How Probe Microscopists Became Nanotechnologists

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Abstract. Nanoscale entities are by definition invisible to the unmediated senses. Yet generating images of these objects has been crucial to the rhetoric of nanotech boosters. Thus, bringing microscopes and microscopists under the nano umbrella has been central to the work of nano proponents. No instruments have been more crucial to this process than the scanning tunneling microscope (STM) and atomic force microscope (AFM). Yet STM and AFM have long histories that precede the advent of nano. I outline this history and show that the connection between probe microscopy and nano is contingent rather than self-evident. The drafting of the probe microscopy community into nano was inspired by role differentiation within that community following the widespread commercialization of the instruments in the early 1990s. As probe microscopists move into nano, it is likely they will remake the field in light of the history of their community.

Introduction

When we talk about nanoscience, there are a few images which often spring to mind, such as Eric Drexler's diagrams of molecular bearings (Drexler 1992), or Jim Gimzewski's fullerene abacus (Cuberes *et al.* 1996), or Don Eigler's famous "IBM" made from xenon atoms (Eigler & Schweizer 1990). Such atomic-scale images have a powerful pull, but their significance is sometimes taken as self-evident, rather than as historically and culturally situated. "Seeing" atoms is often treated, by both participants and analysts, as intrinsically meaningful and fascinating (Barad 1999, Hacking 1992, Buchwald 2000). Seeing and moving atoms is taken as an axiomatically nanotechnological activity, and images of the atom are seen to demonstrate both the reality and the potential of nanotechnology.

I want to problematize these notions by showing that images of the atom only have weight and meaning when mediated through various communities and subcultures. The interplay of different technical subcultures, and different kinds of actors within those subcultures frames how we see and understand the nanoscale. Nanotechnology is a community of communities, and the fields arrayed under its umbrella have long histories that precede their incorporation into nano. Conceptions of nano are diffracted through subcultures with different ways of generating and judging evidence, commercializing knowledge, training new members, and dealing with other communities. The social organization of these subcultures provides for quite different stakes in nano among different actors in those fields. Artifacts and techniques – "boundary objects" (Star & Griesemer 1989) – as well as mediating individuals – "boundary shifters" (Pinch & Trocco 2002, p. 313) – can travel between subcultures, quilting together different patches of the nano world. As analysts, we need to follow such travelers to understand how nano will or will not gather coherence over time. I will illustrate this point by telling a story about the history of scanning probe microscopy (SPM) – especially the scanning tunneling microscope (STM) and atomic force microscope (AFM) – and by showing how different kinds of actors have built different

(AFM) – and by showing how different kinds of actors have built different kinds of links between the nanoscience and SPM communities.

Despite the popularity of STM images of atoms, SPM research is still a small fraction of the work that counts as "nanoscience". Moreover, the atomic manipulation touted as exemplary of nanoscience is still far from the mainstream of probe microscopy. Many SPMers use the microscopes to examine "nanoscale" objects, and might willingly be called "nanoscientists" on occasion, but for most it is still not a core identity. There are a few SPMers who enthusiastically espouse nanoscience, and many who are drawn to it, but there is still much ambiguity in this community about what nanoscience is.

It is clear, though, that instrumentation is central to nanoscience. The nanoscale is a mediated world, where most objects are visible only with the aid of esoteric technologies. In forming a new discipline (or transdisciplinary constellation), nanoscience's proponents have marked its territory by giving mediated nanoscale entities an immediate "nanopresence". Little nanoabacuses, nanoguitars, nanotrains, and nanoshovels have a familiarity, a present-at-handness, even if they're only a few nanometers long; moving atoms around gives them reality and presence and even personality. This isn't new – Eric Francoeur's history of chemical models (Francoeur 1997), for instance, shows how important tangibility and familiarity can be in generating knowledge about intangibly small objects. For proponents of nanoscience, though, endowing small entities with this kind of presence has been an unusually effective rhetoric on the road to building a community.

Because nanoentities can only be seen via instrumentation, nano boosters have put enrolling instruments and instrument-builders (especially STM and AFM) at the heart of weaving nanoscience into a coherent practice (Baird & Shew 2004). This paper examines how the rhetoric and promises of nanoscience are perceived and taken up by instrumental communities. In the case of probe microscopy, social differentiation within the SPM community has made nanoscience an attractive proposition. In taking on differentiated roles with respect to building and using these instruments, many probe microscopists have seen the tools of "nanopresence", and the nascent rhetoric and institutions of nanoscience, as an opportunity to plaster over fault lines within their community. In doing so, they may nudge nanoscience closer to reality, but they will likely remake it in the image of the practices and institutions in which their work originated, rather than taking up whole cloth the visions of people like Drexler and Feynman (Drexler 1990, Feynman 1999).

