BETRAYERS of the TRUTH

Hedging, plagiarism, and outright fraud are typically considered rare ingredients in scientific research—research that by its very nature supposedly discourages and discovers perfidy. In fact, however, the exact opposite appears to be the case.

By William Broad and Nicholas Wade

The young congressman from Tennessee rapped his gavel, silencing the audience in the stately hearing room. "I cannot avoid the conclusion," he said, "that one reason for the persistence of this type of problem is the reluctance of people high in the science field to take these matters very seriously."

Fraud in scientific research was the problem of concern to Congressman Albert Gore. As a member of the House Committee on Science and Technology, Gore was troubled at the rash of serious cases that had recently come to light. As chairman of its investigations subcommittee, he was determined to do something about the problem. The hearings he held March 30 to April 1, 1981, marked the first time that Congress had inquired into the issue. Gore and his fellow representatives were moved to visible amazement and then anger at the attitudes of the senior scientists they had called as witnesses.

The first was Philip Handler, then-president of the National Academy of Sciences and leading spokesman for the scientific community. Instead of opening with the ritual profession of thanks for being asked to appear before the committee, Handler at once announced that it gave him "little pleasure and satisfaction" to testify on the subject of scientific fraud. The problem had been "grossly exaggerated" by the press, he said, clearly implying that the committee was wasting its time. Scientific fraud happens rarely, and, when it does, Handler declared, "it occurs in a system that operates in an effective, democratic, and self-correcting mode" that makes detection inevitable. His underlying message came over loud and clear: Fraud is a nonproblem, the existing mechanisms of science deal with it perfectly adequately, and Congress should mind its own business.

It was only a few weeks after Congressman Gore's hearings that yet another major instance of scientific fraud began to erupt, this time in the very heart of the American biomedical establishment, the Harvard Medical School. The case involved John Roland Darsee, the brilliant young protégé of Eugene Braunwald, physician-in-chief for two of Harvard's most prestigious hospitals and one of the country's leading cardiologists. Tall and affable, Darsee worked relentlessly at the cutting edge of cardiovascular research. In two years at Harvard, the young physician published nearly 100 papers and abstracts, a phenomenal number by any standards, and many of them jointly authored with his mentor, Braunwald. Braunwald, who presided over two separate research laboratories and more than $3 million in funds from the National Institutes of Health (NIH), was considering setting up a separate lab for Darsee at Harvard's Beth Israel Hospital. In the competitive world of Boston biomedical research, a promotion of this kind, at Darsee's young age, would ensure a dazzling career.

Darsee was less highly regarded, how

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even, by the other young researchers in Braunwald's laboratory. They could not understand how Darsee, hard though he worked, did all the research on which his prodigious number of scientific articles was based. Secretly observing him one evening in May 1981, they saw him flagrantly forge the raw data for an experiment that was shortly to be published. Darsee confessed when confronted, but insisted that was the only experiment he had ever faked. His colleagues were less inclined to agree. They told Braunwald they believed Darsee was systematically faking his work.

But Braunwald could not believe the troubling event was anything more than an isolated incident. "He clearly was one of the most outstanding, or the most outstanding, of the 130 research fellows I have been privileged to work with," Braunwald later said. "Public disclosure would have ruined him for life." So Darsee was stripped of his Harvard appointment but allowed to stay on in Braunwald's laboratory, doing research and publishing papers as if everything were just fine. Other researchers were not told of the faked experiment, nor were any steps taken to inform scientists who might be depending on Darsee's numerous published results that a question mark hung over the whole corpus.

Among the experiments Darsee worked on was a $724,154 project funded by the National Institutes of Health. In October 1981 the Harvard authorities were told by an official at NIH that there were problems with the data that Darsee had submitted. Only then did they begin to realize that a researcher who had forged one experiment might possibly be tempted to fabricate others.

A blue-ribbon committee appointed by the dean of the Harvard Medical School confirmed three months later that the study done for NIH contained "unusual results which are highly suspect." Furthermore, a study undertaken by Darsee with another researcher appeared "to have been manipulated." Nevertheless, the review committee, made up largely of senior medical officials, could see little wrong with the way their colleagues at Harvard had handled the affair, although a senior NIH official did chide Harvard on national television for the delay in reporting the incident. As this article goes to press, more than one year after Darsee was caught red-handed fabricating data, the extent of fraud in his published work has still not been assessed and publicly pronounced upon by the Harvard Medical School authorities.

A look at actual cases of fraud in science indicates that they are not perpetrated by psychopaths, but by individuals who have learned to manipulate the system.

When a new case of scientific fraud breaks into the headlines, the scientific establishment generally responds with one variant or another of the "bad apple" theory. The faker was a psychopath, or under great stress, or otherwise mentally disturbed, this theory goes. Its unspoken implication is that all blame should be put on the erring individual, not on the institution of science. Philip Handler told the Gore congressional hearing: "One can only judge the rare such acts that have come to light as psychopathic behavior originating in minds that have made very bad judgments–ethics aside–minds which in at least this one regard may be considered deranged."

If every smidgen of fraud can be laid at the door of the poor, unhinged, deranged psychopaths who nevertheless managed somehow to infiltrate the research community, clearly there is no need for any change in the institutional mechanisms whereby science is said to police itself. This is an important rationalization because scientists hold firmly to the notion that their discipline is self-correcting, and to acknowledge that fraud is more than an occasional fluke would be to admit the failure of the underlying system.

In fact, a look at actual cases of fraud in science indicates that, while they are sometimes revealed by the internal checks and balances of the discipline, they are perpetrated not by psychopaths but by individuals who have learned to manipulate the system or to prey on the vanity, credulity, or character weaknesses of colleagues. In many cases, the internal checks and balances system–peer review, referees, replication of experiments–helps unmask the deceit, but it does not prevent the fraud. Furthermore, in most instances, the deceit is denounced, if at all, only after extensive and persistent effort on the part of individual scientists.

But still, scientists remain overwhelmingly, even stubbornly, convinced of the objectivity of their discipline and confident of the effectiveness of the institutional mechanisms by which that objectivity is assured. What then are those mechanisms, and how efficiently do they actually work? While some hold that the individual scientist's devotion to the truth is what keeps science honest, the prevailing view attributes honesty in science to institutional mechanisms, not the personal virtue of scientists. According to Robert Merton, the leading American sociologist of science, it is the verifiability of results, the exacting scrutiny of fellow experts, the subjection of scientists' activities to "rigorous policing, to a degree perhaps unparalled in any other field of activity" that ensures "the virtual absence of fraud in the annals of science."

