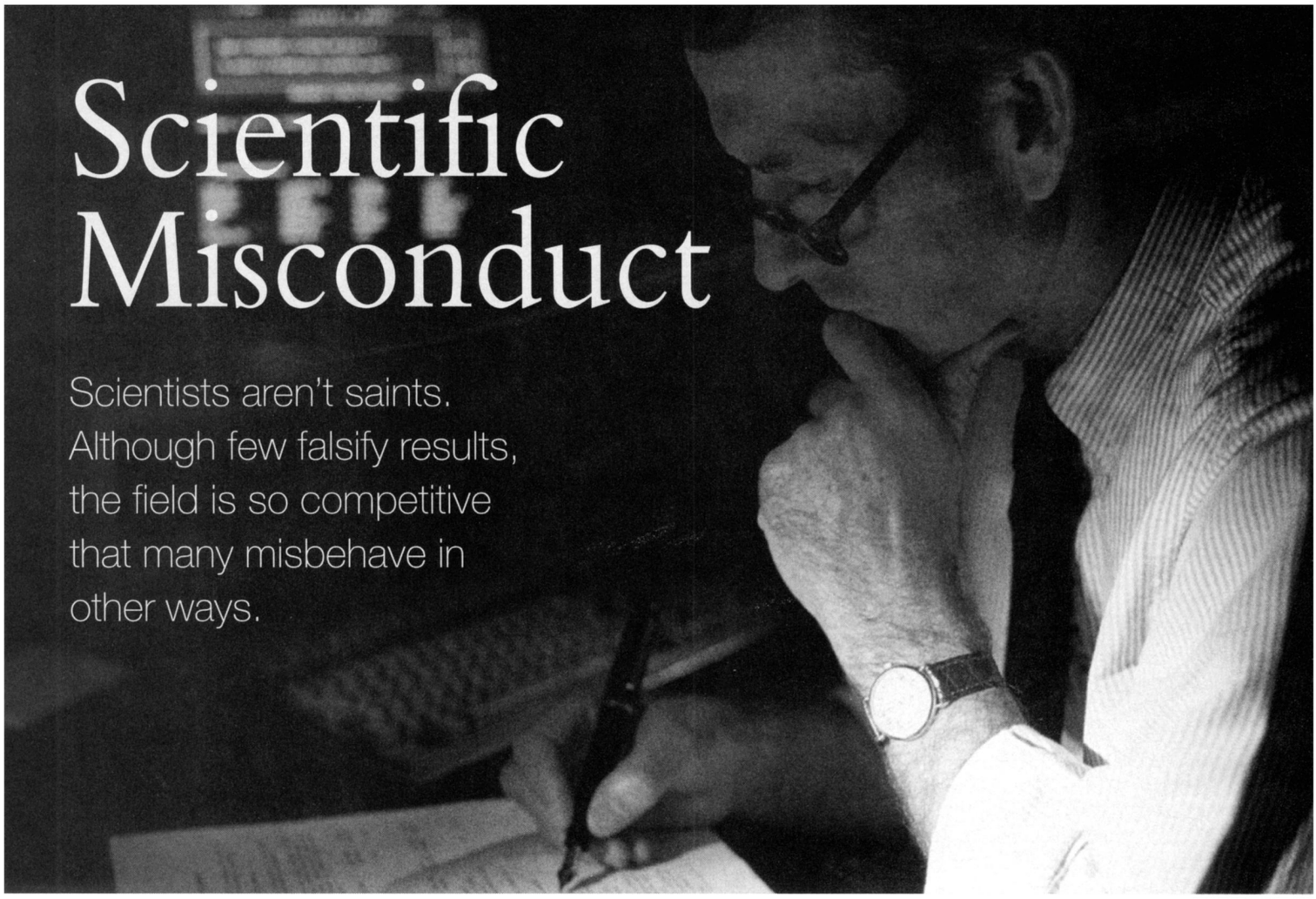


Scientific Misconduct

Scientists aren't saints. Although few falsify results, the field is so competitive that many misbehave in other ways.

