

Looking back on the Schön affair

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As of this writing (early September, 2018), events of the past few weeks have brought to mind the notorious affair of Jan Hendrik Schön, and in discussing this with my students, I've realized that I should probably write down my memories of this episode before I forget even more details. Everything here is basically my recollections and opinions and any mistakes are mine. I have anonymized in places – I'm not writing this to embarrass people about things that happened years ago.

In October of 1998 I began a two-year stint as a postdoctoral member of technical staff at the Murray Hill, NJ location of Bell Labs, then owned by Lucent Technologies after being spun off from AT&T in 1996. Murray Hill doubled as Lucent's corporate headquarters. Lucent's business was largely telecommunications equipment and software, including a very large component of optoelectronics research, development, and manufacturing. The idea was that, as a part of AT&T, the market for Lucent's products was limited to AT&T, but after standing up as an independent company, it would be possible to expand and prosper by selling products to AT&T's competitors as well. This was in the heyday of the first big internet boom of the 1990s, when there was explosive growth of internet traffic, with zillions of km of fiber optic cables being laid down. Data traffic blew past voice traffic as the dominant source of packets transiting the world's fiber optic networks. People openly discussed the idea that wireless networking might become good enough to allow streaming of video someday (!), "fiber to the home" was a phrase that showed up in powerpoint slides, and somehow venture capitalists decided that it was a good idea to fund companies like teacozies.com, which would change the B2B and P2P sales of tea cozies. Who could have foreseen any problems looming on the horizon?

Lucent included Bell Labs, the famous research and development component that had originated back in the early 20th century as part of Western Electric, the hardware manufacturing piece of Bell Telephone. Bell Labs was an amazing place, the result of decades of Bell's monopoly on long distance phone service and associated hardware, and had been responsible for some incredible advances, including the invention of the modern transistor and the discovery of the cosmic microwave background. The recent book *The Idea Factory* does a pretty good job telling the story of the place and giving a sense of the culture there. The phone company roots still ran deep when I got there. The internal phone numbers all started with 582, which matches up with "LUC".

The traditional idea behind Bell Labs was to have a stable of very smart, creative people, who would spend a good chunk of their time working on research intended to be of commercial value to the company. These people were also permitted and often encouraged to do relatively fundamental research – that is, work that was not going to be of immediate or even medium-term application, but could lead to valuable insights or intellectual property. This helped keep the researchers fresh, at some rate produced high profile science that could be valuable as public relations and advertising for the company, and meant that the company had a critical

mass of experts in certain areas (materials growth, for example) to take rapid advantage of sudden opportunities or to tackle high priority technical problems for the business units. Under the Lucent regime, this was meant to be sustained by around 1% of revenue being targeted to the Labs.

All of this was going on during the big stock market runup of the 1990s. With the big shift to institutional investing and internet growth stocks, times were definitely changing from the days when some quiet family investor would buy AT&T stock and hold it for thirty years. Some compensation was being offered to people at Bell Labs in the form of stock options, and upper management (including people like Arun Netravali and Carly Fiorina) seemed to be extremely concerned about hitting earnings expectations to keep the stock buoyed.

The physical sciences part of Bell Labs was organized into departments, and within these departments were some number of scientists and engineers who functioned as “principal investigators”, basically running particular research efforts. The standard title was member of technical staff (MTS). This was a pretty flat structure, though there were a handful of distinguished members of technical staff (DMTS), such as Walter Brown, one of the best semiconductor device physicists of the century. A typical department might have ten to twenty MTS acting as PIs, but individual research groups could be quite small – maybe one scientist, a technician, and a postdoctoral researcher, or the occasional summer student. Alternately, efforts more closely tied to the business could grow quite large. The quantum cascade laser effort helmed by Federico Capasso had several scientists and engineers involved (Claire Gmachl on the device side, Al Cho and his team on the molecular beam epitaxy front).

Department heads were scientists and engineers who were also running their own research efforts. (I’m going to use the term “department head” because that was the title in use when I started there; over the subsequent years there was a lot of title inflation.) Department heads were the ones who had to argue for headcount and budget. There were no research proposals or the like within the Labs, but there had to be justifications for major expenses such as big pieces of equipment, liquid helium, and personnel – getting a postdoc slot or a technician required the MTS to make a convincing case to the department head, who in turn would have to do budgetary battle with the lab director and on up.

Bear in mind that department heads generally had no particular management training. It should not surprise you to learn that not everyone who is an accomplished scientist or engineer is necessarily an effective manager of personnel, and sometimes the people who want the most to be department head are not the best choice for the job. Still, as is true in many such systems, when the overall picture is rosy (company stock going up, comparatively gentle budgetary pressures on the Labs, no big layoffs looming), things can work pretty well.

There was no such thing as tenure, of course – as my postdoctoral mentor Bob Willett explained to me when I was interviewing for the job, “This is the greatest place in the world to work, except that they could decide to fire us all tomorrow.” Raises, promotions, reallocation of directed effort – these were decided through an annual merit review process, involving

presentations by the MTS and meetings between the MTS and the department heads. There were a couple of running jokes about this. One classic was the inevitable claim that despite a great fiscal year for the stock price and wonderful technical output, a MTS would be told, “Well, so-and-so, I’m afraid you picked a bad year to do well”, and be given the standard cost-of-living raise or less. There was a certain gallows humor about merit review. Wholesale termination for poor performance was rare, but continued support in the form of space, equipment, and personnel resources was contingent upon some combination of scientific output and impact on the company.