The three principal mechanisms that constitute the alleged self-policing system of science are peer review, the referee system, and replication of experiments. Peer review is the name given to the practice of asking committees of specialists to recommend which scientists should be supported and which should be denied government funds for research. Peer-review committee members are supposed to scrutinize each of the very detailed applications for funds and rate each according to its scientific merits. This process is the first stage at which any fraudulent research proposal might be caught.

The second safety net against fraud is the referee system, the practice whereby almost all scientific journals ask expert authorities to judge the merits and novelty of articles submitted to them for possible publication, and to spot any defects in argument or technique. Since this is the most rigorous test an article may undergo, it is a prime point at which fraud or self-deception should be caught.

The last and seemingly most formidable defense against fraud is replication. In publishing his findings, a scientist is supposed to describe exactly how he did his experiment, so that others can repeat it and either confirm or refute the result. According to the established wisdom, any fraudulent experiment will be shown up when others try to replicate it. The more important the false claim, the more quickly will attempted replication by others bring it down.

This is the way science should, and in part does, work. Scientists are, by and large, so powerfully wedded to this ideology that they find it hard to see significance in any deviations from it. Yet the ideology is an imperfect description of how science works in practice. Where the conventional ideology goes most seriously astray is in focusing on the process of science instead of on the motives and needs of scientists. Scientists are not different from other people. In donning the white coat at the laboratory door, they do not step aside from the passions, ambitions, and failings that animate those in other walks of life.

Modern science is a career. To be successful, a researcher must secure government grants, build up a laboratory and the resources to hire graduate students, strive to be awarded a tenured post at a university, publish as many articles as possible that may come to the notice of committees that award scientific prizes, gain election to the National Academy of Sciences, and hope to one day win an invitation to the Nobel Prize presentation in Stockholm.

Not only do careerist pressures exist in contemporary science, but the system rewards the appearance of success as well as genuine achievement. Universities may award tenure simply on the quantity of a researcher's publications, without considering their quality. A laboratory chief who has skillful younger scientists working for him will be rewarded for their efforts as if they were his own. Such misallocations of credit may not be the norm, but they are common enough to encourage a certain evident cynicism. It is in the climate of cynicism that a scientist may first consider the previously unthinkable—embellishing the research results he reports.

The term "scientific fraud" is often assumed to mean the wholesale invention of data. But this is almost certainly the rarest kind of fabrication. Those who falsify scientific data probably start and succeed with the much lesser crime of improving upon existing results. Minor and seemingly trivial instances of data manipulation—such as making results appear just a little crisper or more definitive than they really are; or selecting just the "best" data for publication and ignoring those that don't fit the case—are probably far from unusual in science. But there is a difference only in degree between "cooking" the data and inventing a whole experiment out of thin air.

The study of fraud sheds light on how all scientists behave; nevertheless, its incidence appears to be somewhat less in the "hard" sciences, *i.e.*, those such as physics, which have a high mathematical content. The tight logical structure of mathematics virtually precludes falsification, so that highly mathematized sciences possess a certain built-in protection against fraud. In the spectrum that runs from hard sciences to soft sciences, from physics to sociology, the center is probably occupied by biology, a discipline in which fraud is by no means rare. Biology and medicine are also the disciplines in which fraud is likely to affect the public welfare most directly. Consider, for example, the remarkable tale of master plagiarizer Elias Alsabti, and the extraordinary episode of Mark Spector and the kinase cascade, incidents that dramatically illustrate the weaknesses and all-too-human frailties of the internal mechanisms by which scientific honesty is supposedly guaranteed.

At first glance it appeared that Elias A. K. Alsabti, M.B., Ch.B. (Bachelor of Medicine and Bachelor of Surgery), had everything he wanted in life except a scientific career. He had money, power, and a quick mind. He claimed he was a blood relation of the Jordanian royal family. To those who worked with him, it seemed Allah had smiled on this 23-year-old physician who came to the United States in 1977 for a postgraduate medical education, an education paid for by His Royal Highness Crown Prince Hassan, brother of King Hussein of Jordan. In addition to his good fortune, Alsabti worked hard at whatever he did, and rose rapidly into the higher reaches of academia. While gaining a Ph.D. in cancer immunology and membership in eleven scientific societies, Alsabti worked at one U.S. institution after another, including the world-renowned M. D. Anderson Hospital and Tumor Institute in Houston, Texas. He published more than 60 papers. The address listed on many of these papers was the Royal Scientific Society in Amman, Jordan, and Alsabti intimated to a few colleagues in America that on return to Jordan he would be named director of a prestigious cancer institute. In the meantime, he drove to and from work in a yellow Cadillac.

Unfortunately, at the heart of this glamorous story of wealth, accomplishment, and prestige, is a significant flaw. Alsabti had not only fabricated his degrees, but also plagiarized the papers on which his impressive medical career was built. Alsabti's goal, like that of many scientists, was to further his career by compiling a long list of published papers, the scientific article being the basic coin of career advancement. During the three years when his scheme flourished, Alsabti was a veritable factory for the production of papers. Each month saw another group of articles by him appear in various journals around the world. His method was simplicity itself. He would retype an already published paper, remove the author's name, substitute his own, and send the manuscript off to an obscure journal for publication. His tactics deceived the editors of dozens of scientific journals. Alsabti papers were published in the *Journal of Cancer Research and Clinical Oncology* (U.S.); *Japanese Journal of Experimental Medicine; Neoplasma* (Czechoslovakia); *European Surgical Research* (Switzerland); *Oncology* (Switzerland); *Urologia Internationalis* (Switzerland); *Journal of Clinical Hematology and Oncology* (U.S.); *Tumor Research* (Japan); *Journal of Surgical Oncology* (U.S.); *Gynecologic Oncology* (U.S.); *British Journal of Urology;* and *Japanese Journal of Medical Science and Biology.*

In one instance, happening upon an unpublished paper by a University of Kansas Ph.D. candidate, Daniel Wierda, Alsabti made a few cosmetic changes, added his name and the names of two fictitious coauthors, Omar Naser Ghalib and Mohammed Hamid Salem, and sent it to the *Japanese Journal of Medical Science and Biology* where it was published before the original was even in print. Details of this plagiarism show Alsabti's method, and why following his trail through the literature is difficult at best. The text in each paper is nearly identical. But Wierda had entitled his paper "Suppression of spleen lymphocyte mitogenesis in mice injected with platinum compounds," whereas Alsabti had changed the title to "Effect of platinum compounds on murine lymphocyte mitogenesis." It is clear that a computer search based on Alsabti's titles would not necessarily reveal the authors from whom he had lifted material. Moreover, the obscurity of most of the journals virtually ensured that all of the cases of plagiarism would not be tracked down. The

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authors whose work was stolen would most likely never read the pirated material and there the matter would rest.

