Scientific credit and credibility

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The verdict returned by an inquiry into scientific misconduct by Bell Labs researchers left many in the community feeling stunned. Where do we go from here?

Those of us who have monitored, and participated in, the rapid advances in nanoscale materials in recent years are well aware of the exciting science and technological promise they hold. In the past three years, the extraordinary results from the Bell Labs research facility in Murray Hill, New Jersey, on field-effect-induced phenomena at the molecular scale seemed to epitomize the vision proclaimed by Richard Feynman that "there's plenty of room at the bottom".

But a certain uneasiness started to emerge as many groups worldwide, all capable in terms of experience and equipment, consistently failed to reproduce many of the results reported by Jan Hendrik Schön and his principal collaborators at Murray Hill, Bertram Batlogg (now at ETH, Zürich) and Christian Kloc. Very early on there was grumbling about the required magnitude of the gate-dielectric breakdown strength of the aluminium oxide layer they used, which some estimated would have to be an order of magnitude above bulk in order to induce the carrier concentration necessary for superconductivity to occur.

The credibility dam burst this spring when several researchers[1] attempting field-effect measurements according to the Bell Labs recipe noticed peculiar consistencies (and inconsistencies) in the graphical data published in a number of papers on different materials and sample configurations (see Fig. 1). These workers filed a complaint with Lucent management who quickly responded by forming an independent "investigation commission" chaired by Malcolm Beasley of Stanford University.

On 25 September 2002, the Beasley Commission issued its report[2] on "the possibility of scientific misconduct in the work of Hendrik Schön and coauthors", which found Schön guilty of misconduct on the basis of compelling evidence of data fabrication, unjustified manipulation and purposeful misrepresentation. This report, in my opinion, establishes a paradigm of thoroughness and fairness to be followed in such unfortunate cases. The committee cleared Schön's co-authors of misconduct, but left open the question of professional responsibility on the part of the co-workers (Fig. 2), Bell Labs management and the peer-review process. Reading between the lines, I have the strong impression that the committee struggled intensely with this question, and that there were divisions of opinion on how each should be defined.

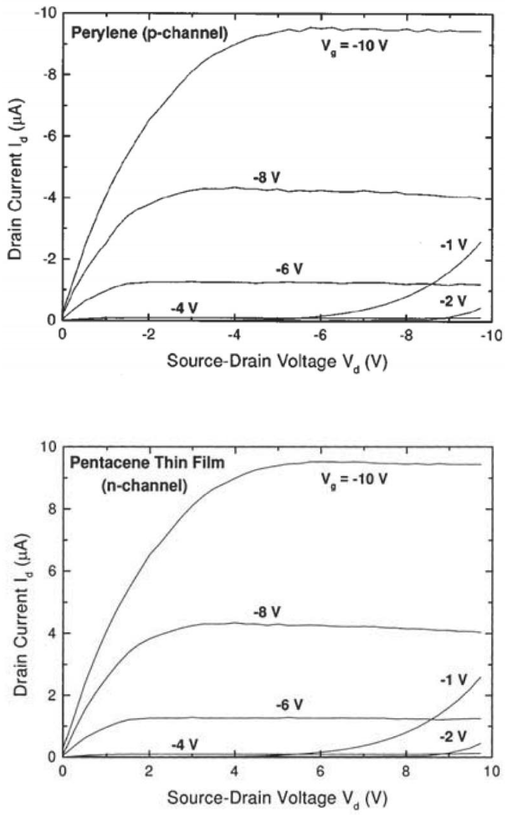

Figure 1 **Spot the difference. Striking similarities between data plots in several different papers led some researchers last May to question the work of Schön and colleagues. Here, data for two ambipolar transistors constructed from different materials (perylene and pentacene) and operating under different conditions look almost identical, including similar details in the random noise. The Beasley committee concluded that these published figures[4,5] contain identical data, despite the inverted scales, and are evidence of data fabrication by Schön.**

But first, the verdict rendered on Schön, sad and depressing as it is, was not entirely unexpected as more and more adverse findings were leaked throughout the summer months. Some, myself included, felt early on that there might be hope that unusual chemistry in the aluminium oxide film could produce fields sufficiently

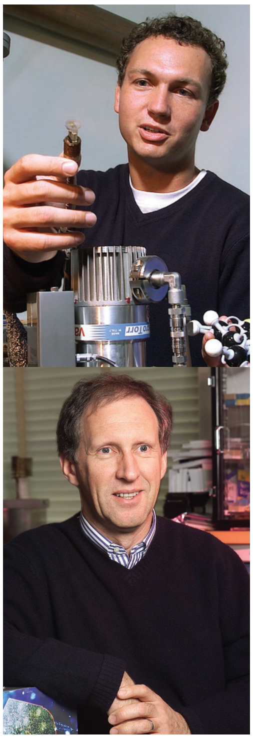

Figure 2 **How much professional responsibility do co-authors share for fraudulent results? The colleagues of Jan Hendrik Schön (top), including Bertram Batlogg (bottom), shared much of the credit for his discoveries, what portion should they also share in the shame?**

Photos: Bell Labs

high in the device channels to induce superconductivity. But, by mid-September, that hope became more desperate.