By David Goodstein

My career in scientific misconduct began more than a decade ago. That's when I realized that federal regulations would soon make it necessary for all universities to develop formal rules about what to do if the unthinkable were to happen: that scientists at their institutions would be suspected of fraudulently misrepresenting the results of an investigation or the procedures needed to replicate those results.

Since then, scientific misconduct has become a virtual academic subspecialty for me. I have given lectures, written articles, and taught courses about it. I have also drafted regulations, seen them adopted by my institution (the California Institute of Technology), copied by other universities, and, much to my dismay, put into action in a high-profile case at Caltech.

David Goodstein is vice provost and professor of physics and applied physics at the California Institute of Technology, where he has been on the faculty for more than thirty years. In 1995 he was named the Frank J. Gilloon Distinguished Teaching and Service Professor.

During that case, I had the remarkable experience of seeing a skilled lawyer, with a copy of my regulations highlighted and underlined in four colors, guide participants in following every word I had written, whether I had meant it or not. Through all of that, I have learned things about conduct and misconduct in science that I would like to share with you.

Let me begin by stating right up front what I have come to believe. Serious misconduct, such as faking data, is rare. When it does occur, it is almost always in the biomedical sciences, not in fields like physics, astronomy, or geology, although other kinds of misconduct do happen in these fields. Science is self-correcting, in the sense that a falsehood injected into the body of scientific knowledge will eventually be discovered and

rejected. For just that reason, dissemination of falsehoods is never the purpose of those who perpetrate scientific fraud. Still, active measures to protect science are needed, because if the record became badly contaminated by fraudulent results, it would no longer be self-correcting.

For a long time, the government made a mess of trying to protect science. Government agencies performed poorly in this area partly because they mistakenly tried to obscure the important distinction between real fraud and lesser forms of misconduct.

In addition to these observations, I have also concluded that we scientists are complicit in presenting to the public a false image of how science works, which can sometimes make normal behavior by scientists appear suspect. Let me try to explain what I mean by all of this.

First, a word about terminology. People are touchy about words in this business. When a philosopher colleague and I decided to offer a course in this subject, we wanted to call it "Scientific Fraud." But the faculty board, in its wisdom, didn't want us teaching *that* to our students, so we had to call it "Research Ethics." The federal government, in all its gyrations, has to this day studiously avoided using the word "fraud" in connection with scientific misconduct, because in civil law that word has a specific meaning. I, however, am not afraid to call a fraud a fraud.

We scientists are complicit in presenting to the public a false image of how science works, which can sometimes make normal behavior by scientists appear suspect.

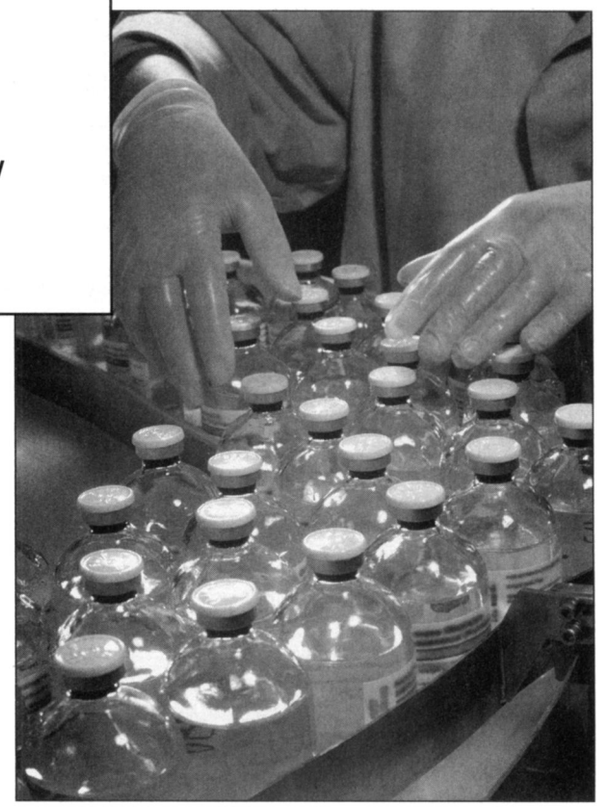
Intent to Deceive

Fraud means serious misconduct with intent to deceive. Intent to deceive is the very antithesis of ethical behavior in science. When you read a scientific paper, you can agree or disagree with its conclusions, but you must be able to trust its account of the procedures used and the results produced by those procedures.

