -except the three anonymous reviewers. On closely rereading their comments, she guessed correctly that the reviewer recommending rejection had been Philip Felig - who therefore had seen her paper before Soman and he had sent their own to The American Journal of Medicine.

Felig, a small-featured, somewhat owlsh man, articulate in speech but cool in manner, recently explained this to me without any evident discomfort. When the Wachslicht-Rodbard paper came to him for review, he said, he asked Soman to comment on it first; then, he himself looked the paper over and wrote his adverse review. He did not consider it improper to do so, even though he and Soman had an almost identical study nearing completion, because reviewers are selected for their special knowledge. In this case, however, it would be touch and go who got priority, and at the least, he conceded, he should have told Dr. Arnold Relman, editor of The New England Journal of Medicine, to view his comments in the light of his competing interest. He did not; looking back, he admitted that "the issue of conflict of interest needed consideration."

That soon became evident. One day in late February 1979, as Felig noted in a memo for his own files, he got a call from Relman, who said he had learned that Soman and Felig had submitted an article elsewhere on the very same topic as Dr. Wachslicht-Rodbard's -after Felig had received and read her paper. Relman wanted to know whether Felig's review had been influenced by this conflict of interest and was concerned about the similarity of the papers. Felig assured him that his review

had been based on the paper's merits alone, and said that his and Soman's work had been completed before the Wachslicht-Rodbard paper had arrived.

Later that day, Felig got a call from Jesse Roth, whom he had known since they were boyhood friends and schoolmates in Brooklyn. According to Felig's memo, Roth told him what had been behind Relman's call: While Roth was away, Dr. Wachslicht-Rodbard had sent back her revised manuscript, along with a letter to Relman accusing Soman and Felig of plagiarism; she enclosed photocopies of her original manuscript and theirs with the parallel passages underlined. Felig said he could not account for this but assured Roth that his and Soman's work had been done quite independently. Roth said he had no doubt that that was the case; still, it would be well, he said, if he and Felig compared the two papers when they met in a few days at an N.I.H. conference in Bethesda.

When they did so, the plagiarism, though not extensive, was unmistakable. Roth added that Dr. Wachslicht-Rodbard had told him she now believed that the Soman-Felig paper might have been wholly fabricated from her own. In an attempt to settle the matter quickly and quietly, the two men worked out a plan to make amends for the wrong they now saw Soman had committed. Felig would insert in his and Soman's paper an indication that Dr. Wachslicht-Rodbard's work had come first; they would not publish their paper until hers had appeared; and, in fact, they would withhold it as long as any "reasonable doubt" remained about the independence of its research.

Felig phoned Dr. Wachslicht-Rodbard the next day, told her he was chagrined by what had happened and outlined his plan to put things right. She did not seem mollified. Helena Wachslicht-Rodbard, a shy, soft-spoken Brazilian, had been transformed into a feminine version of Dumas's vengeful Edmond Dantes. A couple of days later, Roth wrote a letter to his old friend outlining the agreed plan of redress and expressing his satisfaction with it; he asked Dr. Wachslicht-Rodbard to co-sign it, but, as he noted at the bottom of the letter, she refused. She remained unappeased even when her revised paper was scheduled for early publication in *The New England Journal of Medicine*; as she made clear to various colleagues, she felt that a serious wrong had been done by both Felig and Soman, that something should be done about it, and that her own superior was trying to shut her up. (She has told friends that Roth argued with her heatedly about the matter and threatened to dismiss her, and, according to a note in Felig's files, ordered her to stop using N.I.H. stationery and time to pursue her grievance.)

After the meeting with Roth, Felig returned to Yale and confronted Soman, who, abashed, admitted that he had kept a copy of the Wachslicht-Rodbard paper and borrowed a number of phrases from it, because, he said, he was not comfortable with the English language and felt under pressure to get his paper published soon. He also admitted that he had based his calculation on an equation in the Wachslicht-Rodbard paper. "I severely reprimanded him for this," Felig noted in a memo for his own files, adding that he had told Soman what steps he was taking to right the wrong, including assuring Dr. Wachslicht-Rodbard of priority. Aside from Soman's having naively used another's words, Felig still considered him perfectly trustworthy; Felig says he never doubted that the paper to which his name was joined reported work that Soman had done independently and scrupulously. "I had worked with him for years," Felig says, "and I fully believed that my colleague was an honest person. I accepted his explanation and did not feel that what he had done tarnished his entire character."