1. The STM and the 7x7

Although the STM had many forebears – notably the Topografiner at the US National Bureau of Standards in the late 1960s (Villarrubia 2001, Young *et al.* 1972) – it is today taken to be the ancestor of probe microscopy. Likewise, though it was not the first instrument to "see" atoms – the field ion microscope in the 1950s (Melmed 1996) and the electron microscope in the 1970s (Isaacson *et al.* 1976) both achieved this – STM's atomic resolution is usually taken as what makes it special for nanotechnology. When it was invented at the IBM research lab in Zurich in 1982, though, it was intended as a moderate resolution industrial surface characterization tool (Binnig & Rohrer 1985, 1987). No one had any inkling of seeing atoms, nor of its relevance to nano. As the product of an IBM laboratory, STM was envisioned as relevant to only one customer – IBM. Bill Leslie and Russ Bassett have shown that IBM in the early '80s was so large, dominant, and inward-looking that it deliberately pursued narrowly-focused, idiosyncratic technological solutions to its particular problems (Knowles & Leslie 2001, Bassett 2002).

The STM was invented to solve an issue in IBM's development of a Josephson junction-based high-speed computer. These junctions required very thin, extremely homogeneous oxide films. In practice, small defects ("pinholes") in the oxide film were common and routinely ruined junction performance. So the Josephson team at the IBM Zurich lab asked their colleague, Heini Rohrer, to come up with a way to inspect the films and analyze the defects. Rohrer hired a new Ph.D., Gerd Binnig, to work on the project; together, they realized that a sharp metal tip, lowered very close to the films, could locate pinholes by probing the electrical characteristics of the film using tunneling electrons.

When they started, Binnig and Rohrer calculated that the STM's resolution would not be much better than an ellipsometer or an electron microscope. Yet the Zurich lab had the resources to develop even this very expensive instrument in the hopes that it would provide the smallest extra help to a project into which the company had poured a significant fraction of its money, time, talent, and hope. Unfortunately for IBM, the Josephson project quickly ground to a halt in a way that signaled the problems that would decimate Big Blue a decade later. Fortunately for Binnig and Rohrer and the STM, though, it lasted long enough to allow them to build prototypes and develop a feel for the technical problems involved. In that time, Binnig came to the conclusion that the STM might be able to image individual atoms. No one else really believed this, so it became impossible for a while for Binnig to work on the STM full time; but Rohrer put him onto another project that was undemanding and allowed him the resources and the freedom to tinker with the tunneling microscope on a half-time basis while hiding the STM project from upper management.

As Binnig (and two technicians, Christoph Gerber and Eddie Weibel) developed expertise with the STM, they began to look for samples to image with the new instrument. Thus, they started talking with colleagues to find out what samples would be most interesting to them. At first, they asked *crystallographers* at Zurich and IBM Yorktown (in New York state) to prepare samples with large atomic steps that could be used to calibrate the STM. Yet articles presenting data on these materials generated little or no wider interest (Scheel *et al.* 1982). So Binnig and Rohrer next began asking the *surface scientists* at Zurich and Yorktown what samples would be most interesting to their discipline.

With the exception of participants' memoirs (King 1994, Lagally 2003), very little has been written about the history and sociology of surface science. Indeed, the best history of solid-state physics (Hoddeson *et al.* 1992) contains only a short paragraph on surface studies. Yet in the late 1960s, surface science was the nanotechnology of its day: an interdisciplinary umbrella at the intersection of basic research and pressing technological issues (the space race, industrial catalysis, semiconductor manufacturing), with massive corporate and government support, particularly at institutions like Bell Labs, IBM Research, and the National Bureau of Standards. The five years between 1968 and 1973 saw the blossoming of surface science as a discipline; one prominent historically-minded surface scientist has even classified it as a "Kuhnian revolution" (Duke 1984).

This new surface science attached itself to a constellation of instruments, technologies, computing power, theoretical machinery, exemplary problems, and applications. These included: a proliferation of various surface spectroscopies; the perfection of the art of low energy electron diffraction (LEED); to facilitate both of these, the invention of various ultrahigh vacuum (UHV) technologies; in tandem with LEED, spectroscopy, and UHV, a heightened appreciation of "well-defined", ultraclean surfaces, and the elaboration of recipes for preparing them; the coordination of these recipes with LEED patterns signifying various "surface reconstructions"; and the use of theoretical and computing power in pursuing the central problematic of the field, the atomic structure of these reconstructions (that is, a better understanding of how atoms at the surface of a material rearrange themselves into a pattern different from that of the underlying bulk material).

