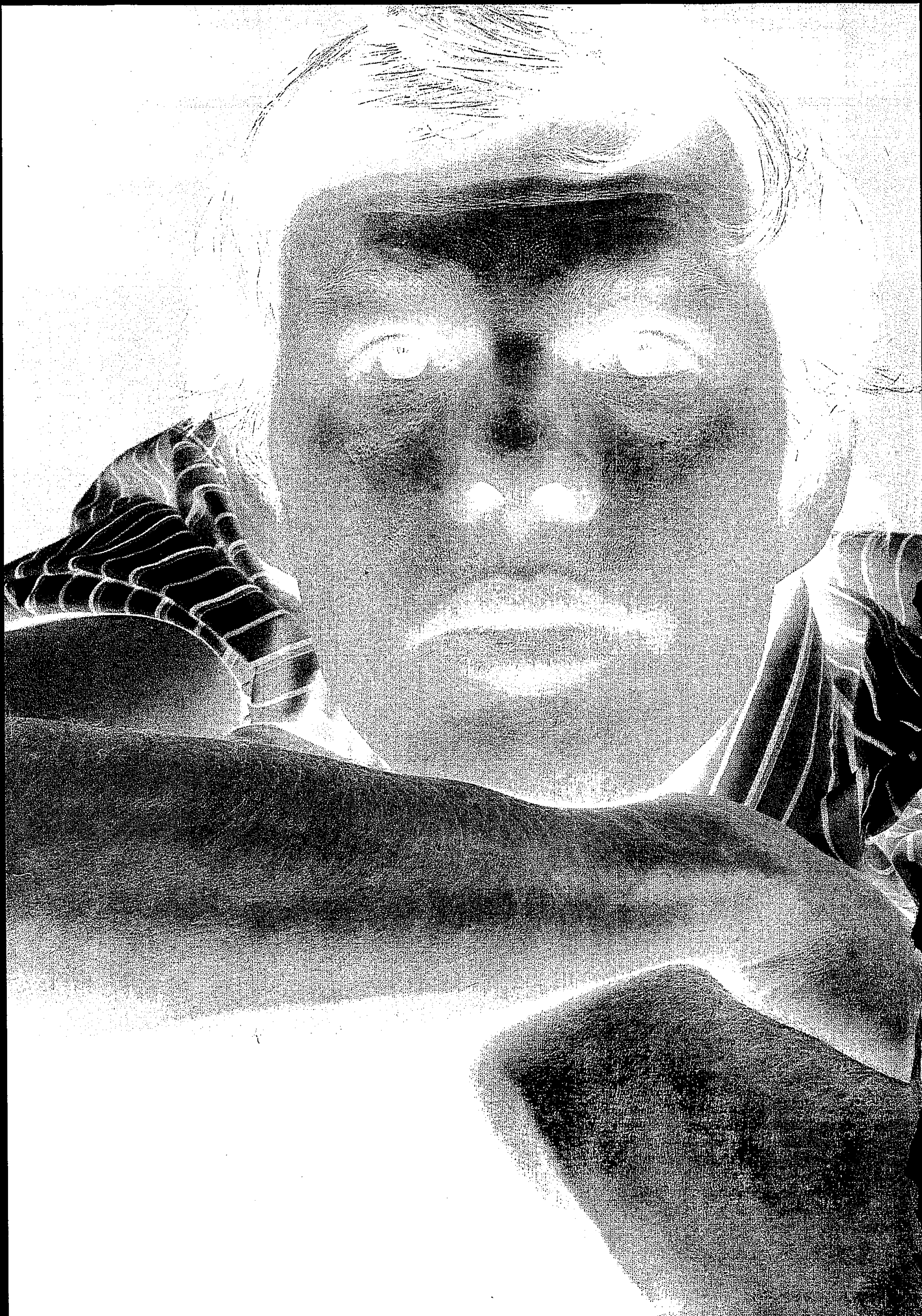


They call him the "terrorist of the lab," but this self-appointed scourge of scientific fraud has reason to suspect that as much as 25 percent of all research papers may be intentionally fudged

INTERVIEW

WALTER STEWART

"When I was young, I always assumed scientists told the truth," says fraud investigator Walter Stewart. Today he knows otherwise. Some scientists, maybe many, he says, fiddle with their data. A few scientists lie. And lots publish erroneous results. "But when something is published that turns out to be wrong," Stewart points out with indignation, "you almost *never* see a retraction."

In courtrooms and before congressional panels, Stewart and colleague Ned Feder have decided to redress this wrong. For their efforts, they have earned the enmity of a number of luminaries, among them Nobel laureate David Baltimore, who has warned angrily that their activities could serve to "cripple American science." Other colleagues just don't seem to appreciate their dedication. Reviewing some of Stewart's early research, J. Edward Rall, a deputy director at the National Institutes of Health (NIH), painted Stewart as a

PHOTOGRAPH BY MIKE MITCHELL

brilliant laboratory investigator who has unfortunately chosen to waste his time "grubbing around in the sewers of scientific stupidity, sloth, and fraud."