There was a regular flux of postdocs (PMTS) through the labs. Some could be tapped by management for recruitment into “permanent” positions, if they really looked like they brought key skills and talent, and perhaps scientific star potential, but this was comparatively rare. At one point in my time there, I shared a small office with three other postdocs. (The office was so small that all four of us could not actually be seated in there simultaneously, because our desk chairs would collide, but that was ok since there was always someone out working in their lab.) Only one of the four of us got an offer to stay as a MTS, and that’s probably higher than the average rate.

In the absence of overall growth, the openings for MTS positions came through attrition, either by retirement or by departure. Since the 1986 breakup of Bell Telephone, there had been an uptick in former Bell Labs scientists and engineers moving to academia. My graduate adviser, Doug Osheroff, was a successful example. Recruited to Bell Labs from the Cornell low temperature physics group in 1972, Doug went to Murray Hill and worked there for fourteen years, including several years as a department head. In 1986 he moved to Stanford, where he became a professor and greatly enjoyed working with students until his retirement. Similarly, at around the same time Steve Chu, a key figure in the development of laser cooling and trapping in atomic physics, also left Bell Labs and moved to Stanford. Art Gossard had gone from Bell Labs to a hugely prolific career as a materials physicist and grower at UCSB. Jim Eisenstein moved from Bell Labs to Cal Tech in 1996 coincident the Lucent spinoff. There were others who were less successful at making the jump to academia – after years of not having to write grant proposals and working with highly trained technicians, the transition to the academic research environment could be rough.

Typical postdoc appointments, at least ones funded internally, were for two years, with no real possibility of extension. That makes for a very compressed timeline if the plan is to end up in an academic job. The faculty job market is highly seasonal, tied to the academic year. For most disciplines, faculty job interviews are in the winter and early spring, for positions that are intended to start the following fall. Starting my postdoc in October of 1998 meant that I would probably have to have to be ready to apply for faculty jobs in the late fall of 1999, barely a year away. Unless I wanted to rely primarily on my PhD work for my application, I would need to have some kind of publishable results, at least a paper or two *submitted* if not published, by then. That’s a tall order.

In that situation it helps greatly to join a PI or group who already has a project underway, rather than starting from scratch. In my case, I was going to work with Bob Willett, one of the world's experimental experts on composite fermions and the quantum Hall effect. That physics involves electrons confined in two dimensions, at the interface between GaAs and AlGaAs layers in structures that could be grown exquisitely, one atomic layer at a time, by our collaborators Loren Pfeiffer and his amazingly talented technician colleague Ken West. (One big lesson I learned to appreciate at Bell Labs: Technicians and research scientists can be every bit as intellectually deep as PIs, regardless of the lack of a doctorate or the choice not to be an independent researcher.) I wasn't going to do 2d electron physics, though. (This is in keeping with my graduate career, when I worked for one of the world's greatest experts on quantum fluids and ^3He , and did not actually do a thesis about ^3He .) Bob, with a previous postdoc Lydia Sohn, had played a bit with an idea of using MBE-grown substrates and chemically selective etching to produce atomically precise templates for materials deposition. That was going to be my project. I walked in the door never having done any nanofabrication, and within a few weeks I was submitting abstracts for the 2000 March American Physical Society meeting stating that I would present preliminary results of fabrication and electronic measurements of ultrathin nanoscale wires produced with this approach. Nothing like a little additional self-imposed pressure to focus the mind, as it were. Every week that passed was around 1% of my postdoc. I'd just gotten married in September as well. This should give you a bit of a sense of the intensity of the experience.

A few days after I started at Bell, there was a heck of a party when it was announced that Bell Labs physicist Horst Stormer, co-discoverer of the fractional quantum Hall effect, would share in the 1998 Nobel Prize in Physics. Stormer was a very impressive figure, who at the time was 49, and in the process of transitioning from Bell Labs to a senior faculty position at Columbia University. The atmosphere was joyous. This was the sixth Nobel for Bell Labs researchers, and the second in a row, since Steve Chu had a piece of the 1997 prize for his work at Bell. This was getting surreal for me, as it was the third such party I'd been to in as many years! My own thesis adviser had shared the Nobel in 1996, to a great fanfare and celebration at Stanford. Then Steve Chu had won in 1997, leading to another celebration at Stanford. Now the party was for Horst Stormer, Dan Tsui – a really nice, quiet, modest, brilliant experimental physicist then at Princeton – and Bob Laughlin, a larger-than-life theorist also at Stanford, famous for deliberate pugnaciousness and writing down the wavefunction for the fractional quantum Hall state while sitting in a trailer at Livermore waiting for his security clearance. The Lucent leadership trumpeted the Prize as a demonstration of the continued excellence and legacy of the company.

After this fun start, I got down to business, working long days learning all about electron beam lithography, electron microscopy, and cleanroom fabrication. I got to know my officemate and friend, Mike Manfra, who was working with Pfeiffer and West learning about growing the best GaAs 2d electron gas in the world while trying to figure out how to grow cubic gallium nitride. Despite having a bit of a fearsome reputation as someone who didn't suffer fools gladly, Bob Willett proved to be a really great mentor, teaching me a lot about how to do challenging experimental work in a very self-reliant way. On my second or third day, Bob introduced me to

our department head, Federico Capasso, the inventor of the quantum cascade laser and a manic guy with frizzy, graying curls who literally could not sit still in meetings. We were in the Semiconductor Physics department. In the hierarchy, above Capasso was Cherry Murray, an expert in colloids and complex fluids who was division director for Physics research; above her was Bill Brinkman, condensed matter theorist and VP for Physical Science research, and the organization was capped by Lab president Arun Netravali, a computer engineer whose most famous affectation was allegedly flying in the corporate helicopter to attend the US Open tennis tournament.