When Binnig and Rohrer approached IBM's surface scientists in 1982, they struggled to accommodate to the language and practices of surface science to make themselves and their instrument credible. Above all, this meant choosing *important, unsolved* surface reconstructions as test materials. At first, they looked at a gold surface – the (1,1,0) 1x2 –

because gold is inert and easily prepared, and, in fact, published an article claiming they had achieved atomic resolution and had actually solved the structure of the reconstruction (Binnig *et al.* 1983). Yet because surface scientists had no ongoing debates about this reconstruction, their reaction to the STM was unmitigated disinterest.

Thus, Binnig and Rohrer turned to *the* canonical unsolved reconstruction of the day: the silicon (1,1,1) 7x7, the "Rosetta stone" or "fruit fly" of surface science. This reconstruction had been known since the late '50s, and had proven extraordinarily generative for surface science, yet had resisted all attempts at solution. Because of its easy preparation and intricate LEED pattern, it was a convenient surface on which to test students, preparation techniques, theories, and instruments such as the STM. By 1982, a couple dozen models of the reconstruction competed, with no clear leader and no obvious experimental way to decide between them. So when Binnig and Rohrer published an atomic resolution image of two unit cells of the 7x7 (Binnig *et al.* 1983), surface scientists took notice. It is difficult to overstate the importance of the 7x7. Surface scientists only paid attention to the STM – whether they believed its results or not (Hessenbruch 2001) – when Binnig and Rohrer made it relevant to their most pressing questions; and although a couple non-surface scientists (notably Cal Quate) had shown an interest in replicating the STM before the 7x7, the subsequent history shows that they (and the Zurich group) would have had great difficulty developing and selling tunneling microscopy without the participation of surface scientists.

2. Replication and Pedagogy

With the 7x7, many people wanted to replicate the STM. When STM came to North America in 1982-3, the earliest work fell into two camps (Mody forthcoming). One was situated in corporate and national research labs – primarily IBM Yorktown, IBM Almaden, and Bell Labs, but also at Ford, Philips, Lawrence Berkeley National Labs, the Naval Research Lab, and the National Institute of Standards and Technology (successor to the National Bureau of Standards). The other was located in academic groups in the Western US – primarily Cal Quate's at Stanford and Paul Hansma's at the University of California at Santa Barbara, but also groups at Caltech, Berkeley, and Arizona State.

In both styles, the STM was as much a locus for training young scientists as it was a means for producing sound knowledge (Kaiser forthcoming). In the corporate labs, the STM was insinuated into well-established methods of creating productive corporate surface scientists, as well as into an entrenched culture of competition, within as well as between laboratories. Just as IBM's control of the business computing market encouraged it to use one-of-a-kind technologies (and as Bill Leslie has noted, Bell's telephone monopoly prompted the same attitude), so the dominance of surface science shared between IBM and Bell encouraged them to look only at each other to determine what would count as good surface science (Leslie 2001). This insularity made for a rigorous and sometimes narrow definition of the field. Thus, the work of the first STMers at Bell and IBM ran in similar directions, often organized around competition to achieve milestones such as being the first to get atomic resolution on various semiconductor reconstructions.

Here, the hard work of adapting the STM for surface science was done. Binnig and Rohrer had suggested STM's surface science capabilities, but (not being surface scientists themselves) had not dwelt on making those suggestions reality. The people who did this work were postdocs and new staff scientists, people who had just arrived at the corporate labs and needed new projects. Building an instrument and making it produce credible, intelligible surface scientific data was an established way of turning a recent Ph.D. into a productive corporate researcher. Since the STM was still unproven, established surface scientists reserved their enthusiasm for the first few years; but for postdocs and new staff scientists trying desperately to survive and thrive in the corporate lab system, there were tempting potential rewards if the STM turned out to be the next big thing. So they built and rebuilt STMs, each time incorporating more and more surface science instrumentation (specimen preparation technologies, ultrahigh vacuum machinery, LEED, spectroscopies, etc.) and exploring more and more of the core problematics of surface science: reconstructions, spectroscopy, defects, crystal growth, adsorption, thin films and interfaces, etc. (Hamers *et al.* 1987, Feenstra & Stroscio 1987, Becker *et al.* 1985, Chiang & Wilson 1986).

The West Coast academic style of early STM was less tied to surface science and more in line with Binnig and Rohrer's own way of working. Initially, though – especially for the two-year period (ending in 1985) when no one in either camp could replicate atomic resolution of the 7x7 – both groups worked in parallel, and the West Coasters drew on surface science to help them get closer to their elusive goal. After replication, though, and especially after they discovered that the STM might work in air or even liquid (everything up to then had been in ultrahigh vacuum), they moved steadily away from surface science (Sonnenfeld & Hansma 1986, Elrod *et al.* 1986, West *et al.* 1986).