Officially, Stewart, is a researcher at the NIH in Bethesda, Maryland, where his current project is the genetic control of the shape of nerve cells in snails. Much of his research time in recent years, however, has on his own initiative been spent investigating cases of questioned science. Stewart and Feder receive more than 100 allegations a year that published work is wrong or crooked—a figure at least four times higher than the number of complaints lodged with the NIH's official misconduct office.

Last summer Stewart joined magician/investigator James Randi and *Nature* editor John Maddox to investigate a mysterious experiment that had just been published in *Nature* and that was making headlines around the world. A team of Parisian scientists led by Jacques Benveniste of the French Medical Research Council had supposedly discovered and documented a biological effect caused by infinitesimal amounts of a human antibody known as anti-IgE, or anti-immunoglobulin E. The experiment suggested a scientific underpinning for homeopathy, a pseudoscience that purports to cure patients with vanishingly small doses of medication. The scientific world was baffled by Benveniste's claims. A number of experts considered the effect Benveniste claimed to have observed—biological effects due to solutions diluted past the point where they could contain molecules of anti-IgE—to be impossible.

After seeing the experiment repeated seven times under various conditions and after examining the laboratory records for the last five years, Maddox, Randi and Stewart decided the "impossible reaction" was a case of self-delusion. Benveniste, however, dismissed the threesome as witch-hunters. They had unleashed, he said, a "tornado of . . . suspicion, fear, psychological and intellectual pressure" and had "terrorized" his staff. "Never let these people into your lab!" Benveniste warned the world.

Back home Stewart and Feder's challenge to a paper published by high-profile immunologist David Baltimore and coworkers attracted congressional attention. Stewart and Feder claimed that the published paper was contradicted by the group's own experimental data. They based their assertions on 17 pages of data discovered by Margot O'Toole, a postdoctoral fellow in the lab of one of the coauthors. O'Toole thought the data showed the paper contained errors that ought to be corrected in the scientific literature. Baltimore and his coauthors disagreed, and they were backed up by two university committees at Tufts and MIT that investigated the matter.

Stewart and Feder also argued that the scientific establishment was trying to look the other way instead of investigating in earnest. A three-man panel chosen by the NIH to look into the matter included a former student of Baltimore's who had collaborated extensively with him and a prominent scientist who had recently coauthored a textbook with him. The researcher who had stepped forward after discovering the 17 pages of lab notes, meanwhile, found herself publicly denounced and out of a job.

Whatever problems there were, Baltimore responded angrily, arose from minor errors, not fraud. Stewart and Feder asked to see the rest of the lab records just to check. Baltimore refused. "External reviews of data are relevant," he argued, "only when probable causes of fraud have been established." Baltimore's stand was seconded by his peers in the scientific community. Others interpreted the message as, "Let the old-boy network take care of it." Baltimore finally agreed to release his team's records to

> *"I noticed the journal paper included a seventeen-year-old with the disease. He was listed as having four children, including an eight-year-old daughter."*

an investigating committee provided, among other things, that Stewart and Feder promise in advance to drop public discussion of the matter if the committee found no fraud. The pair refused, saying they were engaging in the scientific tradition of free and open debate. They have, however, stopped talking about the case publicly while it is being investigated.

Stewart argues that their involvement in cases like this *is* science, not meddling. Science is a search for new and unknown truths, and as such is bound to involve errors. But, he says, scientists have a responsibility to correct published error. Stewart insists that he welcomes criticism but prefers it be focused on correcting factual mistakes or methodological errors he has made, rather than attacking his right to carry on investigations in the first place. His critics seldom feel thus constrained. Daniel Koshland, editor of *Science*, has written that Stewart's and Feder's activities smack of McCarthyism. Arnold Relman, editor of *The New England Journal of Medicine*, warns more ominously that "truth squads and special investigative teams are not only unnecessary but would also be destructive of the scientific spirit."

The forty-three-year-old Stewart's twin inclinations as scientist and reformer revealed themselves at an early age. As a seventh grader at Manhattan's Dalton School, he noticed a flaw in the procedure for casting votes during class assemblies, a simple show of hands in the auditorium. Kids weren't apt to cast unpopular votes if it made them feel conspicuous. He designed and built a portable voting machine for class elections, using parts he scavenged at secondhand shops. His creation, finished with help from his psychoanalyst father, came complete with latching relays to prevent students from voting twice.

After Stewart graduated summa cum laude from Harvard College in physics and chemistry in 1967, Harvard's Society of Fellows appointed him a junior fellow. This honor is given to scholars not enrolled in doctoral programs, letting them pursue individual studies. He came to the NIH in the late Sixties. Though Stewart has worked as a scientist for some 20 years, he never earned a Ph.D.

Today he shares a windowless basement lab in Bethesda with Feder and a large collection of snails. Stewart is considered a talented researcher who has made a number of useful discoveries, including the synthesis of Lucifer yellow, a dye used to study nerve cells. In recent years, however, the NIH hierarchy is said to be dissatisfied with his lack of scientific productivity, an unhappiness reflected in cutbacks in his lab space and equipment. Lately Stewart has been spending less time in the NIH basement and more on Capitol Hill. The NIH has acquiesced in loaning him to a congressional subcommittee headed by Michigan's John Dingell. The subcommittee is looking into scientific misconduct.