One aspect of the place that I really miss is (some of) the interactions between the people. Universities tend to be vibrant places, with a large variety of people around – students as young as teenagers, faculty and staff, and a whole university culture and identity. Even within a single department, there are people with a broad spectrum of interests, and in modern research universities it's rare to find too much overlap in the research programs and expertise of the faculty. Sure, people collaborate, but the whole university research culture selects for individual, independent researchers. That can be great in many ways – I can go to my departmental colleagues and learn about planet formation or the physics of cancer biology – but it does miss out on a key aspect of the industrial lab, having a serious group of PhD-level people with a body of common knowledge. At Bell Labs I could have lunch with a big group of PhD condensed matter physicists, materials scientists, and electrical engineers (and contrary to popular myth, that isn't a socially unpleasant thing). Conversations could be fun and casual, with people busting each other over sports teams, but just as easily the discussion could range over the latest technical topic from the lab or from the literature. The same thing happened at afternoon coffee, which became a daily ritual for several of us.

I first met Hendrik Schön a couple of months into my postdoc. Schön had come to Bell Labs as a postdoc a few months before me, and was working with Bertram Batlogg, who was also the head of the Materials Physics department. When I met Schön, he was a quiet, nice guy who I'd say "hi" to in the hall at some rate – he was working in a lab downstairs from Willett's lab, down and across the hall from the communal printer room. He was of medium height, with sandy, curly hair, and at the time all I knew was that he was one of a group of people who would play soccer for a bit on the field outside by the helipad after lunch some days.

Bertram Batlogg was a very respected scientist. An Austrian with a graduate degree from ETH in Switzerland, Batlogg had done work on a whole variety of "strongly correlated" materials, systems in which electron-electron interactions can be extremely important. Competing electronic and magnetic states vie for control, and Batlogg had been very successful in tuning dramatic properties like superconductivity and magnetism through control of material composition. A smooth, urbane person, he had a reputation for being a canny experimentalist with great tenacity. A case in point: C. W. "Paul" Chu and collaborators at the University of Houston published a discovery paper on the famous high temperature superconductor YBCO six weeks ahead of the Bell Labs group. However, the Bell team (led by Bob Cava and Batlogg) ended up with the patent on the material, after a protracted court battle, because they were

first able to demonstrate that they had single-phase material with a known structure and composition.

Bear in mind that there was a fierce competitive, secretive streak to many who came up scientifically through the high-T_c craze. The potential stakes were enormous, if someone could discover a very high temperature superconductor with useful properties. Materials physicists tend to be clever and creative, and at the same time have laboratories containing a very large fraction of the periodic table and multiple crystal growth instruments of various kinds. Given information about possible composition of a new compound, these folks are often well positioned to make an initial attempt at a new material in only a couple of days. The high stakes and experimental nimbleness were not a good mix. The story circulates that when Paul Chu and collaborators submitted their manuscript to Phys Rev Lett about YBCO, they deliberately mis-stated the composition of the material, writing “Yb” (ytterbium) instead of “Y” (yttrium), because of concern that the referees would take the manuscript and use the information to scoop the discovery. This was the kind of scientific environment in which Batlogg had been working for over a decade at this point, and I suspect it contributed to the bad social dynamics later on.

I didn't really interact much with Schön during much of my first year. I'd see him around, and occasionally I'd be in a conversation (or listening on the periphery of a discussion) with Batlogg and others at seminars. As time went on, it was an open secret that, now that his kids were out of school, Batlogg was looking to move back to Europe and had been working on securing a senior position at ETH Zurich. As a result, he was travelling quite a bit and was less of a visible presence, and somewhere in there I'm pretty sure the department leadership was reorganized in advance of his planned departure.

Sometime around the summer of 1999, I remember learning about what Schön had supposedly been studying: Making transistors based on organic semiconductor crystals. Christian Kloc was a physical chemist with an expertise in growth of large organic molecular crystals. Batlogg had recruited Kloc to Bell to work on organic semiconductors. This was an area of possible technological interest to the company (and organic LEDs, for example, have transformed display technologies), and connected to fundamental questions about charge flow in such systems. Other work at Bell Labs, IBM, 3M, Xerox, Ericsson, and academia had been concentrating on either solution-processed semiconducting polymers or vacuum-deposited thin polycrystalline films of organics. In those systems, charge transport tended to be poor, as quantified by the carrier mobility (ratio of carrier drift velocity to applied electric field). In the favorite units of the time, typical mobilities of organics were around 0.01 cm²/V-s, at best. For comparison, the mobility of carriers was more like 10 cm²/V-s in amorphous silicon, 1000 cm²/V-s in single-crystal silicon, and at low temperatures in Loren Pfeiffer's GaAs/AlGaAs structures, 10,000,000 cm²/V-s.

What this really meant is that charge typically moved through the organic semiconductors by a thermally activated hopping process, unlike the wave-like situation in inorganic crystals. It had been shown in the 1980s, though, that small molecule organic semiconductors could be

purified by successive sublimation, and that mobilities of several hundred $\text{cm}^2/\text{V-s}$ were possible in large single crystals of these. Kloc was going to crank this up to the next level. This isn't easy, in part because the chemical synthesis working definition of "pure" is something like 1% unintended product, while the silicon electronics definition of "pure" is one impurity atom per hundred billion.