To be sure, minor deceptions arise in virtually all scientific papers, as they do in other aspects of human life. For example, scientific papers typically describe investigations as they logically should have been done rather than as they actually were done. False steps, blind alleys, and outright mistakes are usually omitted once the results are in and the whole experiment can be seen in proper perspective. Also, the list of authors may not reveal who deserves most of the credit (or blame) for the work. Such behavior may or may not be correct or laudable, but it does not amount to fraud. Real fraud occurs only if the procedures needed to replicate the results of the work or the results themselves are in some way knowingly misrepresented.

As I have noted, this kind of misbehavior seems to be restricted largely to the biomedical and closely related sciences. A study by Princeton sociologist Patricia Woolf of some twenty-six cases of alleged misconduct that surfaced between 1980 and 1986 revealed that twenty-one came from the biomedical sciences, two from chemistry and biochemistry, one from physiology, and two from psychology. I don't know of any more recent studies, but one cannot help noticing that the U.S. Public Health Service's Office of Research Integrity, which investigates misconduct in biomedical research, seems constantly to be embroiled in controversy. By contrast, the National Science Foundation, which supports all of the sciences, including biology, has an inspector general's office that conducts its business in relative anonymity, unmolested by serious attention from the press.

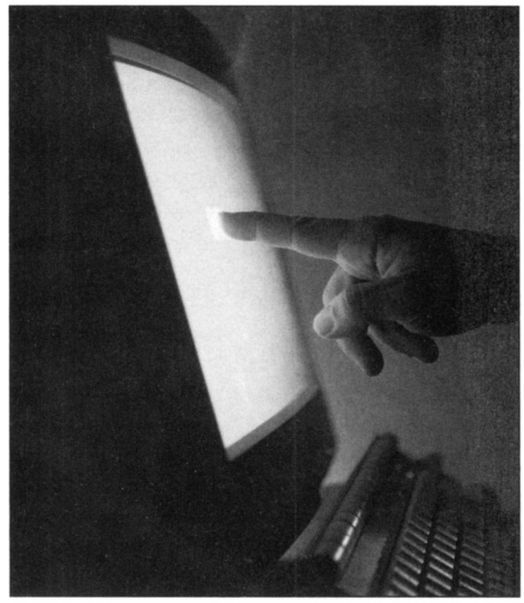
Undoubtedly, multiple reasons exist for this curious state of affairs. For example, many of the cases that have arisen have involved M.D.'s, rather than Ph.D.'s. To an M.D., the welfare of the patient may be more important than scientific truth. In a recent case, a physician in Montreal was found to have falsified the records of participants in a large-scale study of breast cancer. Asked why he did it, he said his goal was to get better medical care for his patients. Most cases, however, arise from more self-interested motives.



Among the incidents of scientific fraud that I have looked at, three motives, or risk factors, have been present. In all the cases, the perpetrators (1) were under career pressure, (2) knew, or thought they knew, what the result would be if they went to all the trouble of doing the work properly, and (3) were in a field in which individual experiments are not expected to be precisely reproducible. Simple monetary gain is seldom, if ever, a factor in scientific fraud.

It is, however, by no means true that fraud always takes place when these three factors are present; quite the opposite, they are often present, and fraud is rare. But they do seem to be there whenever fraud occurs. Let's take them one at a time.

The first, career pressure, is clearly a motivating factor, but all scientists, at all levels, from fame to obscurity, are pretty much always under career pressure. Regarding the second risk factor, if scientific fraud meant knowingly inserting an untruth into the body of knowledge, it would be nonexistent or of little concern, because science is self-correcting. Scientific fraud always involves a transgression against the methods of science, never against the body of knowledge. Perpetrators think they know how an experiment would come out if they did it properly, and they decide against going to all the trouble of doing it right.