Putting in 80-hour weeks on fraud sleuthing has left him less time than he'd like for his family and no time at all for such chores as lawn mowing at their suburban home. His resulting experiment in "meadow gardening" has outraged his neighbors. The county government cited the incipient jungle under the so-called weed law, which creates the legal presumption that plants over 12 inches are dangerous to the public. Stewart, characteristically, has fought the neighbors and county to a standoff.

Interviewer Doug Stewart (no relation) found scientist Stewart to be a man obsessed. Impulsive, excitable, precise, and utterly serious, he would be the quintessential eccentric were it not for the perfectly reasonable explanations he offers for everything he does.

Omni: The editor of *Science* magazine has suggested that "99.9999 percent" of published scientific reports are truthful. Do you agree?

Stewart: Daniel Koshland's estimate is al-

CONTINUED ON PAGE 87

INTERVIEW

CONTINUED FROM PAGE 66

most certainly wrong. Most working scientists assume that misconduct is no problem at all. It's alarming how little we actually know about the level of scientific misconduct. Dr. Jerome Jacobstein, formerly at Cornell University Medical College, testified before Congress that he believed twenty-five percent of scientific papers may be based in part on data that's been intentionally fudged. That's a shocking figure, but it's conceivable.

Omni: What's the difference between misconduct and fraud?

Stewart: Fraud is fabricating results with the intent to deceive. Misconduct is simply behavior that most scientists consider unacceptable. If researchers cut corners they'd be ashamed to admit to in public and go ahead and publish the research anyway, that might be misconduct. If they're simply ignorant of correct methodology, that's just poor science.

Omni: How much misconduct do you suppose is out-and-out fraud and how much is error?

Stewart: Wait a minute. I wasn't talking about error. Error is absolutely intrinsic to the process of science. In trying to roll back the frontiers of knowledge, you're guaranteed to make mistakes. In science—unlike, say, accounting—we have to expect that people will make lots and lots of errors. And that means we have a responsibility to deal with those errors, whether it's our own, a colleague's, or anybody else's. It may be okay to make errors, but unless they're minor, it's not okay not to fix them.

Omni: You recently returned from an investigation of the so-called "impossible experiment" of Dr. Jacques Benveniste. What reaction did he say he saw?

Stewart: The researchers were measuring the way white blood cells react to an antibody from the human immune system. The antibody, anti-IgE [anti-immunoglobulin E], causes white blood cells to release histamine, which is what happens when people have an allergy attack. When you add a particular blue stain to a sample of white blood cells, the cells that have not released their histamine turn red. You measure the strength of the reaction by counting the red-stained cells in your sample under a microscope.

The researchers in Paris were measuring the reaction using progressively weaker solutions of anti-IgE in water. They made a series of one-to-ten dilutions—that is, they poured one tenth of the IgE solution into a new test tube and filled the other nine tenths with water. Now, if you make these dilutions four times in a row, you're left with only one ten thousandth as much of a dissolved substance as you started with. After you've made fifteen dilutions in a row, the chances are there's just a single molecule of the substance still in solution. Make five more dilutions, and there is only one chance in one hundred thousand that you've got any molecules of the anti-IgE left at all. That's only twenty dilutions, but Benveniste reported that after twenty-five dilutions, he was still observing a strong effect! His researchers did experiments with *one hundred twenty* dilutions in a row, and they still claimed to get an effect.

Omni: Did they try the experiment using just water?

Stewart: Yes. They said plain water didn't give the effect.

Omni: Benveniste's paper in *Nature* last summer caused quite an uproar. Were he correct, what would the implications have been?

Stewart: It would have meant, first of all, that doctors could expect to treat certain diseases with water instead of medicine. But more broadly, the whole basis for experimental biological science would be called into question. The universal experience of scientists has been that the effects in any reaction are due to what *is* there, not to what *was* there. Benveniste's results seemed to show that water molecules "remember," so to speak, previous contacts they've had with other molecules long after those other molecules are gone. Benveniste's findings were absolutely extraordinary, especially because his work appeared to have been so carefully done.

Omni: I understand that homeopaths were cheered by his results.

Stewart: Yes. Benveniste's results seemed to show that homeopathy has a scientific basis. Homeopathy claims to treat human ailments with solutions so enormously diluted that they actually don't contain any molecules other than water. It's a branch of pseudoscience that scientists don't take seriously.

Omni: How did you get the opportunity to test Benveniste's findings?

Stewart: I was one of several scientists John Maddox, the editor of *Nature,* asked to review Benveniste's original manuscript in 1987. Benveniste let an inspection team visit his lab because Maddox made this inspection a condition of *Nature*'s publishing the paper. The team consisted of Maddox, me, and James Randi, a professional magician known for his outstanding work showing that various paranormal claims have no factual basis. Randi's job was to ensure that Benveniste's people were doing what they said they were doing—to prevent trickery in other words. My job was to make sure they weren't doing something wrong without realizing it, like putting their thumbs in their samples.

Omni: When did you first notice anything unusual?