On the physics side, earlier work on doping crystals of C_{60} had shown superconductivity. These molecular crystals, held together weakly by van der Waals interactions, should have comparatively narrow energy bands and relatively low dielectric constants, implying that it might be possible to have electron-electron interaction effects that are strong compared to the kinetic energy of charge carriers. Because the building blocks of these crystals are chemically "happy" molecules, it seemed intuitive that these crystals might be comparatively free of the surface states and charge traps that arise in ordinary crystalline semiconductors from dangling bonds, atomic vacancies, and impurities.

Kloc was handing off crystals to Schön, who was supposedly fabricating electronic devices out of them – two-terminal structures for measuring basic IV curves and looking at space-charge-limited currents, and then three-terminal field-effect transistors – and measuring these. Note that handling crystals like this and getting reproducible results turns out to be very challenging. The materials in question were waxy and brittle, and making good contacts between metal electrodes and these molecular systems was a bit of a black art. Anyway, data appeared. Papers began to be submitted by Schön and company and appearing in print.

As has been pointed out by others including the investigating panel and Eugenie Reich, getting a paper out of Bell Labs was *supposed* to be more difficult than in a university. In academia, I can write up and submit a manuscript to a journal without having to show it to anyone or getting anybody's approval. Of course, as a postdoc I had my co-author and mentor read the manuscript – we discussed which journal would be most appropriate, whether the analysis was correct, and whether I needed to take some more data or include a micrograph to clarify a figure. That goes on at universities, too. At Bell Labs, though, my draft paper was then sent to someone else in the division, not directly connected with the work, for a once-over before it could be submitted. In my case, for the three first-author papers I authored as a postdoc, that person was Don Monroe. The point of this review process was not to slow things down, but to improve the papers through a bit of internal refereeing, and as an industrial lab, to confirm that no opportunities were being missed in terms of potential intellectual property.

By late 1999, Schön had been apparently quite productive, producing half a dozen papers over about a year and a half, looking at electronic properties of devices based on Kloc's single crystals. It sure looked like these materials were going to lead to great things, as the reported results indicated mobilities in the hundreds, and climbing with decreasing temperature, an indicator of clean, band-like transport! These positive material properties and Schön's impressive productivity, combined with Batlogg's endorsement, were enough to net Schön a staff position, transitioning from postdoc to full MTS. By this time, the lab had reorganized, and Schön was in John Rogers' department.

By summer of 2000, that hiring decision looked like a very good one, as Schön had published a Science paper and a Nature paper in January and February, respectively, on comparatively conventional semiconductor devices based on crystals of pentacene. In April, he published a Science paper reporting field-effect-modulated superconductivity (!) in devices based on molecular crystals of C₆₀. The claim was that this superconductivity set in at extremely high gated charge densities at the material surface. This was plausible, because it had been known since 1991 that it was possible to make bulk C₆₀ crystals superconduct with sufficient *chemical* doping, with a seemingly ideal number being about three chemically-added electrons (donated by alkali metal atoms like potassium) per C₆₀ molecule.

Getting electrons-per-molecule charge densities in a field-effect device was a remarkable claim in and of itself! In an accumulation-mode FET, the channel is made by using the gate electrode as one plate of a capacitor, with the semiconductor surface of the channel as the other plate. Applying a positive voltage to the gate relative to the source and drain electrodes (and therefore the channel) capacitively sucks a layer of electrons from the source and drain into the channel. In an ambipolar device, the other polarity also works, and a negative gate voltage accumulates a layer of holes in the channel. The maximum achievable charge density that you can get for a choice of materials is set by the capacitance per unit area (proportional to the gate insulator's relative dielectric constant) times the maximum dc electric field that you can put across the insulator before it fails. Silicon dioxide ($\kappa = 3.9$) has great properties and can sustain an applied electric field of around 10^9 V/m before breakdown. That leads to a maximum charge density of $((8.85 \times 10^{-12} \text{ F/m})(3.9)(1 \times 10^9 \text{ V/m}) / (1.602 \times 10^{-19} \text{ C/e})) \times (1 \text{ m}^2 / 10^4 \text{ cm}^2) = 2.1 \times 10^{13} \text{ e/cm}^2$, though that's very hard to achieve in practice. Schön was claiming to achieve charge densities more than ten times higher than this (!) by using amorphous Al₂O₃ as the gate dielectric (κ closer to 9), and with a breakdown field higher by a factor of several than any ever reported for aluminum oxide.

This supposed dielectric is a key part of the story. The experimental claims were all related to a regime of charge density that was literally unreachable by anyone else in the world at the time. That is one reason why no one else could immediately go to the lab and confirm or refute the claims. Schön, who had done his doctoral work on semiconductors and photovoltaics at Konstanz in Germany, said that he was growing the aluminum oxide at a particular sputtering system at his old alma mater.

When asked about this amazing result, Batlogg was downright coy, and implied that there were more exciting developments where that came from. It was unclear to me exactly where and how the measurements were being done; I assumed in the lab below mine.

At the time, I didn't really have any time to sleuth around about the details, as I was deep in the process of completing my faculty job search. In classic irony mode, Lucent had not been particularly interested in hiring me to a full MTS position until about the same week that I received my final offer from Rice. This is a standard issue in academia as well: Perceived market value of an employee is often driven by external offers, not internal evaluations. It also

came across from a couple of levels up that my productivity level, while fine, wasn't in the same league as that of some others (Schön) who had made the transition to MTS, though perhaps that was not an intended message.