In the course of constructing his academic illusion, Alsabti forged his medical degrees, duped the Jordanian government into giving him tens of thousands of dollars, fabricated his relation to the royal family, lied his way into U.S. universities, bestowed a Ph.D. upon himself, and, while allegedly doing research in a handful of prestigious U.S. labs, had pirated many, perhaps all, of his 60 published papers. In addition, his lies and legerdemain took in the governments of two Middle Eastern countries, the review committees of eleven scientific societies, and administrators from six U.S. institutions of higher education. In the end, after three years of plagiary, it was his brash manner, his cavalier theft of whole papers word for word, that brought his downfall.

By 1980, a tempest was brewing among the researchers whose work had been lifted by Alsabti. Wierda, the Ph.D. candidate whose paper had been pirated, wrote to the news sections of several scientific journals, and articles describing Alsabti's exploits appeared in *Science* and *Nature*. In July 1980, the *British Medical Journal* recounted two additional cases where the published articles of reputable researchers had been appropriated by Alsabti. Under the title "Must Plagiarism Thrive?" the article speculated on the possibility of preventing such theft:

> "There are at least 8,000 medical journals in the world, and many of these receive thousands of papers a year. Checking credentials of authors would be a vast and embarrassing business. And checking to see if a paper has been published before (under a different name and probably with a different title) would be nigh on impossible. Editors would seem to have little choice but to trust to the integrity of their contributors and the astuteness of their referees."

As the Alsabti case shows, the editorial boards of scientific journals do not provide an effective check against deliberate fraud. Furthermore, the sheer number of journals published makes it virtually impossible for anyone to be familiar with all of the literature in a given field and greatly increases the opportunity for spurious or plagiarized research to be published.

There are several reasons for the increase in the number of scientific publications: one is the tremendous increase in the ranks of scientists themselves; it has been estimated that 90 percent of all scientists who have ever lived are alive today. Another is the comparatively new emphasis on a scientist's bibliography as a criterion for securing government grants and academic promotions. As recently as two decades ago, the inflated bibliography was unknown. In 1958 when James D. Watson, then a young biochemist, later a Nobel Prize winner, earned the rank of associate professor at Harvard, he had just 18 papers on his curriculum vitae. One of them, coauthored with Francis H. Crick, described the structure of DNA, the master molecule in all living things. Today, the bibliography of a candidate in a similar situation often lists 50 or even 60 papers. Since few scientists, much less administrators, have time to read these articles, the quantity of scientific papers listed on curricula vitae can often be more important than their quality.

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A safe generalization is that many of the scientists and much of what they publish today are mediocre at best. As the sociologists Jonathan and Stephen Cole noted in a trenchant analysis of scientific productivity published in 1972, about half of all scientific papers are never cited in the year after they are published. Since scientists are supposed to cite all papers on which their own work depends, for an article never to be cited means that it probably has had no influence on any other scientist's work, and hence no impact on the progress of science as a whole.

The epitome of mindless publication is the case of Alsabti. According to Stephen M. Lawani, a library science graduate, not one of Alsabti's articles had been cited by another scientist prior to his exposure as a grand purloiner. Alsabti stole insignificant research, and thereby avoided immediate detection. But the list of papers he compiled still gave him instant entrée into the higher reaches of U.S. academia.

Alsabti resorted to plagiarism in order to pad his bibliography with mediocre papers. Other researchers attain the same goal by a variety of other methods. There is, for example, the Least Publishable Unit, or LPU, a euphemism for getting as many separate publications as possible out of a single piece of scientific work. Instead of publishing one comprehensive paper that ties work together, a researcher will publish four or five short ones.

Another way to get more for less in the publishing game is via coauthorship. Editors at the *New England Journal of Medicine* say coauthorships have risen exponentially since the journal's inception, today averaging about five authors per paper. The Philadelphia-based Institute for Scientific Information, which indexes 2,800 journals, says the average number of authors per paper rose from 1.76 to 2.58 between 1960 and 1980. And that is the *average*. It is not uncommon to see a paper with a dozen or more coauthors. Some of this rise is related to the increasing mix of different subspecialists who work on a single research problem. But much is strictly related to career advancement and the gratuitous addition of coauthors by a researcher trying to curry favor.

The scientific paper was at one time a vehicle for the transmission of scientific truth and for speculation on the workings of nature; today its importance has been diminished as it more and more has become a tool of the careerist. Of course, converting LPU's into articles and signing on unnecessary coauthors are not in themselves fraudulent, but they can be easily abused by scientists more interested in the pursuit of fame than in the search for truth.

The meteoric career of Alsabti ran roughshod over all three barriers to fraud—peer review, referee checks, and replication—but his might seem at first to be a special case. It was not Alsabti's practice to endure the tedium of doing experiments, so he never needed to apply for a grant through the peer review system. Since his articles were copied, not invented, there was nothing inherently false for referees to discern. Besides, Alsabti was operating on the periphery of the scientific mainstream, publishing in obscure journals on backwater subjects. A severer test of the replication system would be a fraudulent experiment at the cutting edge of a frontier of science, one that made so important a claim as to attract the attention of all the leading researchers in the field. Enter Mark Spector and the kinase cascade.

In the spring of 1981 a new superstar burst into cancer research and seemed to shed a marvelously illuminating light over the intractable field. Mark Spector, a 24-year-old graduate student at Cornell University, and his professor, Efraim Racker, announced a remarkable new theory of cancer causation. The theory, for which Spector had provided all the prodigious experimental evidence, was of such strength and elegance that many were convinced it would win him and his professor the Nobel Prize.