Stewart: We learned as soon as we got to the lab that the experiments weren't always successful. This flabbergasted me because up until then it had been either said or implied that the experiment never failed. If Benveniste's article had said, "We *sometimes* observe these results," it would have been a great deal less publishable. He'd first have had to answer the question, What are you doing sometimes that you're not doing at others?

Omni: Did you notice other odd things?

Stewart: Well, one astonishing thing we found out was that the experiments worked best—by far—when one particular scientist, a Dr. Elisabeth Davenas, was doing them. Her salary, we also learned, was being paid by a French company selling homeopathic medicines. Now, to put the kindest interpretation on this, one researcher can have a "touch" that another lacks. But even so, right away the science is less convincing. Experiments that work intermittently pose a problem. When you have trouble repeating an experiment, you've got to fix that *before* you publish the result, not afterward. And other odd things turned up. Researchers had to agitate the series of solutions violently for exactly fifteen seconds, as I recall, for the experiment to work. That sort of thing is a tenet of homeopathy, as it happens. Another member of Benveniste's team turned out to be a practicing homeopath, which hadn't been mentioned before our visit.

Omni: How did your own investigation proceed?

Stewart: At first the three of us just stayed out of the way while Davenas carried out the experiment three times. The first experiment was fairly successful, the second and third spectacularly so. In the fourth experiment, we agreed that she would read the slides in a blind fashion, meaning she would look through the microscope and count the number of red-stained cells without knowing what dilution they had been treated with. This experiment, too, was a striking success.

Omni: So in every case a strong reaction seemed to occur even using astronomically diluted solutions?

Stewart: Yes. Finally we did three more experiments. This time Davenas prepared the dilutions while I watched very carefully. The test tubes were then placed in front of a video camera, and Davenas left the room. The video camera made an objective, unbroken record of what we did. In that way we couldn't be accused later of mistakes or deliberately introduced errors. We then relabeled all of the test tubes with a random code. The key to the code was sealed in an envelope and taped to the ceiling of the lab in full view of everyone. At the last minute I suggested we add five controls—test tubes with plain water only. Once we began insisting on ground rules for the experiment, the atmosphere in the lab grew increasingly tense. We began to encounter objections, even animosity, from the staff. Until then Benveniste had been extremely friendly, greeting us on our arrival with delight. Davenas, on the other hand, had maintained an extremely guarded attitude during our entire stay. We had planned to pour everything into a new set of test tubes so the tubes themselves couldn't have been secretly coded. But the French objected, saying that they couldn't be sure of the effect of transferring a solution to a new test tube.

Omni: How could they object to that?

Stewart: Well, it was rather extraordinary. Randi and I had begun making lists of all the little things that they said could interfere with the experiment: the source of the blood, the quality of the stain or the distilled water, the number of ordinary blood cells, the age of the chemicals, observer fatigue, pipetting more than twice—the list went on and on. Earlier Benveniste had drawn me aside and told me in an elated way, almost alarmingly so, about some mathematical theory that a series of one-to-nine dilutions—I don't recall the exact number—wouldn't lead to a reaction, while one-to-ten dilutions would. It was just inconceivable. The whole situation began to seem very silly.

Omni: In these final experiments, did you also insist on tightening up the way cell counts were made?

"Of course power corrupts! But absolute power is even better!"

Stewart: Yes, we arranged for two researchers to read the slides without knowledge of the other person's counts. These people had never attempted to take into account observer bias and didn't seem to understand the necessity of doing so! Whenever you're measuring something, you have to ask what your errors in measuring are. Counting chickens is relatively simple. But in counting cells with red granules under a microscope, you have a number of decisions to make. Do you count cells with bluish-red granules? What about cells with purple granules or faded pink granules? People sometimes count the same cells twice. Whenever you use a human as a scientific instrument, as you do when you have an observer count or measure something by eye, you have to find out how prone they are to making mistakes.

Omni: So no one knew how successful these final tests were until the codes were unsealed at the end of the day?

Stewart: That's right. And as the time came to pull the envelope containing the codes off the ceiling in order to decode the observations, the tension present during the experiments dissolved into euphoria. Benveniste seemed enormously confident that the results were going to be positive. He had even scheduled a press conference immediately after the code was to be broken. By this time I was pretty certain the experiments would turn out to be failures and was feeling extremely uncomfortable.

Benveniste is an emotional man. Everything was being videotaped, and he was saying things like, "Someday these videotapes will be famous." He even told Maddox that when this was over he'd be happy to offer him a job. He was apparently serious, but I was flabbergasted. Even the world's top scientists don't go around offering jobs to John Maddox, who, as editor of *Nature*, already has a rather distinguished job. I found Benveniste's euphoria a little eerie.

Omni: Were the experiments a failure?

Stewart: Well, when we were about eight or ten test tubes into decoding the first experiment, Benveniste said, "That blood's not working. Try another." His comment seemed to be a metaphor for what they had been doing all along. It was soon clear that none of the experiments had worked. As we were working out the results in the conference room, one of Benveniste's secretaries put her head in through the door and said, "Dr. Benveniste, the TV cameras are here." And he said, "Tell them to go away. Tell them we will be in discussion all day." There was enormous excitement in the French media about this, you see. Homeopathy seems to be much more popular in France than it is here. In Paris you see homeopathy remedies advertised all over the place.

