In the Matter of J. Hendrik Schon

By David Goodstein

Published in Physics World, November, 2002

"The physicists have known sin" J. Robert Oppenheimer is famously said to have said. That was on the occasion of the first nuclear explosion. Sin in the form of faking scientific data seemed to be reserved to biology and related sciences, not physics. I used to think I understood why.

"There are three danger factors in scientific misconduct," I would lecture to my classes in Research Ethics and anyone else who would listen. Not that research misconduct happens whenever these factors are present. They are often present and misconduct in science is very rare. But these factors were present in every case I've studied. First, the scientist is under career pressure. That's not much of a discriminator, because all scientists are under career pressure all the time, but it does point up the fact that this kind of misconduct is not motivated by simple monetary gain. Second, the perpetrators always think they know the right answer. In other words, faking data is never done with the intention of inserting a falsehood into the body of scientific knowledge. The intent is always to insert a truth without bothering to go to the trouble of doing the experiment properly. This kind of misconduct is always a violation of the scientific method, never purposely a violation of scientific truth. And finally, the work is always in a field where reproducibility is not expected to be very precise. For example, if you take two organisms that are as nearly identical as you can make them, say, two transgenic mice, and expose them to the same carcinogenic agent, you don't expect them to develop the same tumor at the same time in the same place. So, biologists who are otherwise disposed to cheat generally don't have to fear that someone will quickly prove them wrong merely by repeating the experiment. That, I would conclude, is why faking data occurs in biology, not physics.
Now two high profile cases of cheating in physics have suddenly surfaced. One involves the announcement and later retraction of the discovery of elements 116 and 118 at Lawrence Berkeley National Laboratory (LBNL). The other involves a young researcher at Bell Labs named Jan Hendrik Schon. These cases promise to pose a severe test for my theory.

Unfortunately, as in many cases of scientific misconduct, little is known to the outside world about the LBNL case. An investigation took place, and a scientist named Victor Ninov was fired as a result. But the report of the investigation has not been made public. Quite the opposite is true in the Schon case. In a rare instance of openness in the murky field of scientific misconduct, the management of Bell Labs made it clear from the outset that it intended to make public the results of its investigation. It has now done so.

The general outlines of the case have been widely reported [reference previous Physics World reports]. Jan Hendrik Schon seemed to be a brilliant young condensed matter experimentalist zooming straight toward Nobel Prize country. The field was organic or carbon-based semiconductors, and one after another Schon seemed able to grab every

Holy Grail in the business. Many of the samples were fabricated at Bell Labs and prepared for measurements at the University of Konstanz while Schon was waiting for a visa to join Bell Labs. He managed, for example, to use field-effect doping—the use of very large electric fields to change the electron concentration in his samples—to induce such remarkable phenomena as superconductivity and the Quantum Hall effect. Other researchers had been unable to reach high enough fields to detect these miraculous effects because of electrical breakdown in the insulating layers that are essential for such experiments. But Schon, in a humble apparatus in Konstanz, had managed to produce aluminum oxide films of unprecedented resistance to breakdown. In the period from 1998 to the summer of 2001, he produced research papers on the average of one every eight days, together with a total of twenty collaborators. A blazing superstar of physics had been launched.

Then the wheels started to come off. The announcement of a single-molecule transistor--the logical endpoint of Moore's Law—triggered the beginning of an unsuspension of disbelief. Anomalies were pointed out. The data were too perfect. Different experiments had identical noise. And so on. In the Spring of this year, Bell Labs appointed a committee, chaired by Professor Malcolm Beasley of Stanford University to investigate. The committee's report was released to the public, as promised, on September 25.

The report detailed some 24 specific allegations the committee had investigated, and found that scientific misconduct by Schon had occurred in at least 16 of them. Schon had done all of his experiments alone, he kept no laboratory notebooks, all his raw data files had been erased from his computer, and all of his original samples had been either destroyed or discarded. With only the slightest of misgivings, the report exonerated all of Schon's collaborators. Schon was immediately fired by Bell Labs.

The case raises a number of issues. To begin with, I find it amazing that when it arose, Bell Labs had no formal policy on how to handle cases of research misconduct. All American universities that accept federal research funds are required to have such policies, but Bell does not have federal funds. The attitude there seems to have been one that was common in the universities a couple of decades ago—it couldn't happen here, so why do we need such a policy? The Beasley committee resolved this dilemma by choosing to follow the federal policy that guides the universities. That, for example, established the level of proof of guilt required. Not, as in a criminal case, beyond a reasonable doubt, but rather a preponderance of the evidence would be sufficient. I would imagine that Bell and other industrial laboratories will now get the message and put appropriate policies in place.

A more difficult issue concerns the responsibility of the other authors. The Beasley report defines this as an issue not of scientific misconduct but of professional responsibility, and decides that "…no clear, widely accepted standards of behavior exist", because it is an issue that "the scientific community has not considered carefully." In fact the issue here is trust among scientists. Collaborations take place precisely because different scientists bring different skills to the table. If we are responsible for looking over the shoulders of our collaborators, collaborations will fall apart, and much damage will be done to

science. Still, it makes one uneasy that there were so many collaborators who never suspected wrongdoing.

What about my theory? Those three danger factors I wrote about? In this case they seem to hold up pretty well. Was Schon under career pressure? You bet he was, as is everyone at a place like Bell Labs (or my own Caltech for that matter), perhaps made all the more brutal by the intensely competitive nature of the field he was in, and the unyielding pressure to stay ahead of the curve on Moore's Law (crudely, the continued exponential growth of the number of transistors that can be crammed onto a computer chip). Did he believe he knew the right answer? He still does. In a response attached to the Beasley report, Schon admits having made mistakes, but writes "I have observed experimentally the various physical effects…such as the Quantum Hall effect, superconductivity in various materials…I believe that these results will be reproduced in the future…." Finally, is it a field in which results are not easily reproduced? It is. Results in this field are notoriously sample-specific. That is, they depend crucially on the skill and luck of the person who prepares the sample. Failure to reproduce any given result in any given sample is not considered proof of anything. Nobody could prove Schon had cheated just by demonstrating that a given result he has reported doesn't show up in a particular sample. So, my theory survives to be disproved another day.

The Schon case has put scientific misconduct back on the front pages of the newspapers, and this time it is physics that's on the firing line. Inevitably, there will be much debate and soul-searching about what to do. Whatever we do, we must remember this. Science is a marketplace of ideas, where good ideas must be proven wrong in order to be replaced by better ones. Being wrong, then, is an essential part of progress in science. To the public, it's easy to confuse being wrong with being guilty. We cannot allow that to happen. If scientists start to fear being accused of misconduct when they are wrong, enormous damage will be done to the enterprise of science.

In this case, the system worked. Science is self-correcting, as it's supposed to be. But we must not be complacent. If this kind of misconduct were to become commonplace, science would cease to be self correcting and would be no better than any other belief system. Rooting out scientific misconduct in a sensible way will always be a grave responsibility for all of us.

Today, September 27, 2002, the stock of once proud Lucent Technologies, the parent company of Bell Labs, closed at 77 cents a share. I bought a thousand shares.