It is easy, indeed tempting, to take a superior, sanctimonious and condescending stance towards this incident. How could this have happened? Whatever could have been the motives of the principal investigator? Why were the co-authors and administration not more vigilant? I (that is, you, the reader) would certainly never behave this way! But it is one more reminder that throughout history, science has always been practised by imperfect and fallible human beings. We all have our flaws and most of us are lucky enough to escape circumstances in our lives that would expose them for all to see. I recall a common aphorism my Irish Catholic mother would often invoke when one of her friends would stumble on the moral pathway of life, "There, but for the grace of God, go I". Schön will be unlikely to find employment at an institution of note ever again, and the reputations of his co-workers will forever be linked to this imbroglio. That's sentence enough.

But why didn't the system work? Actually, it did. In a previous Commentary for *Nature*, I alluded to Michael Polyani's 'Republic of Science', in which all citizens participate in validation of the claims of each other[3]. The difficulties in reproducing Schön's experiments led to the investigation that has now 'voted him into exil'. Nevertheless, it's not unreasonable to further inquire, "Why didn't the system work sooner?" Several observers of this incident have concluded that there must be fundamental blemishes and warts in our present systems of institutional oversight and peer review. What are the professional responsibilities of those associated with a particular piece of research as co-authors and supervisors — what the Beasley report calls the first line of defence — and to what standards should they be held?

THE FIRST LINE OF DEFENCE

My entire career since graduate school has been spent in industrial research, almost all of it materials related. During that time, I have observed the scientific practice followed not only in my own institution, but other industries, government laboratories and academia as well. It has been my universal observation that the five or six major industrial laboratories in existence in the United States since the Second World War have been much better at assessing and preventing the release of experimental and theoretical results either simply in error, or by outright fraudulence, than their academic counterparts. One has only to point to the shenanigans of the cold fusion era as an example.

During my research career in the field of superconducting materials, I have documented many cases of an 'unidentified superconducting object' (USO), only one of which originated from an industrial laboratory, eventually landing in *Physical Review Letters*. But USOs have had origins in many universities and government laboratories. Given my rather strong view of the intrinsic checks and balances inherent in industrial research, the misconduct that managed to escape notice at Bell Labs is even more singular.

Why are universities more likely to err scientifically in public than industry? My belief is that it's inherent in the tenure system, which, under the cloak of academic freedom, impedes the setting up of effective internal peer-review processes before the release of results for publication. As any dean will tell you, individual professors are powers in their own right, and invariably resist intrusion from their fellow departmental colleagues. I can understand the necessity for academic freedom in politics, philosophy and the fine arts where scholarship is harder to quantify and judge, but for the physics faculty I never could fathom why in this day and age.

Industrial researchers, on the other hand, are employees of a corporation and fall under the traditional oversight and discipline associated with that culture. When serving in management, staff members in the IBM Research Division, where I spent the bulk of my 'productive' years, had the responsibility to clear for publication and patent protection the work of their team members. Should questions arise outside the manager's particular expertise or speciality, advice from others could be freely sought. One popular screening technique was the department seminar, which quite often resulted in more critical comment than the speaker would have liked. If questions of practice or reproducibility arose, managers could insist on sharing of samples or oversight of measurements by others.

At IBM, the final responsibility for quality control always fell to the manager. But the most important check in my opinion was the intense scientific curiosity displayed by research staff members both in and out of management when something especially exciting and unusual was afoot. During the discovery period of high-temperature superconductivity, we continually had people from levels up to the division director in our laboratories and offices. It is really hard for me to imagine pulling off something like Schön apparently did under such scrutiny. So what went so terribly wrong at Bell Labs? It seems that in some cases Schön was able to ignore many of the standard practices of internal review, even when the results were groundbreaking. The checks and balances existed, but they weren't sufficient.

CO-AUTHORSHIP CONDUCT

The complexity surrounding the issue of professional responsibility entailed by co-authorship emerges in any truly multidisciplinary venture. Before the Second World War, almost all collaboration that occurred within one's speciality reflected a communal sharing of inspiration and efforts resulting in a paper that each co-author — and there were seldom more than three — could equally defend. Today the prototypical co-authorship of a materials research publication from an industrial laboratory consists of a properties measurement physicist, a materials scientist specializing in analytical methods, a theoretician and a synthetic chemist. I liked to insist, as did my managerial peers, that each co-author be capable of defending the work at a professional society meeting of one of his colleagues. This practice may sound sadistic, but it sure worked.