Maddox, Randi, and I all felt it was important at this point to inform Benveniste of the grave reservations we had about the way he and his staff had conducted their experiments. Randi made the point that extraordinary claims require extraordinary proof, explaining that if a man claims to have a goat in his backyard, you might verify this by calling up a neighbor and asking him to look over his fence to check. But if the man claims to be keeping a unicorn in his backyard, you'd want a higher standard of proof.

Omni: Did Benveniste argue with you?

Stewart: Yes, fiercely, almost angrily. I actually thought Benveniste had taken advantage of *us*. We'd wasted a lot of effort coming there simply because he had not disclosed all the facts he knew. I thought I had a duty to advocate my belief as forcefully as possible. It was never a screaming match, but I wouldn't call it a casual conversation. The whole situation was unpleasant for everybody. People in the lab were crying. On our way out we noticed one of the staff putting away bottles of champagne, unopened.

Omni: Did Davenas cheat or not?

Stewart: That's certainly one explanation. But whether Davenas was cheating or not, among all of the gross breaches of proper scientific practice that we saw there, observer bias was certainly a key factor. Numerous studies have shown that when you know what result is expected of you, you're more likely to reach the result that fits your theory. When Davenas counted the same slide more than once, her duplicate counts were too good—they were in closer agreement than sampling error and the laws of probability allowed. When we pointed this out to Benveniste, he said, "But you don't understand how meticulous her technique is." *He* just didn't understand.

Omni: What was your first investigation of scientific misconduct?

Stewart: The John Darsee case, although it wasn't actually Darsee whom Ned Feder and I investigated. A heart expert, Darsee had worked at Emory University and Harvard Medical School. His peers considered him to be a brilliant researcher. He had published an unusually large number of papers for someone so young. In 1981 he was being offered an assistant professorship at Harvard when some people in his lab noticed that he had fabricated a piece of evidence. When confronted, Darsee admitted to only this one fabrication.

As Harvard alumni, Feder and I received a report from the school detailing its own investigation of the matter. We immediately saw very serious flaws in this investigation, which concluded that Darsee had fabricated no more than three isolated pieces of evidence in his career. As scientists, we knew that the evidence the Harvard committee reviewed couldn't possibly have supported that conclusion. When the NIH reinvestigated the matter some time later, it discovered a huge amount of fraud. Darsee had fabricated data on an absolutely blatant scale throughout his career.

Omni: Whom *did* you investigate?

Stewart: We decided it would be interesting to tabulate the practices of the scientists who had coauthored papers with Darsee. We simply read the reports the investigative committees at Harvard and Emory had written and then Darsee's papers. There were eighteen full-length research papers, three book chapters, and eighty-eight abstracts.

We were astonished to see that Darsee's papers contained a very large number of obvious errors. For example, one paper, published in *The New England Journal of Medicine,* introduced a human pedigree [genealogy] showing the inheritance of a new disease that the paper described. It later turned out the entire pedigree was fabricated. I noticed the paper included a seventeen-year-old with this disease. He was listed as having four children, including an eight-year-old daughter. We thought the journal's referees should have caught this kind of thing. Instead, it was published right there in Figure 1 of the lead article in what is widely considered to be the leading journal of medicine. When I phoned up Dr. Arnold Relman, the journal's editor, to call his attention to this, he said instantly, "That's a misprint." Well, obviously it wasn't. The seventeen-year-old's age was mentioned twice elsewhere in the article.

Omni: How did Darsee's coauthors react when you started calling them up?

Stewart: Those who were completely cooperative were a minority. Some felt quite threatened, which is understandable. One Harvard professor refused to talk to us at all. Our overall tally showed thirty-five of the forty-seven coauthors engaging in scientific practices that would not generally be considered acceptable. Some had made false statements that they either knew or, in our judgment, should have known were false. An example would be hiding the fact that the control for an experiment had been done a year before. This would make the experiment look much stronger than it actually was.

Many of the errors were minor, probably caused by haste. But some—like the seventeen-year-old with four children—were so fundamental as to undermine the truthfulness of the paper as a whole. Some statements gave the appearance of an intent to deceive. I believe coauthors, except in cases where they're collaborating across disciplines, have a responsibility to ensure the accuracy and truthfulness of the entire paper.

Omni: What became of your report on Darsee's coauthors?

Stewart: We sent a draft off in 1983 to Maddox, who was quite interested in publishing it. The draft also went to many of the coauthors. Shortly after that, *Nature*, the NIH, and Ned and I began to receive letters from lawyers clearly implying that we'd all be sued for libel if *Nature* published our report. We spent a year and a half replying to these arguments and wrote about fifty different drafts, trying to accommodate the lawyers.

Finally we withdrew the paper and sent it informally to about fifteen other journals. All but one said, "No, we couldn't even consider it." *Cell*, a journal at MIT, was actively moving toward publication but then made two unusual stipulations. The first was that we take complete financial responsibility for any litigation involving the journal or MIT. We agreed, although I'd never do that again. The second was that we agree never to discuss the subject with anyone in any forum. This was later softened to five years but was still unacceptable to us. We feel that scientists have an absolute right to say what they believe.