With a glowing recommendation from his professor at the University of Cincinnati, Mark Spector arrived at Cornell University in January 1980 and began working as a graduate student in Racker's laboratory, where his success with tricky experiments quickly earned him a reputation as a prodigy with "golden hands." Racker gave him the task of purifying an enzyme known as sodium-potassium ATPase, which is part of the wall of living cells. Racker had reason to believe that an inefficiency in the enzyme's operation is one of the distinctive marks of certain cancer cells. Several people had tried and failed to purify the form of the enzyme found in cancer cells, but Spector did so in two months. Next, he found evidence to show that the ATPase operated inefficiently in cancer cells, but efficiently in normal cells, a discovery that dramatically confirmed his professor's predictions.

Spector quickly ascertained the reason for the enzyme's inefficiency: in cancer cells, the enzyme undergoes a chemical modification known as phosphorylation. Each chemical change in a cell is mediated by a particular enzyme, so the next step in Spector's trail was to find the enzyme that causes the phosphorylation of the ATPase. The second enzyme, called a protein kinase, turned out to be present in all cells, Spector reported, but it assumed an active form only in cancer cells. The young genius capped this electrifying discovery by bringing to light a series of four different protein kinases. Like a row of dominoes, each kinase phosphorylated, and thereby activated, the next kinase in the cascade, with the last kinase phosphorylating the ATPase.

It is usually a year's work for a graduate student to purify a single enzyme, especially if it is a minor one. But by mid-1980, six months after his arrival in Racker's lab, Spector had purified the ATPase and the four kinases. The kinase cascade was a wonderfully interesting mechanism, suggestive to biochemists of all kinds of signal amplification and control systems. But there was something even better to come. Spector managed to tie the cascade to an extremely important new development in the study of viruses that cause tumors in animals.

The tumor-causing gene of these viruses, known as the SRC "sarc" gene, is one that specifies a protein kinase enzyme. The

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viruses are thought to have pirated this gene, early in their evolution, from the cells of the species they infect. Cancer researchers had scrutinized animal cells for the present-day versions of these pirated genes, the so-called endogenous SRC genes, but no one had managed to isolate the genes' protein kinase products. No one until Mark Spector, with the astonishing news that certain of his cascade kinases were the products of the elusive endogenous SRC genes.

At last a unified theory of cancer causation seemed possible. A tumor virus infects a cell. Its SRC gene makes unmanageably large quantities of a kinase that trips off the cell's otherwise inactive cascade of kinases. The last kinase in the cascade phosphorylates the ATPase enzyme, making it inefficient and thereby setting off the further changes characteristic of malignant cells.

"Seductive" is the word biologists used to describe the compelling intellectual attraction of the Racker-Spector theory. The two of them had picked the most exciting new developments in cancer research and demonstrated, by a sequence of beautifully executed experiments, how each fitted into the overall theory. Before the details had even been published in the scientific literature, Racker started mentioning the theory in lectures given around the country.

A biochemist with a background in psychiatry, the 68-year-old Racker was an eminent figure—a winner of the U.S. National Medal of Science—and his authority gave the then-unpublished theory a credence it would not otherwise have had. Soon, under Racker's aegis, Spector was collaborating with such leading researchers in cancer biology as David Baltimore of MIT and George Todaro and Robert Gallo of the National Cancer Institute. When Racker gave a lecture about the theory on the campus of the National Institutes of Health in the spring of 1981, some 2,000 people attended.

Leading researchers began to move into the field, but rather than go through the laborious task of replicating Spector's work by purifying their own kinase systems, they would send their reagents to him for testing. "The striking thing was that when you went there, there were samples from all over the world waiting to be tested by this kid," said Todaro. "If you looked at the labels on the shelves it was almost a Who's Who of cancer research." Some researchers invited the young graduate student to their labs. One by one they became aware of a pattern familiar to Spector's colleagues at Cornell: often the experiments would only work in Spector's hands and could not be repeated without him. But, like Spector's colleagues, they found a simple explanation: Mark was just so good at making experiments go.

Among those intrigued by Spector's theory was Volker Vogt, a tumor virologist who worked in biochemistry at Cornell on the floor above Racker's laboratory. In April 1980 Spector performed some experiments on his ATPase enzyme with a student of Vogt's, Blake Pepinsky. "These results were so clean and beautiful and convincing that I was seduced into putting my time in on this project," says Vogt.

There was one problem. Sometimes the experiments worked and sometimes they didn't. Vogt was worried that results so beautiful should also be so erratic. He devoted the summer of 1980 to trying to understand why the negative experiments didn't work. Whatever the reason, whether the wrong phase of the moon or having impurities in the distilled water, it was too elusive for Vogt to put his finger on. Eventually he gave up. He also decided he could not publish the experiments, however exciting they might be.

Pepinsky, however, continued to help Spector, and a year later, in early 1981, Vogt too was drawn back into the maelstrom when Spector started to find his kinases in cells infected with tumor-causing viruses. The particular experiment that caught Vogt's interest was one showing that an antiserum to one of Spector's kinases also had an affinity for an important but so far undetected protein, the product of the SRC gene of a widely studied mouse-tumor virus.

Pepinsky repeated the experiment twice but it didn't work. Vogt despaired that it seemed to be the same frustrating story as with the ATPase the year before. But this time, he told himself, he was really going to get to the bottom of things. In an intensive two-day effort, Pepinsky and Spector redid the experiment.

It was another spectacular success. "There were fat radioactive bands of protein on the autoradiogram, everything looked as clean as could be," says Vogt. "So I said, 'Here at least is something I can work with.' " He decided his first step would be to analyze the gels from which the autoradiogram was made. "Mark was very upset that I had got my hands on the gels. Previously he had done all these analyses himself," says Vogt.

The gels were the key piece of data from Spector's experiments. Cell proteins picked out by antisera and tagged with radioactive phosphorus-32 would be placed on the gel and subjected to an electric field. Each protein would migrate through the gel a particular distance, determined by its size, and mark its presence by darkening a radiosensitive film placed next to the gel.

Hitherto only these films, called the autoradiograms, had been shown by Spector to his colleagues. When Vogt obtained an original gel, his first step was to run a hand Geiger-counter over it to locate the radioactive protein bands. He realized instantly from the pattern of clicks that something was terribly wrong. The clicks were not saying phosphorus-32. Judging from the amount of darkening on the autoradiogram, they seemed to be saying iodine-125. A scintillation-counter measurement confirmed this diagnosis. But iodine had no business at all in the experiment.