Helena Wachslicht-Rodbard, meanwhile, outraged that what she believed to be a piece of scientific fraud would never be exposed, wrote to Dean Berliner of the Yale School of Medicine in late March, expressing her doubt about the authenticity of the Soman-Felig work and asking for an audit. Dean Berliner's recollections and the files he made available to me are in accord: He asked Felig to verify that the studies had actually been done; Felig asked Soman for such verification, and Soman brought him a list of the patients' names and dates (but not their hospital charts), plus data sheets bearing figures that, he said, were averages compiled from the blood studies of the six patients. Felig looked no further; he told Dean Berliner that the work had been done as represented, and Berliner wrote to Dr. Wachslicht-Rodbard, "There is no question that the studies of Soman and Felig were done as described in their manuscript," adding that he hoped she would now consider the matter closed.

She would not. Soman and Felig were scheduled to present their work at a meeting of the American Federation of Clinical Research in May, and, as later reported in Science, Dr. Wachslicht-Rodbard hinted broadly to Roth that she might denounce them at that meeting unless something was done about her demand for an audit. Roth and Felig thereupon agreed to ask for an independent auditor to examine the evidence. Dr. Wachslicht-Rodbard held her peace, but shortly resigned from the N.I.H., perhaps out of disillusionment with the world of research, and took up a residency at a hospital in Washington.

Roth and Felig selected an auditor, but their designee never seemed able to find the time to visit Yale. Weeks and months passed, nothing happened, and at both the N.I.H. and Yale people seemed to think the problem would simply evaporate.

Felig had good reason to hope it would, for the greatest opportunity of his career had just materialized. A search committee at "P. & S." - Columbia University's prestigious College of Physicians and Surgeons - had recommended that Felig be appointed Samuel Bard Professor and chairman of the Department of Medicine, a key post in medical education in this country. The Wachslicht-Rodbard charges were still confidential and no one at P. & S. had heard about them. By now, in fact, Felig apparently felt so confident the storm had blown over that he took Soman to P. & S. in January 1980, introduced him to administrators and faculty there and recommended that he be appointed an assistant professor.

With uncanny timing, the challenged Soman-Felig paper appeared in The American Journal of Medicine that very month. Felig had promised to withhold it while any reasonable question remained about it; he felt that no such question remained. But Helena Wachslicht-Rodbard called Roth in such a fury that he agreed to act at once. He phoned Felig and said it was necessary to find another auditor; they decided on Jeffrey Flier of Harvard. Roth called Flier and asked him to go to Yale promptly and look for evidence that the data in the Soman-Felig paper came from work they had done and done properly. END PART TWO PICK UP PART THREE

A Typology of Scientific Fraud

Charles Babbage, an English mathematician, reflecting, in 1830, on what he saw as the decline of science at that time, distinguished among three major kinds of scientific fraud. He called the first "forging," by which he meant complete fabrication - the recording of observations that were never made. The second category he called "trimming"; this consists of manipulating the data to make them look better, or, as Babbage wrote, "in clipping off little bits here and there from those observations which differ most in excess from the mean and in sticking them on to those which are too small." His third category was data selection, which he called "cooking" - the choosing of those data that fitted the researcher's hypothesis and the discarding of those that did not. To this day, the serious discussion of scientific fraud has not improved on Babbage's typology.

Of the three types, forging is the most vulnerable to exposure; being based on wholly fictitious data, it is the most likely to look fake to the knowing eye, or to be shown up by attempts at replication. Trimming, in contrast, may long go undetected if the researcher reaches correct conclusions by illegitimate means, as is sometimes the case when a brilliant hypothesist, possessing only imperfect investigative techniques, gets some results that he or she feels sure need "correction." Cooking, similarly, may long go unnoticed, since what it yields, though only part of the truth, is genuine.

'You Do Know How Serious This Is?'