Eventually we resubmitted it to *Nature*, reluctantly agreeing to a few last changes. It's amazing how the longer you wait, the more flexible you become. *Nature* published it in 1987. No one was sued. We didn't even receive angry letters. We got hundreds of letters from the scientific community, many saying the problem was worse than we'd described.

Omni: Have you any concrete suggestions for raising report standards?

Stewart: Individual scientists can make a personal commitment to keeping their data. Many of Darsee's collaborators had failed to retain all their lab records—results of experiments, measurements on patients, things like that. Raw data should be saved for a few years.

With present attitudes it's difficult for an outsider to ask for a scientist's raw data without appearing to question that person's integrity. But that attitude absolutely has to change. You have to distinguish among three things: new ideas; proving them with experiments; and finally documenting your proof and publishing it. I've never suggested that anybody has a right to anybody else's unpublished research. But once you publish a paper, you're in essence giving its ideas away. In return for benefits you gain from that—fame, recognition, or whatever—you should be willing to make your lab records and data available. And there is another reason for full disclosure. Published experiments should be repeatable. A published report can never disclose everything you did in the lab. But you have an obligation to describe the most important parts. A scientist trying to repeat another scientist's work is essentially like someone reading a recipe out of a cookbook.

Omni: But famous chefs are notorious for concealing hard-won secrets.

Stewart: Look, the whole idea of science is to communicate your findings and methods. One of the beautiful things about science is that you build on others' results. Two or three centuries ago scientists often held back essential parts of a procedure to protect their positions. Antonie van Leeuwenhoek, who was honored by [Britain's] Royal Society for discovering microbes, never revealed how he built his microscopes. Today no one thinks that's satisfactory, although it's still sometimes done.

Omni: You haven't published very much yourself—less than a dozen papers after nearly twenty years of research. Why?

Stewart: I publish only when I have something I think is worth communicating to other scientists. That hasn't happened frequently. If I were pressured to publish more papers, it's doubtful I'd make more discoveries. There are something like eight thousand biomedical journals publishing papers today, and most of these papers are unimportant and unbearably dull. What's important is not how much you publish but what you discover and whether it's useful.

Omni: One significant discovery of yours has to do with something called wildfire toxin. What is it?

Stewart: Wildfire toxin is a chemical secreted by a bacterium that causes a disease of tobacco plants. The disease used to spread like wildfire, hence the name, although it was brought under control long ago. The late D. W. Woolley, a brilliant experimental scientist [despite being totally blind since the age of twenty-five], had studied the toxin. In the Fifties he published a paper describing its molecular structure, but it later proved to be wrong.

I decided to use modern methods to determine the molecule's structure. I thought I could do it using old chemicals that Woolley had left behind in his lab at Rockefeller University, where I had been a graduate student after he died. His widow and I went over his old test tubes and notebooks, looking for his results. The experience turned out to be fascinating: Here's a practical example of error in science being corrected through the sharing of data. Woolley was sharing his data with me after his death by means of his carefully kept lab records and chemicals. I was actually able to pick up the research where he left off. After I went to the NIH, I solved the toxin's molecular structure and published it in *Nature*.

Omni: What research have you worked on more recently?

Stewart: One very exciting piece of work I did that's proved useful to others is the synthesis of Lucifer yellow, a fluorescent dye. You inject it with a thin glass needle into nerve cells that are maybe one fifth the width of a human hair, and all of the nerve endings become not only visible but fluorescent. The dye never existed before—I had to invent it. I must have made a hundred different dyes trying to find one with the properties I wanted. It was an obsessive, mad hunt for a miracle reagent. When I finally succeeded and began to get these images under the microscope that no one had ever seen before, beautiful images of nerve cells glowing against a black background, it was incredibly exciting.

Omni: What do you think about NIH deputy director J. Edward Rall's comment about you wasting your talent?

Stewart: Perhaps he believes investigating scientific practices isn't important. If so, I disagree. Scientific misconduct affects not only the health of science but also the public's perception of whether or not scientists care about this sort of thing. In doing an investigation, I'm both using my skills as a scientist and following my own interests.

I remember noticing that pedigree of the seventeen-year-old with the eight-year-old daughter in *The New England Journal of Medicine*. We had been wading through all these very complex medical papers, and it wasn't obvious what we were going to find, if anything. All of a sudden to make this unexpected discovery—it was exhilarating. I felt the joy of discovery that I think all scientists feel when they suddenly understand something they didn't before.

Omni: Arnold Relman recently accused you and Feder of having "arrogated a mission that nobody has given [you . . . and] have set [yourselves] up as more or less grand inquisitors."

Stewart: That's inaccurate. We've used ordinary scientific methods in our investigations of scientific papers. We've had no unusual powers to get this material. It's true that nobody specifically gave us this mission, but nobody told me to synthesize Lucifer yellow either. I just decided I was going to do it. As I recall, Relman's comment refers to our challenge to the accuracy of the paper David Baltimore's group published in *Cell* in 1986. Scientists must feel free to challenge a paper if they have evidence suggesting it's wrong.