The most obvious seeming counterexample to this assertion is Piltdown Man, a human skull and ape jaw planted in a gravel pit in England around 1908. If ever a fraudulent physical artifact was inserted into the scientific record, this was it. Yet the perpetrator was possibly trying only to help along what was known or thought to be the truth. Prehistoric remains had been discovered in France and Germany, and rumors even pointed to findings in Africa. Surely, human life could not have started in those uncivilized places. As it turned out, the artifact was rejected by the body of scientific knowledge. Long before 1954, when modern dating methods showed it to be a hoax, growing evidence that our ancestors had ape skulls and human jaws made Piltdown Man an embarrassment at the fringes of anthropology.

Still, the belief that someone else can repeat an experiment and get the same result can be a powerful deterrent to cheating.

Regarding the third risk factor, reproducibility, experiments are seldom repeated by others in science. When a result is found to be wrong, it is almost always because new work based on the result doesn't proceed as expected. Still, the belief that someone else can repeat an experiment and get the same result can be a powerful deterrent to cheating. This possibility appears to be the chief difference between biology and the other sciences. Biological variability—the fact that the same procedure performed on two organisms as nearly identical as possible is not expected to give exactly the same result—may provide cover for a biologist who is tempted to cheat. This last point, I think, explains why scientific fraud is found mainly in the biomedical area.

Around 1988 the Public Health Service (the parent of the National Institutes of Health) and the National Science Foundation decided to do something about scientific misconduct. Their view of what constituted serious misconduct in science, published in the *Federal Register,* differed greatly from what I have outlined in this article. Both agencies defined scientific misconduct to be "fabrication, falsification, plagiarism, or other *practices that seriously deviate from those that are commonly accepted within the scientific community* for proposing, conducting, and reporting research." (Emphasis added.)

Controversy swirled around the statement from the moment it appeared. Yet both agencies issued it as a "final rule" in 1990. No one took issue with their inclusion of "fabrication, falsification, [and] plagiarism" (referred to as "ffp" by the cognoscenti) in the definition of misconduct. The controversial part was the catch-all phrase "practices that seriously deviate from those commonly accepted." To many scientists and other observers, it raised the horrifying specter of the government forcing scientists into some preconceived mold of orthodox thought.

In the rules developed at Caltech, we fearlessly called scientific misconduct "research fraud" and defined it in a way essentially equivalent to "ffp" with no catch-all phrase. One day, we received a letter from the Public Health Service informing us that we were not in compliance with the agencies' definition, and asking us to submit new rules within ninety days. Eighty-nine days later, I wrote back, explaining that revising rules in a university was not so simple, that changes had to be approved by various bodies, and so on. The Public Health Service told me to send the revised rules when we finished. The heat thus being off, I referred the matter to the faculty's Academic Policies Committee without expressing any sense of urgency. A few years later, before the committee had a chance to act, the government definition changed.

Holding on to our own definition turned out to be of genuine importance. Twice during the intervening period, tenure review committees at Caltech received letters accusing the person under review of committing scientific misconduct. Letters like that can touch off panic in a tenure review committee. What to do? In both cases, the committee chair came to me, as the resident expert, to ask exactly that. I was able to say that even if the accusation were proven, it would not amount to serious misconduct under the Caltech definition. Both candidates were promoted and are today among our brightest stars. I could not have done as much under the original government definition.

In the wider academic world, that definition caused much discontent. Commissions and committees were formed, hearings were held, alternative definitions were proposed, all without serious consequence. Then, nearly ten years after the final rule was issued, a task force under the direction of President Clinton's science adviser proposed a new formulation to be applied uniformly by all federal agencies.

Like the original rule, the new rule, adopted in 2000, defined scientific misconduct to be fabrication, falsification, or plagiarism, but with the "f," "f," and "p" each carefully defined. In addition, a practice would have to deviate seriously from what is generally accepted within the scientific community to constitute misconduct, thus turning the "seriously deviate" phrase on its head. Now, rather than a catch-all phrase, we have an additional barrier that must be overcome to prove misconduct. Moreover, misconduct would have to be committed knowingly or with reckless disregard for the truth—an essential element of the forbidden term "fraud"—and proved by a preponderance of evidence. Within weeks, Caltech adopted revised rules in precise compliance with the new government rule.