Then something even more extraordinary happened: Schön had a paper appear in *Science* in June, claiming to have observed the quantum Hall effect in both electrons and holes, in field-effect devices based on pentacene crystals. This was a remarkable claim for multiple physics reasons. First, it's actually tough to make transistors that show "ambipolar" response, the ability to accumulate either electrons or holes depending on the sign of the gate voltage. Schön's earlier *Science* paper had claimed to show ambipolar response already. Still, the claimed device performance was amazing, since the contacts were supposedly made by evaporating gold directly on the organic crystal surface (hot gold vapor hitting an organic that decomposes above 250 C), and then sputtering amorphous aluminum oxide directly over the organic surface and contacts. The new paper also basically claimed that the effective cleanliness of the pentacene surface (which had been supposedly been directly exposed to argon plasma and sputtered Al₂O₃) was as good as those GaAs/AlGaAs structures I'd mentioned. Not only was there apparent electronic and hole band response, but charge carriers could move so well that the dominant physics was their cyclotron motion (the quantum Hall response), not anything to do with disorder in the organic or at the surface. Indeed, these devices supposedly showed *multiple* fractional quantum Hall states, a response that is very fragile even in atomically perfect GaAs/AlGaAs and Si devices.

This was also a shock within my part of Murray Hill, because this result appeared out of the blue in *Science* with seemingly no one knowing about it until publication. Bear in mind that Bob Willett was one of the world's experimental experts on the quantum Hall effect, as was Kirk Baldwin, Horst Stormer's technician who was still there, working with Stormer's Columbia students and postdocs while Stormer's lab in New York was still being set up. Steve Simon was a theory expert on the subject. None of these people had seen the paper before it had gone out; nor had Schön or Batlogg (traveling a ton) given an internal seminar or talk about it.

By this point, I'd already accepted my assistant professor position at Rice. I was trying to finish up some projects while planning in more detail what my initial research program would be once I got to Houston. This organic semiconductor work was intriguing, but it was clear that learning to grow single crystals of organic molecules like Kloc would be a big challenge, one I probably didn't want to take on right out of the gate. I moved in August and started my first semester as a faculty member, frantically trying to do several jobs simultaneously (design my lab, order equipment, teach a class, hire my first grad student, write proposals).

During that fall, the Schön bombshell results started to flood the literature at a rate that even then should have been realized as absurd. Days after I arrived in Houston, a *Nature* paper appeared reporting superconductivity in organic FETs based on Kloc's single crystals of anthracene, tetracene, and pentacene, seeming to imply that there was nothing special about C₆₀ – lots of molecular crystals could apparently superconduct if only you could get enough charge in there.

After this, Schön really ramped up the pace. There was a Science paper in July about an electronically driven organic laser (!) with Ananth Dodabalapur, an organic electronics expert, as co-author. Not just a light-emitting diode, mind you, but a laser. In November, there was a follow-up Science paper with the same author list reporting a light-emitting organic field-effect transistor. November also saw the published claim that, with *hole* carriers rather than electrons (again at a crazy high charge density), C₆₀ could superconduct with a transition temperature as high as 52 K. This doesn't even count the additional *eight* papers that year in journals like Advanced Materials, Applied Physics Letters, and Synthetic Metals.

This should have raised red flags from many people, if someone was actually keeping track. It's a level of output that is simply absurd for someone who was allegedly functioning as basically a one-person research group. Schön didn't have a technician; he didn't have a postdoc; he didn't have visiting grad students. How was he actually able to fabricate devices (supposedly involving transatlantic travel for the dielectric deposition!) and measure all of these things, get publication-quality data, analyze the data, and write up the manuscripts at this rate? We now know the answer: He didn't.

Were these papers being reviewed by *anyone* at Bell Labs before they got out the door? Unclear, at least to me. Batlogg had transitioned to ETH full time by September, 2000, into a high-level position that sounds like an incredible sinecure to many physics faculty in the US, in that it came with permanent technical support and guaranteed funding at some level without grant proposals.

It's important to realize that around this time was the beginning of serious corporate turmoil at Lucent. Sometime in 1999 (I believe), some business types had a realization. The argument for Lucent spinning off from AT&T was so that they could sell technology to competing long-distance/internet providers and make even more money. (In 2000, Lucent had already spun off its slow-growth phone business as Avaya, partly because this wasn't viewed as a high-flying direction that would act like a growth stock.) The same idea now popped up about optoelectronic and networking components: If the components people could be spun off so that they could sell not just to Lucent, but also to competitors, everyone could get rich. JDS Uniphase, another opto component company, was seeing its stock price double every 90 days (!). So, the wheels were set in motion to create Agere. Some decent fraction of the physical sciences and electronics people from Bell Labs were likely to go with Agere when the spin-off happened. There was real uncertainty about who would stay, who would go, how the labs would be reorganized, etc. The Bell Labs management had a lot on their plates.