It was forgery, very cunning but quite simple. The forger was evidently finding proteins of the right molecular weight to reach the desired point in the gel, tagging them with radioactive iodine, and mixing them into the antisera-tagged proteins just before they were put onto the gel. Vogt was overwhelmed.

"I knew it was a big event in my career, in everyone's. I went home and brooded over it for one day," he recalls. "Then I went over to see Racker. He didn't doubt the actual facts but he was loath to believe that everything was wrong immediately. At that point we thought it might be a recent aberration."

Anticipating that he would accept responsibility, Vogt and Racker confronted Spector. To their surprise he instead claimed he didn't do it and that he didn't know how it happened. Racker gave Spector four weeks in which to purify the ATPase enzyme and the four kinases from scratch and to put them in Racker's hands for testing. Spector agreed, saying the task would take him two weeks, not four. In the end Spector's attempt to vindicate himself was a failure, yet an ambiguous failure. He showed Racker that he could reproduce some, but not all, of his claimed results, thus creating uncertainty as to whether some or none of his previous work was reliable. At the end of four weeks Racker told Spector not to come back to his laboratory.

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Given the basic premises and methodology of science, why wasn't the falsity of Spector's results discovered much earlier? Why hadn't the many biologists caught up in his theory tried to replicate some of the basic results? Apparently they had.

The first clear danger signal from the outside came from Raymond Erickson, of the University of Colorado, a leading expert on the virus SRC gene problem. Racker asked him to test an antiserum Spector had prepared. As a favor, Erickson did so, and found the antiserum was not what Spector had represented it to be. Erickson informed Racker of his findings in November 1980, almost a year before the scandal finally broke. Evidently Racker didn't believe Erickson, and in an article in *Cell*, later retracted, even attacked Erickson for failing to properly recognize the antiserum.

Another clear warning signal came from Robert Gallo of the National Cancer Institute. He sent a monkey virus protein to Cornell in February 1981 and was told by Spector that it was related to one of the cascade kinases. After numerous attempts to repeat the experiment in his own laboratory, Gallo finally sent one of his postdoctoral students to Cornell to perform a joint experiment. Because he was suspicious of Spector's remarkable results, Gallo instructed the student to code the samples given to Spector rather than identify them by name. Yet, despite the coding, Spector twice picked the correct one out of nine samples in demonstrating a relationship between one of his cascade kinases and the gene product of Gallo's monkey virus.

Gallo also asked Racker to send him the reagents so he could repeat the experiment in his own laboratory, but the reagents never arrived. "So I just quit the project. I thought there was something funny but I didn't know what, and I didn't want to get too speculative. I just told Racker we couldn't repeat it here," says Gallo. Asked why he didn't also mention to Racker his belief that something was amiss, Gallo replies, "If you can't repeat something, if you don't have the reagents, what can you say? A lot of times people can't replicate things, and it's because they're doing it wrong."

Ironically, as the kinase cascade theory started to fall apart, so did significant parts of Mark Spector's background. On September 9, 1981, he withdrew the thesis that was about to win him a Ph.D. degree earned in one and a half years instead of the usual five. Belated checks that should have been done when Spector entered the Cornell graduate school showed that he possessed neither an M.A. nor a B.A. degree from the University of Cincinnati, as he had claimed. "A check with law enforcement agencies in Cincinnati," reported the *Ithaca Journal*, "shows that Mark B. Spector pleaded guilty on June 12, 1980, to two forgery charges in connection with writing two checks for $4,843.49 to himself from his employer. . . . He was sentenced to prison and the sentence was suspended and he was put on three year's probation."

Had it not been for Volker Vogt, the fraudulent nature of Mark Spector's cancer research would probably not have been detected until much later, and it might well have escaped attention altogether. Spector was about to leave Cornell and set up a laboratory of his own. In that circumstance, it is hardly likely that others would have gained access to any iodine-spiked gels. Spector's lack of B.A. and M.A. degrees would probably have come to light through bureaucratic mechanisms before he could receive his Ph.D., yet without the evidence of fraud he might well have stepped across this little obstacle.

As for Racker, what should have been the crown of a distinguished career ended in fiasco. Elderly, brusque, authoritarian, Racker was so impressed by his young protégé that he was arranging for Spector to take over part of his laboratory. "I treated him like the son I never had," Racker told a colleague. For Racker, considered by his peers to have long been a candidate for the Nobel Prize, the kinase cascade was probably his last chance to win recognition in Stockholm.

"I think Racker very much wanted to believe," says a researcher who followed the case closely. "He is a very well respected scientist, but I think to some extent he might have suspended a little of his critical judgment. Because he had a lot of faith in Spector, things were not checked as carefully as they could have been."

"I think we did all reasonable checks," responds Racker. "It is unfortunate that this should happen to me because I am so well known for checking things." His point is a perfect illustration of the fact that even the most critical of scientists can let critical faculties be lulled when more powerful motivations come into play. A brilliant kid with golden hands, who took one's most promising theories and clothed them in the appearance of reality, who built a shimmering castle of dreams that mesmerized every cancer researcher in the country—few scientists could be sure of their immunity to such a Siren call.

Pride, ambition, excitement at a new theory, reluctance to listen to bad news, unwillingness to distrust a colleague—these are the ingredients that caused the kinase cascade theory to get so far. The point is not that these are dishonorable feelings—they are not—but that for a long time they prevented the institutional mechanism of replication from being put into effect. As the Spector case so clearly illustrates, it is not by public replication but by private verification that fraud is most often brought to light.

Scientists, like other highly trained professionals, must proceed with a basic confidence in the integrity of their colleagues; this assumption naturally makes them reluctant to directly challenge another's research by attempting to replicate it. Beyond this, exact replication of an experiment is an impractical undertaking for several reasons. First, published descriptions of an experiment are often incomplete, not in their major conceptual elements but in little details of practical technique. Just as cookbook recipes omit tiny points that every cook knows, so do scientists in describing their experiments. But these details are often crucial to a successful outcome. They also may be unknown, despite the author's assumptions to the contrary, to all but an intimate network of researchers. Second, repeating an experiment often requires a major investment of money to purchase equipment and of time to master a technique and to acquire, for example, special cells or reagents. Finally, there is no credit to be won in replicating and validating someone else's experiment except in unusual circumstances. The prizes go for originality; being second wins nothing. In general it is only indirectly, in the course of trying to build upon another's work, that scientists confirm it.