I remember many years ago when our synthetic chemist at the time was invited to talk at an American Physical Society (APS) meeting on the Raman spectroscopy of one of his compounds, an experiment he did not perform himself. This particular individual, very defensive around physicists, was terrified, but with a month's tutoring delivered an outstanding talk, and was permanently cured of his 'fear of physics and physicists'. Likewise, I would on occasion have to deliver a talk to the American Chemical Society on sample synthesis, an area I viewed as pretty much a black art, but after watching what I had measured being prepared, felt confident I could repeat the recipe if necessary.

In March this year, one of Schön's colleagues gave a talk on sample preparation at the APS meeting in Indianapolis. Almost all the questions related to the physical measurements for which he had no answer — an experience likely to stick with him. I would maintain that the practice followed at IBM makes co-authors pay more attention to the work of their colleagues that they're signing up to support and, at the same time, expands their own technical vitality. The challenge for leaders of multidisciplinary laboratories and institutes is to ensure that fruitful interactions between disciplines can take place without neglecting the necessary scrutiny.

THE REPUBLIC OF SCIENCE

Finally, we have to address the much maligned peer-review process and the fact that the Schön papers sailed through it. To what extent do journal editors and referees possess professional responsibility for their decisions? Some, but not as much as the co-authors and institutional management. The quality of journal editors and referees and the attention paid to a given submission vary widely. Under the best of circumstances, peer review can improve the quality and impact of a manuscript by suggesting alternative interpretations and bringing unreferenced previous work to the attention of the authors.

But, as we all know, the thoroughness with which a specific paper is refereed depends very much on how close-to-home it falls — that is, how much the manuscript impacts the reviewer's current line of research. This is unlikely to change until professional credit is given to researchers who competently fulfil their duty as reviewers, something that would require a shift in attitude among the leaders of both academia and industry. In the case of the Schön papers, perhaps more careful scrutiny on the part of editors and referees would have helped. There is no indication that any of the papers investigated by the Beasley committee received allegations of misconduct before publication, so it is hard to see what could be done differently. Although the review process is anonymous, allegations of misconduct remain extremely rare, presumably because serious accusations cannot remain behind the cloak of confidentiality. The system is not designed to catch fraud, but it is not clear whether most journals have policies were such allegations to be made; something they may now want to consider.

So what did happen in Murray Hill that let this unfortunate episode unfold within the halls of the world's oldest and most illustrious industrial research establishment, one that throughout its history has produced six winners of the Nobel Prize in Physics? My guess, reinforced in part by conversations I've had with previous staff of Bell Labs, is that its upper management, even within basic research, has had to focus increasingly on the business issues of its parent, Lucent Technologies, a company currently under severe economic stress, and thus were distracted from activities going on in the trenches that did not affect the bottom line. That, and the temptation of Schön's otherwise innocent collaborators to 'see in the data what they wanted to see', brought this embarrassment on the condensed-matter physics and materials science community at large.

SCHÖN WILL BE UNLIKELY TO FIND EMPLOYMENT AT AN INSTITUTION OF NOTE EVER AGAIN, AND THE REPUTATIONS OF HIS CO-WORKERS WILL FOREVER BE LINKED TO THIS IMBROGLIO. THAT'S SENTENCE ENOUGH.

To its great credit, Bell Labs reacted swiftly to the allegations of scientific misconduct and began corrective action even before the release of the Beasley report. Whether economic conditions and changing corporate culture will allow Bell Labs to move beyond this incident and recapture its former stature as a leader in basic research remains to be seen. If not, I'm afraid, as I said in June this year[3], the bell may be tolling for the fate of basic research in industry at large. This would be a shame because academia could learn much from the business-like (as opposed to business-orientated) manner in which the most effective industrial labs are run.

JUDGE THE SCIENTIST NOT THE SCIENCE

I've heard some comments that the uproar over this newest 'corporate scandal' will diminish confidence in the viability and future of nanotechnology, resulting in reduced funding from research agencies and venture capital. The short answer to these views is simply, "don't be ridiculous". That's not to say that nanotechnology has not suffered its fair share of mega-hype, but the body of Schön's work, even had it been reliable, did not really have discernible immediate application. Moreover, it is important to note that the focus of the Beasley report was strictly on issues of scientific misconduct and professional responsibility; the science itself was not in the dock. On the contrary, to quote the committee's view directly on this issue:

"In the end, the correctness of the fundamental physical claims in the work in question will come through the normal processes of science — specifically through the reproduction, or not, of the results. On the basis of the evidence at hand, the Committee cannot exclude the possibility that some of the specific results claimed in the papers in question will someday be shown to be true."

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