Soman was waiting on the station platform at New Haven; he drove Flier to his office in the Farnum Building on the medical-school campus, where he had laid out on his desk a number of hospital records, data sheets and notebooks. (Felig had planned to be there, but his mother had just died.) Soman, though affable, seemed to be somewhat nervous, Flier told me, and kept saying, with a giggle, things like, "Isn't it silly that you have to be bothered with this?" Flier, in an effort to ease the tension, diverted the conversation to the research each had been doing recently. "After half an hour," Flier recalls, "I thought we were ready, so I said, 'Well, Vijay, I guess we have to get on with this.' I asked to look first at the data on the individual patients, and we began to go through their hospital charts. There were only five of them, rather than six, and Soman didn't say why one was missing. But I could see that the five patients had all, as reported, been diagnosed as having

anorexia nervosa and had all gained significant amounts of weight in the course of treatment.

"Next, I asked Soman for evidence that insulin-binding studies had been done on these patients before and after treatment. He handed me a data sheet for the first patient. I was surprised. I had expected graphs for each patient, showing the data plotted out and curves drawn through the points, but what he gave me was just a sheet of raw numbers. 'Don't you have graphs?' I asked him. He seemed flustered, and said, 'Well, we threw away the individual graphs after a year because we had no storage space.' I started to feel uneasy. You don't throw away graphs with data that have just been published; it made no sense.

"So I studied that first sheet and visualized what the graph would look like, and it was clear, as their paper had reported, that there was more binding of the insulin when the patient was anorexic than after she gained weight. But it was also clear that the numbers on the data sheet did not at all conform to the kind of curve we always get in insulin-receptor studies - nor to the curve published in the Soman-Felig paper, which was said to represent the pooled findings on all six patients.

"I said, 'Vijay, it's funny that instead of having a sharp falloff, as in the published curve, this all looks very flat. It doesn't look like what you reported or what we'd normally expect.' He looked at the sheet and said, 'Gee, you're right. That must have been a bad one. Let's look at another one.' We did, but it was no better. One by one we went through the printouts, and all of them proved to be deficient in one way or another. Something was seriously wrong. There was no way that the beautiful composite curve they had in the paper could have been derived from the data I had been looking at."

Flier recalls that he then, with some difficulty, asked, "Vijay, what am I to think about this? It looks as if the published data don't conform to the data you're showing me." Soman, looking increasingly distressed, blamed the inaccuracies on a technician, but Flier asked, "Even if this was the fault of the technician, wouldn't you decide not to publish if the data weren't good?"

"Somewhere about then," Flier says, "I used the word 'fudged' for the first time: I said something like, 'Were the published data fudged? Were they made to look good?' Soman fumbled around and then said, basically, yes, they had been fudged; Felig hadn't known about it; nobody had. I also asked about some discordant data that seemed to have been suppressed, and he admitted to doing that, too." The paper had thus involved both trimming and cooking; the missing sixth patient, moreover, turned out not to have been anorexic but to have been labeled so for the purposes of the study, thereby constituting an instance of forging.

Flier says that he felt "pretty devastated" by Soman's admissions, but with some effort continued to play his role: "I said, 'You do know how serious this is?' and he said, 'Yes,' and started to defend himself. He said he'd been under great pressure to publish as soon as possible so as to obtain priority for the finding. He said that the laboratory he worked in was oriented toward productivity and success.

"The situation had begun to seem strange and unreal to me. Vijay grew more and more disordered and irrational in his thinking and started to say peculiar philosophical things. I felt troubled and disoriented, too."

The Self-Policing Structure of Science

Audits are seldom resorted to as a means of revealing scientific fraud because science, by its very nature, tends to expose fraud spontaneously. Far more important than such procedural safeguards as peer review or the audit is the nonduplicability of false science. As Dr. Harriet Zuckerman, chairman of the sociology department at Columbia University, has observed, scientists know that "the others working in their own and related lines of inquiry are all potential detectives. ... In the process of research, even long-accepted contributions periodically come under renewed scrutiny, not by design but as a byproduct of using them for further research." What cannot be reproduced by reported methods is suspected of being, at the least, error due to incompetence, and, at the worst, patent fraud.