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Clearly, replication is not an essential ingredient in the cookbook of academic science. Certainly it is added for flavoring occasionally, but that is about all. How much erroneous or fraudulent science might be revealed if replication were regularly practiced, if self-policing were more than an imaginary mechanism? The question can be answered indirectly by examining biological testing, an arena of scientific inquiry in which a small measure of external policing exists.

Each year the Food and Drug Administration (FDA) and the Environmental Protection Agency (EPA) receive from industry thousands of test results on the safety of new foods, drugs, and pesticides. After reviewing the data, government officials can send inspectors to examine the records of the physician or laboratory if they find cause to doubt the validity of the tests. Even the limited police powers of the FDA and the EPA detect a disturbing number of falsified test results, as the following sample of cases from the past decade reveals:

• *The Rabbit Ebenezer Case.* A Scientific Investigations Group within the FDA discovered that 16 out of 50 physicians audited between 1967 and 1973 had submitted false data on drugs to the sponsoring companies and the government. One physician had submitted slides of liver sections from the animals in his study; the sections were all from a single liver. Similar economy was attained by a researcher who had performed all his animal trials on a single rabbit who went by the name of Ebenezer.

• *The Andrea Doria Phenomenon.* FDA inspectors find raw data are so accident-prone that they call it the Andrea Doria Phenomenon. At hearings held before Senator Edward Kennedy in October 1979, FDA officials related the tale of "Dr. 31," who had furnished identical data to two companies on two different drugs. Asked for his records, Dr. 31 explained that he was such a compulsive worker that he had taken all the original data with him on a picnic. They had been lost when the rowboat he was in capsized. The appeal of this charming story diminished when the inspectors learned that Dr. 31 had tried to persuade a nurse to state that she had been in the rowboat when the accident occurred.

• *Magic Pencil Research.* In 1975 an FDA official accidentally uncovered a file on Naprosyn, an antiarthritic drug tested by a company called Industrial Bio-Test (IBT). The file revealed a number of horrors: Rats were recorded on the data sheets as having died twice; weights of some continued to be logged in after they had been listed as dead; lab technicians jotted observations on their coat sleeves and sometimes neglected to autopsy dead rats until decay rendered the task moot. Technicians nicknamed one experiment the "magic pencil study" because the final report contained analyses that had never been performed.

Based on evidence in the file, the president of IBT and three other senior officials were indicted on charges of falsifying test results. The officials were accused of faking data on four animal studies between 1970 and 1977. The indictment alleged that they had concealed the fact that TCC, an agent in many deodorant soaps, caused the testes of mice to degenerate even at the lowest test doses. They were charged with fabricating blood and urine studies on Naprosyn and inventing cancer study data on an insecticide and a herbicide.

Unfortunately, the four studies cited in the indictment represented only a fraction of the debacle. Industrial Bio-Test had been one of the country's largest independent testing labs, responsible for checking the safety and effectiveness of more than 600 chemicals, drugs, and food additives. Substances approved on the basis of IBT-conducted tests are found in consumer products ranging from garden pesticides to ice-cream dyes, jellies, fruit drinks, contact lenses, and household bleaches. Most of these tests are probably invalid. An audit by the EPA has showed that 100 percent of IBT's long-term rat studies and a majority of its other tests are unreliable.

In response to the widescale fraud discovered at IBT and other laboratories, the FDA has drawn up regulations for good laboratory practice, and standards throughout the testing industry are being tightened. But cases of blatant fraud continue. Of the nation's estimated 12,000 clinical investigators, "perhaps as many as ten percent do something less than [honest research]," FDA officials said in 1980.

Beyond failures in the basic checks and balances of the discipline, scientific fraud is made possible—even encouraged, some would say—by abuses in the master-apprentice system whereby young scientists are trained to become full-fledged members of the scientific community. Today it is not unusual for the name of a prominent scientist to appear on many hundreds of papers. Frequently these prodigious numbers are not the result of great creative energy and tireless devotion to the truth, but rather of the deft exploitation of the lab-chief system. Such a quantity of publication is often the result of graduates and postdocs working long hours in the lab, producing reports and papers to which the lab chief will graciously add his name. Given the hierarchical limbo graduate students inhabit, the credit often is attributed to the lab chief. In such an environment, scientific truth can become almost an accidental by-product. The laboratory can more accurately be considered a research mill, a factory for the mass-production of scientific articles.

As the young scientist is pushed to publish papers in which he will appear as a minor light in the constellation of a senior scientist, the temptation to cut corners, to improve on the results, or even to fake data entirely, is often difficult to resist. The temptation is probably strongest where the workers have no stake in publication, which is a chief intellectual reward of research. They work, but their names do not appear on the published papers.

According to sociologist Julius A. Roth, "Even those who start out with the notion that this is an important piece of work which they must do right will succumb to the hired-hand mentality when they realize that their suggestions and criticisms are ignored, that their assignment does not allow for any imagination or creativity, that they will receive no credit for the final product, . . . When this realization has sunk in, they will no longer bother to be careful or accurate or precise. They will cut corners to save time and energy. They will fake parts of their reporting."

The disillusion and stress that develop in subordinates who are under pressure to produce is illustrated by the celebrated case of William T. Summerlin.

Despite the public success, and lectures given by Summerlin, other researchers were increasingly skeptical as they tried and failed to repeat his skin-transplant work.

In 1974 Summerlin was a junior colleague of the respected immunologist Robert A. Good. Both worked at the Sloan-Kettering Institute for Cancer Research, a Manhattan-based lab complex with a worldwide reputation. An exceptionally bright scientist with a flair for teaching, Good, 50, had tremendous energy, drive, ego, and a penchant for promoting himself. In 1973 his picture appeared on the cover of *Time* magazine. Good was also the epitome of a well-organized lab chief. In a five-year period he had coauthored almost 700 scientific reports, a feat achieved by establishing a large empire of research workers under his personal banner.

Far from puffery, the papers he signed were highly regarded. Over a fourteen-year period, work with Good's name on it was cited by other scientists more than 17,600 times—making him the most frequently cited author in the history of research.