To make matters much worse was the one big problem with the Agere plan: Once the spin-off process got underway, at some point it became unstoppable, and right in the middle of all this, the telecom crash started. Lucent upper management (people like Fiorina and her contemporaries, McGinn (COO) and Schacht (CEO) as prime examples) had already been doing some weird things to goose the stock price (and hence their options), like lending money to customers who would then buy Lucent products to keep up the quarterly sales numbers. The

stock started to fall in the 2000 after hitting its all-time peak in December of 1999, and by 2002 the price had fallen by a factor of about 40. There were serious technical people who got spun off into Agere and then immediately laid off. I'm not making excuses for any particular people in Bell Labs management, but when people ask rhetorically, "How could there be so little oversight of Schön? How did this happen?", it's important to remember the business context: Just as Schön shifted into overdrive from (apparently) very productive to "ludicrous speed", from a management perspective, the corporate walls were on fire. Schön was only one of 20 researchers in one department out of many, in a place with a long-established culture of researcher independence.

(In my view, there are serious problems with corporate governance in some companies, especially as practiced since the 1990s. Upper executives in such companies are short-timers looking for the next big thing. They are only going to be in their role for three or four years, and there can be enormous financial incentives to maximize instantaneous appearances of success with no regard at all for whether the company will still exist in ten years. Corporate boards that allow this are terribly negligent, but in these same poorly run companies, the board members are also often not there for the long haul. Institutional investors like mutual funds tend not to care about the long-term fate of individual companies. This can all combine into a toxic economic mix that incentivizes bad behavior. I wish I had a realistic fix to suggest. Thankfully most companies are not run this way.)

While I was getting my research started at Rice, I was also looking for ways to branch out beyond the nanoscale mesoscopic physics I'd been doing. I had been looking at molecular-scale electronics for a while, and started working in that direction. Organic electronics seemed like an exciting area, though looking at Schön's work, I was leery of trying to ramp up anything that apparently required two technical breakthroughs (high quality single crystals of molecules, and the wonder-dielectric). Then in March of 2001, Schön et al. published a paper in Nature claiming superconductivity in organic field-effect transistors based on a semiconducting polymer, poly(3-hexylthiophene) (P3HT). That made me sit up and take notice, because reasonably high quality P3HT was available commercially – I could just buy it, so that left only one hurdle, the dielectric. I decided to try to work on this. I figured I could try collaborating with someone who could make dielectrics, like my then-faculty-colleague Susanne Stemmer, who had an excellent oxide sputtering system and could make Al_2O_3 as well as high-k dielectrics like strontium titanate and lead magnesium niobate. Likewise, I contacted Paul Clem, a materials scientist at Sandia with an expertise in very high-k ceramic films.

I jumped into this pretty seriously. In late May of 2001, I emailed both Schön and Batlogg, independently, about how they were making these apparently world-beating Al_2O_3 films. Schön's reply was friendly and had technical details, like rf power, source-sample distances to try, etc. In hindsight, of course, these all appear to be fiction. Batlogg's reply was cagey and implied that he would be happy to talk after they eventually published their own paper on the dielectrics: "As you understand, this is obviously an important aspect of the work and we would like to push it further. I suggest you contact me later and I will be glad to give you then

the current state of affairs.” Spoilers: There never was a paper about alumina dielectrics from Schön and Batlogg.

My second grad student, Behrang Hamadani, came to Rice over the summer of 2001 before officially matriculating, and right away we got to work. It still amazes me that it’s trivially easy to make (not very good) polymer transistors. Start with a highly doped Si wafer to use as a back-gate, and have it coated with good SiO₂. Use your favorite patterning method (we started with simple stencil masks) to put down gold source and drain electrodes. Drop-cast P3HT dissolved in chloroform or chlorobenzene onto the surface. Voila, you’ve made a p-type organic FET. Behrang and I spent a bunch of time that summer and fall making P3HT FETs, trying to figure out what it took to get reproducible results (that is mobilities that didn’t vary apparently at random over three orders of magnitude, from terrible, 10⁻⁵ cm²/V-s, to merely bad, 10⁻² cm²/V-s). I also wrote a NSF proposal, laying out my plans to look at this stuff, and submitted it at the beginning of November.

At almost the same time I was submitting that proposal, Schön published, along with co-author chemist Zhenan Bao, another Nature paper in October, this one claiming to have made field-effect transistors in which the channel was a self-assembled monolayer of molecules. The supposed device involved running a gold source electrode right up against an oxidized, etched step in a doped Si wafer. Then a molecular layer was self-assembled on the source, again supposedly right up to that step edge, and a top gold drain electrode was evaporated onto this monolayer. Supposedly there were not issues with shorting through the monolayer, and the gate allegedly affected the monolayer within a few nm of the step. This was followed up by a Science paper in December, this one claiming that by substituting molecules into the monolayer, it was possible to discern field-effect transport through individual substituent molecules.

When I saw these papers, I pretty much could not believe what I was reading. There were many issues that jumped out at me. For example, the devices seemed to act like full-sized FETs in terms of their electrical characteristics (e.g., I-V curves that saturated at high source-drain voltage, with saturation currents varying roughly like gate voltage squared) despite the fact that the physics in the monolayer would have to be completely different than what goes on in a bulk semiconductor. The devices were unfabricatable as described – you just could not make something like that and have it not short through. Even if you could make such a structure, the electrostatics of the devices made no sense; it should not have been possible to have the gate controlling the conduction in the channel as described. There were no images of the actual devices, and the diagrams were so completely out of scale with the reported geometry that they were outright misleading.

Right after the first of these papers was published, I went to a planning meeting at Sandia for the soon-to-begin DOE nanocenter (Center for Integrated NanoTechnologies) joint between Sandia and Los Alamos. Flying into Albuquerque in early October, 2001 was a weird experience. The 9/11 attacks had only been three weeks before, airport security in Albuquerque was being

handled by uniformed National Guard troops with automatic weapons, and the Air Force base adjacent to the airport was flying combat air patrols.