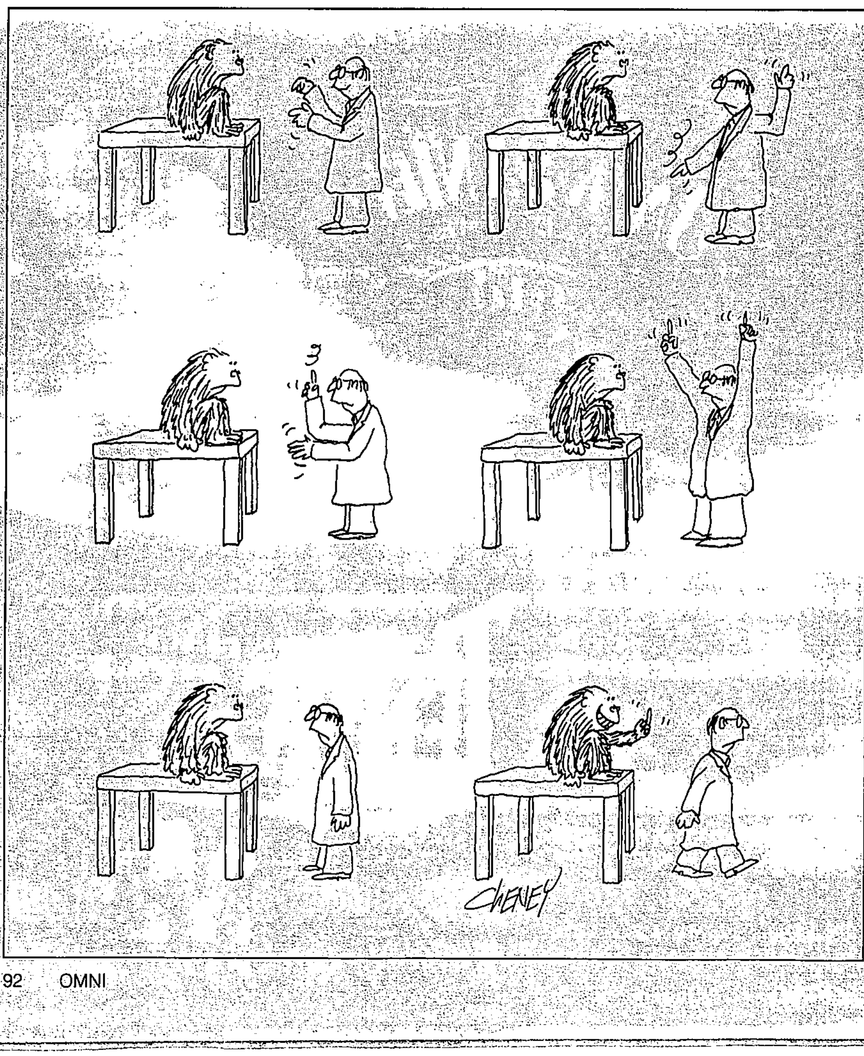
Omni: The Benveniste affair wasn't the first time you contributed a published rebuttal of someone's paper in *Nature*.

Stewart: Right. There was the scotophobin paper in 1972. Scotophobin was the name given to a chemical that was said to transfer learned information between rats. You'd allow a rat to enter a dark box, which rats like to do, then you'd punish it with an electric shock. After the rat learned to avoid the box, you supposedly extracted from its brain this chemical, scotophobin—*scoto* means "dark", *phobos* means "fear." When you injected the chemical into other rats, these rats supposedly acquired an instant fear of the dark. It seemed to be the first demonstration of learning caused by a chemical, not experience. A scientist in Texas claimed to have discovered it. Other teams confirmed the phenomenon. If it had been real, it would have profoundly advanced our understanding of learning.

Nature sent me the scotophobin manuscript for review. I wasn't impressed with the data, and I suggested that *Nature* ask the authors to put their best evidence together, then ask an expert to write a rebuttal. *Nature* chose me to write the rebuttal. The scientists were not willing to publish all their data, which was a tip-off that there were problems.

Omni: What was the gist of your rebuttal?

Stewart: They had used their mass spectrometer to analyze the chemical's composition. [The mass spectrometer displays each chemical's composition as a unique spectrum, like a fingerprint.] But they had used the results selectively. They didn't present the whole spectrum. I got a spectrum of a completely unrelated chemical and, using their reasoning, showed that it fit their analysis just as well. What they were extracting from the rats'



brains could have been any number of things. The authors were allowed a fifteen-hundred-word reply to my analysis, in which, as I recall, they complained of not having enough time to reply and advanced some pretty poor scientific arguments. No one believes in the existence of scotophobin today.

Omni: You said several other labs had confirmed the scotophobin phenomenon. How do you explain that?

Stewart: Observer bias—wishful thinking. It happens all the time. Just after the turn of the century a respected French physicist named Blondlot discovered a phenomenon he called N-rays. This was supposedly a new kind of radiation given off by a variety of sources: heated metal, the sun, even muscles. Blondlot believed N-rays passed through aluminum, and he carried out experiments using an aluminum prism to focus the radiation. A number of other laboratories confirmed his observations. *Nature* got involved too, agreeing to publish a report by American physicist R. W. Wood, who went to France to see for himself. When Blondlot wasn't looking, Wood slipped the aluminum prism into his pocket. The effect continued to work perfectly, utterly demolishing the basis of Blondlot's theory, which was instantly discredited in most of the scientific community. But as so often happens, Blondlot and some colleagues remained convinced of the validity of what they thought they'd observed. So the next time you hear someone arguing that independent repetition of experiments will show up bad results quickly, you can remind them of N-rays.