Summerlin was tall, balding, 35, and affable. Born in a small town in South Carolina, he was sure of himself and his work. But he had little grant money to pursue his ideas. He had gone to work for Good in 1971, when Good was at the University of Minnesota and director of one of the nation's largest immunology research groups. The arrangement was to the benefit of both. Good had money to support Summerlin, and the unknown Summerlin wanted to follow up a breakthrough he believed he had made in transplantation research. The grants soon started to roll in: An early paper by Summerlin, the first to which Good added his name, acknowledged funding from the National Institutes of Health, the Veterans Administration, and the National Foundation (March of Dimes).

Soon after Summerlin's arrival, Good decided to move, with a retinue of 50 researchers, from the University of Minnesota to the Sloan-Kettering Institute, where he had been invited to become director. Summerlin was now an established researcher and to a certain extent had to financially fend for himself, even though Good still signed papers he produced. In March 1973, Summerlin flew off to a science-writers meeting run by the American Cancer Society, where he presented an outline of his work in progress. Having applied for an award from the society that over a five-year period would run to $131,564, Summerlin hoped that favorable media coverage would improve his chances of receiving the grant.

At the meeting, Summerlin reported to eager journalists that "after human skin is maintained in organ culture for four to six weeks, it becomes universally transplantable without rejection." Not only that, he further announced that he had transplanted human corneas, after culture, into rabbit eyes without rejection. It seemed that one of the major hurdles to transplant surgery of all kinds was about to be surmounted. The next day's edition of *The New York Times* carried a three-column headline declaring "Lab Discovery May Aid Transplants." Overnight, Summerlin had become a scientific celebrity.

Despite the public success, and lectures given by Summerlin at scientific meetings, other researchers were increasingly skeptical as they tried and failed to repeat his work. Good, supposedly the intellectual inspiration behind the research, reassured some immunologists, putting the strength of his own reputation behind his words. Most distressing was that the English immunologist Peter Medawar and his colleagues were unable to reproduce Summerlin's results.

Medawar, who had received a Nobel Prize for his studies of transplantation, was also a member of the board of directors of the Sloan-Kettering Institute. In October 1973 Summerlin, in a presentation to the board of his work with corneal transplants, produced a rabbit that he said had received corneal grafts in both its eyes. As Medawar later described the scene, "Through a perfectly transparent eye this rabbit looked at the board with the candid and unwavering gaze of which only a rabbit with an absolutely clear conscience is capable. I could not believe that this rabbit had received a graft of any kind, not so much because of the perfect transparency of the cornea as because the pattern of blood vessels in the ring around the cornea was in no way disturbed. Nevertheless, I simply lacked the moral courage to say at the time that I thought we were the victims of a hoax or confidence trick."

Even people in Summerlin's own lab were having trouble with the experiments. The situation by March 1974 had deteriorated to the point that Good felt it would be necessary to publish a report by a junior member of Summerlin's lab announcing failure to repeat certain of Summerlin's experiments. The report would probably throw Summerlin's career into a tailspin. On the morning of March 26, at four o'clock, Summerlin rose from the cot in his office on which he slept in times of crisis, and prepared for a critical meeting with Good. His goal was to persuade Good that the negative report was unnecessary because success was just around the corner. A new experiment in transplanting skin between mice was going well, and Summerlin planned to show two of the animals to his mentor.

On his way to Good's office about seven o'clock, Summerlin whipped out a felt-tip pen and inked in some black patches on the white mice. Summerlin later claimed he did this simply to make the black patches of transplanted skin stand out more clearly. Good did not notice Summerlin's handiwork. It was the lab assistant to whom the mice were returned who noticed the embellishment and informed his superiors. Summerlin was immediately suspended from duty.

Why did Summerlin stoop to deceit? The official explanation from Sloan-Kettering was that the man had gone out of his mind. Lewis Thomas, president of the center, announced in a formal statement of May 24: "I have concluded that the most rational explanation for Dr. Summerlin's recent performance is that he has been suffering from an emotional disturbance of such a nature that he has not been fully responsible for the actions he has taken nor the representations he has made. Accordingly, it has been agreed that the Center will provide Dr. Summerlin with a period of medical leave on full salary ($40,000), beginning now, for up to one year, to enable him to obtain the rest and professional care which his condition may require." It is a recurring theme. Episodes of deceit are often said to be caused by insanity, at least according to administrators where the deed was performed.

A more thoughtful explanation was provided by Medawar, who suggested that, early in his career, Summerlin had perhaps successfully transplanted skin between genetically unrelated mice, but had been unable to repeat the experiment: "Being absolutely convinced in his own mind that he was telling a true story, he thereupon resorted, disastrously, to deception." Recent experiments suggest that Summerlin's approach may hold a certain promise, even if not in his hands.

A committee set up by Thomas to look into the affair suggested that Good himself should share part of the blame for the Summerlin affair, in particular for allowing Summerlin's results to be trumpeted to the press before they had been adequately confirmed. The committee also opined that "Good was slow to respond to a suggestion of dishonesty against Dr. Summerlin at a time when several investigators were experiencing great difficulty in repeating Dr. Summerlin's experiments."

Despite the mild rebuke, however, the committee concluded by excusing Good on the grounds that a high-powered administrator like him was too busy to supervise a subordinate, and that "the usual presumptions of veracity and trustworthiness on the part of co-workers" would have made it difficult for him to entertain the notion of fraud.

Summerlin himself, one of the most outspoken members of the scientific hall of shame, did not think that these arguments exonerated Good. "My error," he said in a formal statement, "was not in knowingly promulgating false data, but rather in succumbing to extreme pressure placed on me by the Institute director to publicize information." He expanded on this theme for the *Journal of the American Medical Association*: "Time after time, I was called upon to publicize experimental data and to prepare applications for grants from public and private sources. There came a time in the fall of 1973 when I had no new startling discovery, and was brutally told by Dr. Good that I was a failure in producing significant work. (Dr. Good denies this.) Thus, I was placed under extreme pressure to produce."