This being apparent to every reasonable scientist, it is not surprising that the incidence of fraud is

still very low, despite the seeming rash of it in recent years. But Dr. Patricia Woolf, a research sociologist at Princeton University who is concerned with the publication of false science, told the Gore subcommittee that even a very low incidence can be seriously damaging in a number of ways. One is the spread of the misinformation, innocently restated by others in their own publications. Another is the waste of time, effort and money by honest scientists who base their work, in some part, on the fraudulent material. But perhaps the most severe effects are felt by the co-authors of the fraudulent research papers, whose resources also are wasted and whose careers may be blighted, according to Dr. Woolf, "by perhaps unjustified guilt by association."

Perhaps unjustified. Still, considering how costly false science can be to the scientific community, it is understandable that any suspicion of dishonesty, or even of tolerance of it in a colleague, may ruin a scientist's reputation - and, as Harriet Zuckerman says, "Research scientists have very little left if they lose their reputation for being trustworthy."

Investigation and Retraction

On Feb. 12, 1980, Dr. Samuel Thier, a compact, energetic man in his mid-40's who is chairman of the department of internal medicine at Yale, in which Felig worked, received an urgent telephone call. The emergency turned out to be an ethical one, not a medical one. Felig, as Thier recently told me, was on the line with very bad news: When Felig had asked Soman how the audit had gone, he had replied that Flier was prejudiced against the two of them and had found some things wrong. Felig immediately telephoned Flier, who told him there was no doubt that Soman had falsified data in the just-published anorexia nervosa paper co-signed by Felig, who realized that his reputation and, to some degree, that of the Yale Medical School itself were in peril.

Thier met Felig, and the two conferred with Dean Berliner, who softly said Soman had to go. Soman was called to Felig's office, and Thier conducted the short, painful proceeding. His recollection: "I said, 'Vijay, this and this is what's been presented to me. What's going on?' He looked very shaken and went through several denials that didn't hold up. Then he repeated what he'd told Phil about Flier's being prejudiced in some way. I said, 'Vijay, this doesn't make sense. There's no reason for this man to come here and make false statements about your work. Now what, in fact, happened?' Finally he said, 'I smoothed out the data. I took the curves and smoothed them out,' and he began to cry. It was dreadful."

Both of the senior men tried to extract from Soman something that would make sense of what he had done, but he could only mumble something about this being his fate. After a while, Soman asked, "What do I do now?" and Thier told him that the best choice was to resign and to give up research. Soman agreed to do so and to leave Yale within a few weeks.

Difficult as that had been, it was the easy part. The Soman-Felig paper would have to be retracted - a minor scandal in itself - and if some or all of the dozen or so other papers Soman had produced with Yale colleagues were also fraudulent, the scandal could become a major one. But not to examine them and make known any shortcomings that came to light would be far more scandalous yet. Felig, Thier and Berliner decided to impound all of Soman's records at once and to commission an extensive audit by Dr. Jerrold Olefsky, an endocrinology researcher at the University of Colorado, who agreed to come to Yale in two weeks.

Meanwhile, Felig tried to make some repairs. He wrote apologetic letters to Roth and to Dr. Wachslicht-Rodbard (she was not placated). Of far greater moment, since some part of the scandal was bound to come out, Felig knew he had to tell P. & S. about it at once. In late February, he went to P. & S. to conduct a seminar; afterward, he met with Donald Tapley, dean of the medical school, who had offered him the new post. Today, Felig and Tapley have somewhat different perceptions of what took place at that meeting. Felig says that he openly and plainly told Tapley about the audit and Soman's fudging of the data, adding that of course Soman now could not be considered for a P. & S. appointment. Tapley, a dignified, silver-haired man, says only, "Felig told me about it casually, in the course of discussing other things."

Late in March, Olefsky arrived, expecting to audit a total of 14 Soman papers, all but one of which had already been published. But Felig, in organizing the materials for him, says he was dismayed to find that most of what was needed was missing. He asked Soman where the materials were;

Soman replied that he had discarded many records and data books a year earlier.