At the CINT meeting, I ran into a highly regarded condensed matter theorist, who had not yet left Bell to go to academia. I mentioned my issues with these latest papers to this theorist, and he didn't seem very concerned. At all. Thinking back, this is the first time I ran into what I later called the "But he's a genius" phenomenon, which seemed to afflict some very smart, talented scientists who had been near Schön's orbit. When confronted with concerns about Schön's work, a standard response was along these lines: Ok, maybe this particular issue you mentioned is unclear or needs to be addressed, *but he's a genius!*

In January of 2002, I managed to swing by Bell Labs on a trip back east. The atmosphere was not happy. The stock had plummeted and layoffs were in the air. You know a company is having financial issues when they decide that they should turn off half the hallway lighting to save energy, and they decide that they really only need custodial services in the rest rooms every couple of days. It was great to talk to my friends and colleagues who were still there. At lunch in the cafeteria, with several of those folks, I brought up the subject of these latest papers, and the fact that no one had been able to reproduce any of the other work yet. I said, completely joking but genuinely frustrated, "So, is all this stuff made up, or what?" This was met with nervous chuckles and exchanged glances. This whole area was a sore subject, and there must have been unhappy discussions among various people internally about the work, particularly how these papers one after another were ending up in print when there were technical questions that should have been asked in-house.

The big APS March Meeting of condensed matter and materials physicists was in exotic Indianapolis that year. The first day of the meeting, I gave a ten-minute contributed talk about the progress that Behrang and I had been making with P3HT FETs. The room was pretty packed, and Batlogg was there. We'd found big variability from device, but there were some interesting points: Larger gate biases (higher carrier densities) did give larger mobilities; contact resistances were hugely important; and we found a negative linear magnetoresistance in this stuff near room temperature, though I was never happy with drifts in those experiments and others eventually published much nicer data. I still remember Batlogg chatting with me after the talk, being encouraging and basically saying, don't worry, you're making good progress and will get there soon.

At the meeting there were all kinds of stories flying around – that Schön and Batlogg had been nominated for the Nobel Prize, that Schön had been offered a big endowed professorship at Princeton, that Schön was being considered for a Max Planck directorship back in Germany. At the same time, no one had been able to reproduce *any* of the results, even the ones that didn't seem to require exceedingly high carrier densities.

Near the end of the meeting, there were rumors that someone I knew had had some success in replicating the dielectric to at least get close to the alleged charge densities. I spoke to this person and asked if he'd be willing to coat some substrates for me to try. His response was

interesting: He'd already spoken with a very senior, prize-winning faculty member at a big-name school, Prof. A, about substrates for polymer devices – perhaps I should contact Prof. A and see if there were opportunities to collaborate, because the grower didn't want to give me substrates as well without Prof. A's ok. I thought that was odd – lots of material growers share samples all the time with many groups, some of which are competing. Still, I did as had been suggested. When I got back to Houston I emailed Prof. A, very deferentially, mentioning that I was just starting out, that the grower wanted to make sure that Prof. A was ok with the sharing of substrates, and offering to collaborate if that was of interest. Prof. A wrote back very bluntly, pointing out that I had nothing to offer in the way of collaboration that he didn't already have available, and no, he was not comfortable with the grower sharing substrates with me. I guess I should have been flattered, since Prof. A seemed to consider me a real competitor. In the end, this was all irrelevant. The grower had been mistaken in the first place, and within two months the whole Schön edifice was going to crash down.

In mid-May, I got a phone call at work from Bob Willett. He told me that there were apparently terrible problems with several of Schön's papers – it had been noticed by Lydia Sohn (then at Princeton) and Paul McEuen (at Cornell) that there were clear cases of suspicious figures, relabeled in different papers but clearly the same data replotted. To say I was shocked was an understatement, but once you actually look at the figures side by side, there is no doubt at all that there are problems. I found a few more that hadn't been pointed out yet once I started looking, and forwarded those on to Don Monroe, who was serving on the hastily convened investigative panel that Bell Labs pulled together. Most embarrassing to me was that I had not spotted an example of figure manipulation in that P3HT superconductivity paper: The two resistance vs temperature curves supposedly taken at the highest gate voltages were *exactly* the same, just vertically shifted with respect to each other, and with an apparent superconducting transition edited onto the bottom curve.

I then had to have the painful conversation with my student. Behrang was mortified. His initial reaction was, I'm sure, the same as many others around the world working on this stuff: It can't *all* be fraudulent, right? Some of it *must* be real. Unfortunately, that doesn't seem to be the case. I would not trust anything that Schön had ever published, including the work on his doctoral thesis.

The investigative panel chaired by Mac Beasley from Stanford, a very nice person who had run a great solid-state seminar course when I had been a student out there, really got going in July. That's also around the time that I ended up with a copy of a powerpoint document put together by Dan Ralph and Paul McEuen that contained a pretty exhaustive compilation of all of the suspect figures.

The final report of the investigation committee was issued in September and still makes for mortifying reading: https://media-bell-labs-com.s3.amazonaws.com/pages/20170403_1709/misconduct-reveiw-report-lucent.pdf

The short version: Despite claims in the intro that “by all accounts Schön [was] a hard-working, productive scientist”, it sure looks like he never actually did any of this stuff. There were no

samples (they supposedly all burned up/fried themselves). There were no data files (allegedly because of hard drive failures and the claim that data was erased because of limited storage space, a claim so ludicrous that my eyes almost rolled out of my head when I read it).