Omni: Arnold Relman and others insist that peer reviews weed out bad science before it gets into journals. So why are investigations necessary?

Stewart: Peer review, to Dr. Relman, means that bad stuff doesn't get into *his* journal, that's all. That's clearly wrong, because John Darsee had a field day with *The New England Journal*. The only function peer review ever serves is to decide which magazine an article gets published in. There's virtually no article so bad it's not publishable somewhere. Peer review doesn't control the quality of what's published; it just assigns a rating. Journals have a pecking order. If your piece doesn't get accepted by a first-rate journal like *The New England Journal*, you can always drop down to a third- or fourth-rate journal. As for fraud, it's almost never detected by peer review or by attempts at replication. Fraud is usually discovered only when an insider tells someone else about it, as with Darsee. Somebody rats. Bear in mind, though, that the results of a fraudulent experiment may be perfectly correct.

Omni: There have been suggestions in Congress that scientific fraud be treated as a white-collar crime.

Stewart: I don't agree. First, scientific fraud already breaks various laws. If you misrepresent your results in a grant application, you're lying to the government to get money, and that's illegal. Second, the problem of fraud is so complicated and poorly understood that using legislation as a remedy could do more harm than good. I would prefer to see scientists solve the problem themselves.

Omni: How does the NIH now handle allegations of misconduct that it receives?

Stewart: Until recently it had an office staffed by two full-time people and one part-time secretary, responsible for reviewing allegations on approximately five billion dollars' worth of research. The office received allegations about only a tiny amount of misconduct, approximately twenty-four claims a year.

Before the congressional hearings last April, Ned and I estimated we were receiving about a hundred allegations a year. The rate has probably gone up since then. Typically, researchers who believe they have evidence of fraud or misconduct are apprehensive about going through the standard channels. So far we can't tell them they won't suffer professional harm if they choose to proceed. Even if people have documented their allegations in detail, if they report their allegations to anyone, it usually gets back to their university. Universities in the past haven't been very sympathetic to these so-called whistle-blowers.

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Jerome Jacobstein, for example, accused a colleague at Cornell of improper conduct. The colleague was exonerated in a meeting that lasted only two hours and resulted in a single handwritten slip of paper. The disruption to Jacobstein's career afterward was enormous. He had to spend thirteen thousand dollars of his own money on legal fees related to defending his charges. The NIH finally concluded that he was right in almost all of his allegations. The people who make their charges stick are usually, like Jacobstein, extraordinarily tenacious. Whistle-blowers are often accused of being malcontents, but that's not been my perception at all. They tend to be people who believe strongly in making the system work. If anything, they're a bit naive about their chances.

Omni: It has been widely publicized that you refused to mow your lawn and that your neighbors are furious.

Stewart: First of all, we *do* mow an acre around the house but have let the other six acres become a meadow. The house is in a neighborhood of large homes with large lawns. The first year we owned the house, we bought a tractor and mowed everything. The following year I began to question why we were mowing the whole seven acres, maintaining an ecological monoculture. We thought it would be better just to let it go wild, giving birds and mammals a habitat they sorely need.

Very quickly our five-year-old figured out that the reason we have rabbits and our neighbors don't is that rabbits need tall grass to hop off into. Now we've had four or five hummingbird pairs, hawks, bluebirds, a pileated woodpecker, a fox for a while, and all sorts of mice, woodchucks, raccoons, opossums, even deer. The county cited our meadow as a health hazard, and it was going to take us to court to make us mow it. There is a real question about whether that would have been constitutional. I told the prosecutor we were planning to bring in nine or ten national experts on our side—it was going to be a trial to end all trials. She said, "Maybe we just won't go forward with this."

Omni: Didn't *Science* run a half-page article on your lawn?

Stewart: The way *Science* handled that was a disgrace, especially because they've given my work on matters of science little coverage. Then a half page to my lawn. Of all the articles about this, *Science*'s was among the least thoughtful and most gossipy. Even the article *People* did on our lawn battle was more thoughtful. The scientific establishment is obviously unhappy with some of the things Ned and I are doing. I'm not the only person to see the *Science* article as an attempt to discredit me personally.

Omni: The editors of the two top American scientific journals, *Science* and *The New England Journal*, criticize your investigative work. Does this bother you?

Stewart: The main thing that bothers me is that I don't understand their criticisms—Relman's comment that we're "needlessly inflaming" the issues in the Baltimore case, for instance. We've asked people to criticize our work on the basis of its factual accuracy or appropriateness to specific issues. When people make general criticisms like that, I don't even know what they're trying to say. In a way, these editors *are* agreeing with us, because now they're at least discussing the question of misconduct publicly. We wanted a public debate—not with this degree of acrimony, perhaps, but a debate—and that's starting to happen. You're pointing out a success of ours: We aroused the attention of two very prominent editors.