Olefsky spent two days poring over the materials and making calculations from the data sheets for the five papers backed up by records. In his report to Dean Berliner, he wrote that he could not find insulin-binding-study data for all of the patients in any one paper; anywhere from a quarter to half the data were missing in every case. Most of the conclusions reached in the papers, he wrote, seemed "qualitatively accurate," but "quantitatively, the data are not precisely as indicated in the publications. ... One is left with the impression that there has been a general tendency to smooth the data up a bit." Of course, since so much material was missing even from these papers, the fraud might have been much more extensive than Olefsky could tell. Of the 14 papers by Soman and others, Olefsky was able to give a clean bill of health to only two. The rest either contained falsified data or were suspect because the original data had been destroyed; Felig was a co-author, along with others, of most of these papers.

Too many people were now privy to these developments for the affair to remain confidential. From March on, rumors were humming through the world of biomedical science. P. & S. was in turmoil; the air was full of stories about improprieties by the chairman-elect of the department of medicine and Samuel Bard Professor-to-be.

In May, Felig was asked by his senior advisory faculty committee at P. & S. to meet with them and clear the air. He did so, but apparently confined himself to Soman's falsifications and to the audits; at any rate, the faculty members present later attested during an inquiry that they heard nothing from Felig about the plagiarism issue or about his failure to discover in the course of a year what Flier had been able to find out in three hours. One member of the committee suggested that it would be wise to retract not only the fraudulent papers but those for which the data were missing, and Felig agreed. Shortly, letters began to go out to a number of journals, withdrawing 12 papers emanating from the Yale Medical School, eight of them co-signed by Philip Felig.

In June, Felig and his family moved to a sublet in New York, where he assumed his new post. Meanwhile, the rumors reaching P. & S. via the grapevine included all those darker aspects of the matter that Dean Tapley and Felig's advisory committee had not heard about. In July, when Paul Marks, then vice president of Columbia University (today he is president of Memorial Sloan-Kettering Cancer Center), returned from a vacation, he heard of the rumors that were troubling the faculty. The following day, he told me, he began to ask questions at the medical school and had someone call Berliner at Yale. Soon realizing that P. & S. was faced with a crisis, he got word to Tapley, who interrupted his vacation and returned home.

The Causes of Scientific Fraud

Those who are concerned about scientific fraud have offered a number of disparate conjectures to account for it. Perhaps the most common is that it is due to individual psychopathology. Ronald Lamont-Havens, for instance, chief of research at Massachusetts General Hospital, when testifying before the Gore subcommittee about the falsifying of cellular data at M.G.H. by Dr. John Long, said, "The rational individual - and I stress rational individual - knows that the deliberate falsification of data to fit a given hypothesis will inevitably, sooner or later, be found out by others. ... I feel, as you see, that Long is an extremely complex individual."

Other observers argue that psychopathology only makes fraud possible, not inevitable; other forces must conspire to induce such strongly deviant behavior. As Robert H. Ebert, former dean of the Harvard Medical School, has said, "It would be a mistake to consider (Long's fraud) an example of human frailty and nothing more." He pointed, instead, to the spirit of fierce competition fostered these days by medical schools and academic research centers for grant money with which to support their laboratories. Such competition has been intensified recently, it is often said, by the "grant squeeze." Inflation and cutbacks are threatening to end many programs, thereby increasing the pressure on scientists to make up impressive data for their papers and grant proposals.

But perhaps psychopathology and competition both play a part, within a changing milieu in which there is more room than formerly for fraud to get by, at least for a while. The flood of scientific articles has become so great that the peer-review system is faltering; according to one speaker at a recent meeting of the Council of Biology Editors in Boston: "It is now 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."

Moreover, as advanced research becomes ever more complex, it is increasingly often conducted by teams of specialists from different disciplines; much of the time, each contributes work the others take on faith. Senior authors, in particular, often join their names to papers reporting work they have directed without understanding the details of what their junior colleagues have done. The opportunity for fraud is increased by this practice - as is the risk of seeming to be a party to fraud when one is guilty only of the lust for publication. END PART THREE PICK UP PART FOUR

'Ethical Insensitivity ... Unacceptable Standards'