There were no lab notebooks. That one really got me. In my 22 months at Bell Labs I'd filled about 8 black Lucent Technologies notebooks. No one ever asked to see my notebooks; Bob never inspected them – he didn't need to, since he saw me daily and knew I kept good records, nor did Capasso – he knew Bob would be mentoring me properly, but I kept them because that's what you do, and it absolutely makes your scientific life easier. As my students are tired of hearing me say, you will never look back on your research career and think, gee, I wish I'd kept less complete records.

What about the co-authors? Kloc had been handing over beautiful molecular crystals, the best in the world at the time, to Schön, but it is not unusual for a material supplier not to witness resulting measurements or analyze resulting data first hand. Likewise, Zhenan Bao had been providing molecules for SAMs, but had also never actually gone to Schön's lab to see the data coming in. Scientific collaborations are built on trust, and that led to vulnerabilities in this case.

As for Batlogg, truly that is a mystery to me. If my postdoc or grad student told me that I had a result like the ones Schön was claiming, you couldn't have kept me away. I'd've been in the lab dialing around the knobs myself, not out of a lack of trust, but out of pure excitement and the need for multiple sets of eyes on the experiment. I would not have tried to surprise my colleagues with new results in the literature out of the blue – I would have been running down the hallways anxious to show the data to people, to get feedback and suggestions. When it looked like he might share a big prize, Batlogg was happy to be along for the ride. In summer of 2002, when the story broke, he quickly threw Schön under the bus. The quote, as reported in the NY Times in October of 2002, was "When I am a passenger in a car and the driver drives through a red light, then I am not to blame." I think I actually yelled out loud at the screen when I read that the first time. He tried to walk that back later, but it's hard to un-read that sentiment.

Why did Schön behave this way? From the report, it's clear that he acted in a way that was far beyond what would have been rational (if amoral) cheating. I'm no psychological professional, but his actions do not seem like those of a well person.

Gossipy stories circulated about whether the glossy journals had any responsibility here. There were rumors, all denied by Science and Nature, that some of the papers had been accepted over negative referee reports. I remember asking a Science editor about this when he visited Rice late in 2002, and getting an emphatic, rather chilly denial.

So, what was the long-term fallout?

- Science did work, in the sense of arriving at the correct result, albeit not before many people around the world had sunk an enormous amount of effort into trying to reproduce

what turned out to be wholly fictional data. The whole process, from the appearance of the highest profile results to the revelation of the fraud, took about two years. While that was agonizingly slow in some ways, in absolute terms it was actually typical. That's about how long it took to disprove the mistaken claim of the discovery of n-rays, and faster than the debunking of polywater. On the other hand, the number of researchers looking at this and the internet-enabled flow of information make two years seem like eons. Essentially every major paper of Schön's has been retracted, and his doctoral degree was revoked.

- Essentially every person who at some point had nominal managerial responsibility over Schön ended up in academia in extremely prestigious positions, including Batlogg, who has now retired. Circumstances at the time within Bell Labs (high turnover, corporate upheaval, enormous trust in the oversight of the immediate senior figure Batlogg) surely contributed to the flux of papers that flew through without normal scrutiny or an opportunity for internal reviews of concerns.
- The APS put in place guidelines about proper responsibilities of coauthors. It's unclear whether these guidelines would have had any effect on this situation, but better to have something clear written down than the alternative.
- In 2003, the DOE put together a working group, chaired by Allen Goldman of Minnesota, to look at the control of electronic transport in novel field-effect structures. The unofficial purpose of this was to review the research area in the wake of the Schön scandal, to outline what real science there was to do here. One eventual result was this 2006 Reviews of Modern Physics article (<https://journals.aps.org/rmp/abstract/10.1103/RevModPhys.78.1185>), which has been cited over 330 times.
- Bell Labs still exists, now as a part of Nokia, after a sojourn with Alcatel. Physical sciences research has drastically shrunk. Industrial research labs in general still face profound headwinds, in part because of economic incentives that reward short-term growth, though perhaps the rise of quantum information applications is an opportunity.
- In my own group, we actually did some work on both organic semiconductor devices and single-molecule transistors of which I'm quite proud. While we did not find superconductivity in P3HT, we did learn a lot about charge injection and the nature of contact resistances. The timing was actually fortunate in some ways – Behrang really didn't lose much time because of Schön, since we had to figure out what we were doing with the organics anyway. Likewise, we learned some interesting things about inelastic and spin processes in single-molecule devices.
- People did succeed in making high quality crystals out of small molecule organic semiconductors, particularly of materials like tetracene, and did show that under the right circumstances it is possible to see band-like transport (mobility increasing with decreasing

temperature) over a broad temperature range. However, no quantum Hall physics, and no superconductivity in those systems.

- There actually *are* ways to achieve gated charge densities on a par with the values that Schön made up, using ionic liquids and gels as gates, though one must always be careful to distinguish between true electrostatic gating and chemical doping or unintended reactions. Applying this technique to a variety of materials has led to a lot of interesting physics, including gate-driven superconductivity in cuprates and some 2d materials.

I'm concerned that in the fast-paced world of research, this whole episode seems to have faded into comparative obscurity. Many current grad students and postdocs in condensed matter and materials science have never heard of Schön, and that seems like a missed opportunity to teach about responsible conduct, the supervision of research, and the importance of critical thinking when reading the literature. The whole saga is also a powerful reminder that science is a human endeavor, including the foibles and fallibility of the members of the community.