When apprentices are discovered playing scandalously loose with data, the institution affected by the transgression often feels obliged to appoint a blue-ribbon committee to look into the affair. Such committees seldom deviate from a preordained script. Their basic role is to reassure outsiders that everything is well with the institutional mechanisms of science. A pro forma knuckle-rap will be administered to the lab chief, but the weight of the blame is always placed on the errant apprentice. Since he has already been caught red-handed (always by his fellow apprentices, not the busy master), he has little choice but to play his allotted role of scapegoat, a custom in which, it might be noted, the cursed beast is sent off into the wilderness bearing not just his own sins but those of the whole community.

Because science appears ideally as a rational endeavor pursued by individuals of Olympian objectivity, many of the very human and mundane motivations of its practitioners are denied or overlooked by both the public and the scientific community itself. However, just as such nonrational characteristics as creativity, imagination, intuition, and persistence are essential to the scientific process, so also such qualities as ambition, envy, and the propensity to deception play a role in the working of science. The existence of fraud in science proves that nonrational elements are at work, both in the individual who fakes data and in the community that accepts them.

The rationality evident in science has also been misinterpreted as meaning that science is the only rational exercise of intellect in society, or at least the highest and most authoritative. Some scientists, in their public appearances, play up this role, which seems to invest them as cardinals of reason propounding salvation to an irrational public. The rigidity imposed by such a role is evident in the typical response of the scientific establishment to fraud. Establishment spokesmen generally find it difficult to suggest that a certain background level of fraud should be expected in science as much as in any other profession. They are also reluctant to concede that the practices or institutions of science should shoulder any part of the blame for fraudulent behavior. Only by abandoning the conventional

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ideology of science would it be possible for them to accept fraud for what it most probably is: a small, but not insignificant, endemic feature of the scientific enterprise.

By rejecting fraud as a serious issue, by insisting that internal mechanisms provide all the necessary checks on the system, and by essentially asserting that violators of the ethical ideal have lost their senses, spokesmen for the scientific establishment such as Philip Handler, Lewis Thomas, and others leave the scientific community in an awkward position, especially when the implications of scientific fraud move beyond the world of pure research to the realm of public policy. In such areas as the testing of food-and-drug ingredients, fraud can have immediate and significant implications. Besides the practical damage done by some scientific frauds, each new revelation of laboratory legerdemain diminishes the credibility of science with the public.

Rather than persist in such all-too-common ritual denials of the existence and significance of various types of fraud within the scientific community, it would be in the interests of scientists to remedy the causes of fraud. In general, there is no absolute defense against fraud that would not bring the whole machinery of science grinding to a halt. But the detection of fraud is of far less importance than its prevention. What is required first and foremost are steps to diminish the inducement to fraud.

A simple but valuable reform would be for the scientific community to set itself more formal guidelines for the assignation of credit, in particular for that critically important part of a scientific paper—the authorship line. Two principles might be established. First, all people named as authors should have made a definably major contribution to the work reported. Any minor contribution should be explicitly acknowledged in the text of the article. Second, all authors of a paper should be prepared to take responsibility for its contents in precisely the same measure as they stand to take credit.

Medical research is another specific area in urgent need of reform. The pressures put on students trying to enter medical school encourage and reward a kind of competition that often includes deception. "Stories of cheating among premedical students are common, and the race for high grades so as to insure admission to medical school is hardly designed to encourage ethical and humanitarian behavior," says the former dean of the Harvard Medical School, Robert H. Ebert. When those accustomed to cheating experience the fierce competitive pressures of the medical world and the prestige that attaches to doing research, they do not find it too unnatural to clean up data or even invent experiments. One solution to the problem would be a wider degree of separation between medical research and medical education.

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The excessive proliferation of scientific papers is a problem that affects research in general. Too many simply worthless scientific articles are published. Such publications not only prevent good research from receiving the attention it deserves, they also indirectly protect bad research from scrutiny by cluttering up the communications system of science. Alsabti and his fellow plagiarists were able to achieve success only because of the shelter given them by the ocean of unread and unreadable articles of which scientific literature is so largely composed. Furthermore, in the present system, researchers are rewarded for extracting the maximum number of separate articles from a single piece of research, so as to amplify their publications list. This pernicious habit makes reviewing the literature almost impossible. Scientists who fragment their results should be criticized rather than rewarded.

The root of the publication problem lies in a system that is carefully protected from market constraints. Research journals publishing articles that no one needs receive two forms of subsidy from the taxpayer. Unlike commercial publishers, publishers of scientific journals levy page charges on authors to defray printing costs. The scientific libraries that buy the journals are also supported by government funds. Such subsidies help to explain the ease with which almost any scientific article, however poor, can get into print.

Attempts to tighten the refereeing process are seldom successful because a paper rejected by one journal will eventually be published in another. What is needed is greater competition caused by a sharp reduction in the number of journals, especially in medicine and biology. Many of these journals are little more than what commercial publishers refer to disdainfully as vanity presses. Unfortunately, it is the taxpayer who indirectly supports the vanity publications of the sciences through grants awarded to researchers. The practice of page charges should be sharply curtailed and market forces of supply and demand introduced wherever possible.

Just as the emphasis in publication should be shifted from quantity to quality, so promotions and grant renewals should not be handed out on the basis of a long list of seemingly important publications. Administrators should develop sophisticated means of reading and evaluating a research record, such as citation analysis, where the influence of a scientist can be measured by the number of times his work is cited by other researchers. Such techniques tell much more about the real worth of a scientist than a long list of publications on a curriculum vitae.

A reduction in the number of scientific articles, of course, suggests a more radical kind of surgery, that of a reduction in the number of scientists. The available evidence indicates that the great majority of research responsible for the advance of science is produced by a small number of scientists. This small elite depends overwhelmingly on the research of other members of the elite, not on that of the wider majority. The pace of scientific advance would not obviously be slowed if this majority did not exist. It might even be enhanced if pursued by a leaner and fitter community of researchers. Perhaps there are too many scientists. Perhaps basic scientific research would more appropriately be supported by private patrons, as economist Milton Friedman has suggested, instead of by the government.

In his book *The Decline of the West*, the philosopher Oswald Spengler cited fraud by scholars as one of the signs of a decadent civilization. It is not necessary to believe Spengler's thesis to be alarmed by the persistent, however minor, presence of fraud in science. The idea of progress is a sustaining value of Western societies, and scientific research is an important means to that end. Scientists are professionally committed to ascertain the truth on society's behalf; when they betray the truth for personal gain, neither the scientific community nor the public can afford to ignore so serious a corrosion of principle.□