On Wednesday, July 23, 1980, Felig arrived at Dean Tapley's Columbia office bearing a dossier of 18 documents, including copies of the two audits; another dossier, containing 26 letters and memos, had arrived directly from Dean Berliner. In response to urgent calls from Tapley, everything Felig had not hitherto revealed was now being laid bare. Felig, aware that his position was in grave danger, explained the entire series of events to Tapley and the associate dean for academic affairs, Thomas Morris. Dean Tapley later remarked, "I thought he came off fairly well, considering that he had to tell such a ghastly story" - meaning, as Tapley said when I pressed him, that Felig had put on a good show, not that he had convinced him and Marks that he was blameless. Tapley, in fact, felt that Felig's reputation had been irremediably damaged. Paul Marks, for his part, was troubled by the fact that Felig had never told him much of what had only now come out - not even when, months earlier, Marks had dined at Felig's home.

After Felig left, Tapley called into his office six senior faculty members; he asked them to function as a special committee to study the Felig question and to recommend a course of action as soon as possible. That same afternoon, Felig met with three members of the committee. They said that they were gravely concerned about his lack of candor in dealing with P. & S., and, according to Felig, when he asked how he might now best proceed, the chairman of the committee, Dr. Henrik Bendixen delivered a crushing blow. Speaking for himself, he thought Felig should consider resigning. Felig began to fear that the outcome had already been decided.

The committee, however, went about its task with due care. Over a four-day period, the members spent some 10 hours reviewing the letters and documents, interrogating Dean Berliner and Felig and conferring with several Columbia officials, including counsel for the university. The committee's mood seemed to Felig to be epitomized by one member's remark that people in the field were saying, "Soman was forced to resign, but Felig got off scot-free."

By Friday, the committee produced a seven-page report reviewing the events of the past 19 months. The members concluded, among other things, that Felig had failed to communicate incriminating information about himself to Columbia; had been guilty of "poor judgment, if not negligence" in the way he handled the review of Dr. Wachslight-Rodbard's paper and in his failure to thoroughly review Soman's work after she had made her charges, and had used poor judgment in recommending Soman for an appointment at P. & S. The conclusion of the report was unsparing:

"The Committee must conclude that the events disclosed by the correspondence, and Felig's attitude when asked about these events, reflect ethical insensitivity and the application of unacceptable standards to scientific research." The committee concluded "with the deepest regret" that Felig should not retain his professorship and posts at Columbia.

That same day, Tapley asked Felig to come to his office, told him of the outcome and handed him a copy of the committee report. Felig, though his world had just collapsed around him, spent the weekend writing a nine-page letter to Michael Sovern, president of Columbia University, offering a point-by-point rebuttal of the committee report; Dean Berliner, simultaneously, wrote a scathing letter of rebuttal to Dean Tapley, excoriating him and the committee. But President Sovern did not intervene, Dean Tapley did not respond to Dean Berliner's letter, and on the following Tuesday, P. & S. and the Yale Medical School issued a joint press release announcing Felig's resignation because of "deep differences" between himself and officials at the Columbia Presbyterian Medical Center over the withdrawn scientific papers.

Then and now, more than a year later, people I have spoken to at P. & S., Yale, and elsewhere in the scientific world are sharply divided as to how Felig had been at fault. Berliner, Thier and other friends admit that some of his actions were unwise, but that essentially, as Thier puts it, "he was careless and spread too thin - and that's endemic in academic science today."

Those who are less sympathetic say that they cannot know what Felig really did, but that, at the least, he was guilty of a violation of trust, an attempted cover-up and an inordinate drive to publish; at the worst, he may have been guilty of a grossly unethical effort to beat out Dr. Wachslight-Rodbard and of the publication of an article hurriedly completed with faked data.

The people I interviewed at P. & S. say nothing about possible fraud on Felig's part; the moral issue, as Tapley sees it, is that the person who is the chairman of the department of medicine and Samuel Bard Professor has a direct shaping influence on every medical student who goes through P. & S., and that Philip Felig's several misjudgments and his failure to disclose the events with which he had been associated had "tainted him" and made him an imperfect role model. "The only way to deal with such behavior," Tapley says, "is to say you won't put up with it, which is what we did." Paul Marks says that "once the charges of plagiarism and fraud had been made against a work bearing his name, a truly responsible scientist would have gone to great lengths to make sure those charges were unfounded before letting that work be published. Judgment is the crucial quality someone must have in the position he had assumed at Columbia; that's the issue, and not whether he did or didn't fudge."