New Pressures

Unfortunately, instances of scientific misconduct may not remain as rare as they have been in the past. Throughout most of its history, science was constrained only by the limits of its participants' imagination and creativity. In the past few decades, however, that state of affairs has changed dramatically. Science is now held back mainly by the number of research posts and the amount of research funds available. What had been a purely intellectual competition has become an intense struggle for scarce resources. In the long run, this change, which is permanent and irreversible, will probably have an undesirable effect on ethical behavior among scientists. Instances of scientific fraud will almost surely become more common, as will other forms of scientific misbehavior.

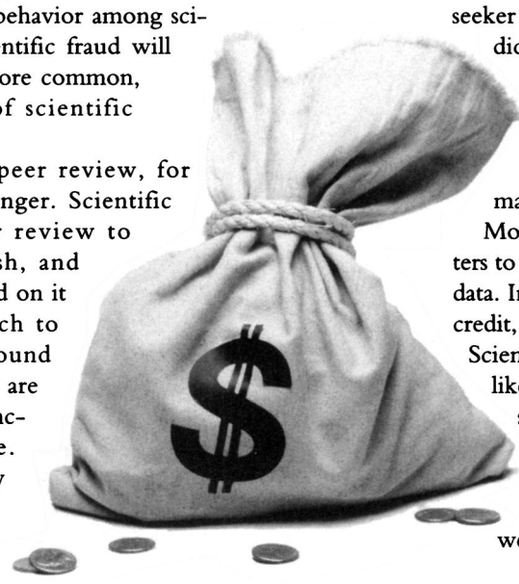
The institution of peer review, for example, is now in danger. Scientific journals rely on peer review to decide what to publish, and granting agencies depend on it to decide what research to support. Obviously, sound decisions in these areas are crucial to the proper functioning of science. Journal editors usually send manuscript submissions to referees who remain anonymous to authors. Funding agencies sometimes do the same, especially for small projects.

Peer review is a good way to identify valid science. It was especially useful when the only limit to scientific progress was the number of good ideas available. It is not, however, well suited to adjudicating an intense competition for scarce resources, such as research funds or pages in prestigious journals. The reason is obvious enough. The referee, who is usually among the few genuine experts in a field, is a competitor for those same scarce resources. Most scientists have high ethical standards and try not to let their self-interest interfere with their scientific judgment. But every scientist I know has war stories of having been mistreated by anonymous referees. Because the referees perform a professional service, almost always without pay, they are never held to account for their actions. The temptation to find fault with a rival's efforts must sometimes be irresistible.

Misconduct of this kind is, I fear, rampant in all fields of science, not only biomedical science. Recently, in a presentation to a large audience of mostly young researchers at a prestigious university, I outlined the crisis in peer review. The moderator, a famous senior scientist, was incredulous. He asked the audience how many disagreed with my heresy. No one responded. Then he asked how many agreed. Every hand in the house went up. Many of us in my generation want to believe that nothing important has changed in the way we conduct science. We are wrong. Business as usual is no longer a real option.

Scientists are not disinterested truth seekers; they are more like players in an intense, winner-take-all competition for scientific prestige and the resources that follow from that prestige.

As I mentioned earlier, I believe we scientists are guilty of promoting, or at least tolerating, a false popular image of ourselves. I like to call it the Myth of the Noble Scientist. It arises, I think, out of the long-discredited Baconian view of the scientist as disinterested seeker of truth who gathers facts with mind cleansed of prejudices and preconceptions. The ideal scientist, in this view, would be more honest than ordinary mortals, certainly immune to such common human failings as pride or personal ambition. When people find out, as they invariably do, that scientists are not at all like that, they may react with understandable anger or disappointment.



Most scientists are rigorously honest about what really matters to them, like the accurate reporting of procedures and data. In other areas, however, such as disputes over priority or credit, they tend to behave like the ordinary mortals they are. Scientists are not disinterested truth seekers; they are more like players in an intense, winner-take-all competition for scientific prestige and the resources that follow from that prestige. The sooner we admit to these facts and learn to distinguish between serious scientific misconduct and common human conduct by scientists, the better off we'll be.