Felig himself feels it was a "mistake" not to investigate his junior colleague's work thoroughly, early in the affair, and a misjudgment to publish the paper before the audit. But he still sees nothing about his behavior to warrant the actions taken by Columbia; as he recently told me, "I can't make sense of it. I consider the actions of the people at Columbia so outrageous that I have never been able to explain them in my own mind."

The Public Perception of Scientific Fraud

While no one knows to what extent scientific fraud has increased of late, it is undisputed that a good deal more attention is being paid to it now than formerly. The recent episodes of disclosed fraud have been given considerable prominence not only in scientific periodicals but in media addressed to the general public, and committees of both houses of Congress are actively investigating the phenomenon.

The public, according to a number of observers within the scientific establishment, perceives scientific fraud as part of, and as indicative of, a general loss of virtue on the part of scientists, an attitude stemming from a growing fear of the consequences of scientific knowledge. Nuclear accidents and the potential of nuclear warfare are the most obvious sources of that fear, but perhaps more deep-seated is the alarm engendered in the public by the geneticengineering research that is now finding ways of modifying the forms of life itself.

The attention being given to scientific fraud is generating pressure for new safeguards against it. The N.I.H. is gingerly installing review procedures that will bar grants to those who are proved to have committed scientific fraud. The Food and Drug Administration has considerably increased the number of field inspections it makes of medical investigators testing new drugs. Certain academic laboratories have set up rules concerning coauthorship, designed to insure that no scientist puts his name on a paper unless he can fully explain everything in it. Social intervention in the form of Congressional pressure on grant-making agencies to penalize deviant scientists already exists at the level of discussion, with the possibility of concrete action in the future.

All this may portend a major change in the scientific ethos. As formal barriers and penalties are created and watchers are stationed, the behavior of scientists may come to be governed chiefly by the risk of exposure and punishment, a milieu in which one does no wrong - unless he thinks he can get away with it. What outcome the change in ethical control from a sense of right and wrong to a system of guardians and punishments might have on the communal quest for knowledge one cannot begin to foresee.

Aftermath

Vijay Soman disappeared from the Yale Medical School in April of last year, and by summer he and his family were back in Poona, India; nothing is known in American scientific circles of his life there today. All efforts to reach him have proved unavailing.

Helena Wachslicht-Rodbard is completing her residency in internal medicine and will shortly enter private practice; she has told acquaintances that it is unlikely that she will ever return to research.

The College of Physicians and Surgeons of Columbia University still has no permanent successor for the posts vacated by Philip Felig a year ago. As Henrik Bendixen told Science, "Search committees used to ask polite and gentlemanly questions. Now ... we're in the eyes of the public. ... Search committees will not rest until they have asked some very hard questions."

The Yale University School of Medicine, after a painful process of review that lasted three months, reappointed Felig as tenured professor last November, though it did not restore him to his endowed chair.

Felig spends most of his time in research these days and only a little in teaching. He is currently a participant, with several collaborators, in a number of research projects. Not having been found personally responsible for fraud, he has had his major N.I.H. grant renewed, and continues to hold others from the American Diabetes Association and the March of Dimes Foundation.

Nonetheless, his life has been much changed. As a friend says, "He has had a tremendous fall from grace. He's been stained." Prior to the events at P. & S., Felig had been offered nearly every important chairmanship of medicine in this country, but most people I talked to feel that it will be a long while before he is offered another good one, if ever. Felig recently characterized the happenings of last year as "catastrophic," but added, "If one believes in himself and has support systems, one goes forward. It wasn't like the loss, God forbid, of a loved one; it wasn't unsalvageable." Clearly, though, his career, whatever he manages to make of it, can never reach the heights it had been headed for. As Representative Gore said to him at the subcommittee hearings last March, "You obviously have been penalized severely for your involvement in this messy business with Dr. Soman." END STORY

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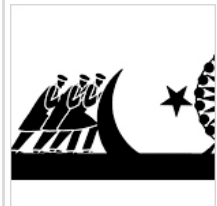
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