Ultrahigh vacuum chambers are large, cumbersome, finicky, time-consuming devices, so with air STM, it became possible to tinker with the instrument much more rapidly, to achieve a higher throughput of samples, and to look at samples too fragile for the UHV environment. In the West Coast academic labs, this allowed a reorganization of STM work and new ways of molding STM-building to the labs' pedagogical mission. Above all, graduate students worked to make the more flexible microscopes able to image a wider range of materials that would be relevant to a wider range of audiences. This drive to expand tunneling microscopy in all directions at once gave rise to STM in air, in oil, and in water, as well as new types of microscopes like the scanning ion conductance microscope and, most importantly, the atomic force microscope (which, unlike the STM, could image insulating materials as well as conductors).

Unlike the corporate labs, where researchers had a well-established surface science framework to guide their experiments, the academic labs were characterized by chaotic flux. Many instruments would be under construction at once, and when one was built, many different kinds of samples would be put into it. Especially after the advent of the AFM widened the range of imageable materials, students often resorted to "found" samples – polaroids ripped from cubicle walls, salt from the kitchen, liquid crystals broken out of watches, bone from rib-eye steak, the electrochemistry of Coke versus Pepsi, blood drawn from lab personnel, etc. In many of these cases, specimen preparation was non-existent; if, for example, students wanted to see what ice would look like in an AFM (as happened in the Quate lab), they simply stuffed a microscope into the nearest refrigerator and a couple hours later they had their answer. This bricolage (Knorr-Cetina 1981, p. 34) extended to the building of microscopes as well. Microscopes were put together rapidly (sometimes in less than 24 hours) and with all available cultural materiel. For example, the Baldeschwieler group at Caltech reported achieving atomic resolution using a pencil lead for a tip, while the Hansma group tried doing tunneling experiments with store-bought razor blades as tips, then moved on to making AFM probes out of pawn shop diamonds glued to aluminum foil cantilevers using brushes made from their own plucked eyebrow hairs.

The problem was, it was difficult to know what images meant that were produced in this way, and it was even more difficult to make them credible to anyone outside the group. Unlike the corporate labs, which could afford to be inward-looking because they had a large proportion of the surface science discipline in one place (and so credibility could be established at home rather than abroad), the academic groups had to look elsewhere for validation. So they set out to enroll a variety of disciplines in the use and promotion of probe microscopy. To this end, they constructed a local "trading zone" (Galison 1996), and instituted a division of labor within the lab to facilitate the exchange of knowledge and the generation of new designs. Students continued to build instruments, but now practitioners from various disciplines (usually postdocs and junior professors) would come into the lab for a few weeks or months, learn a bit of probe microscopy, teach the students some of their own techniques and knowledge, write a few articles (with a student), consult on the design of the next generation microscope (geared to their discipline), and then leave (often with a microscope) to set up their own STM or AFM group elsewhere.

Thus, in the late '80s and early '90s, a probe microscopy community coalesced, through, for example, the annual "STM Conferences" sponsored by the American Vacuum Society, the professional society of surface science. The Hansma and Quate groups became the centers of a dense network in this community. Postdocs, students, samples, preprints, information, and microscopes all flowed into and out of these groups. Around these foci, the probe microscopy community experienced a centripetal and centrifugal dynamic. *Centripetal* in that, as new communities of practice became interested in probe microscopy, they often approached Palo Alto or Santa Barbara for help; and, as probe microscopy became routine, there was a mass of people doing it who could exchange information, referee each other's articles, organize conferences with each other, exchange students and samples, etc. Practitioners developed thick ties to each other, often through key intermediaries such as Quate and Hansma. The *centrifugal* dynamic, though, was simply the flip side of this; as more and more disciplines became interested in probe microscopy, the instruments started to be used in an astonishing variety of ways. As new microscopes were developed for new techniques, "probe microscopy" flowered into a thicket of 30 or 40 different kinds of instruments, and hundreds of different operating modes. Researchers began to specialize in one or two of these modes, leading to a fragmented Babel in the SPM community. Also, as many SPMers came to rely on commercial, black-boxed microscopes, there was less reason for a separate "STM Conference" focused on innovations to the technique, and many users dispersed back to the professional conferences of their home disciplines – the American Physical Society, the Materials Research Society, etc.

3. The Gold Rush

Both the centripetal and centrifugal dynamics found even stronger expression as the West Coast groups spawned startup companies to manufacture commercial versions of the STM and AFM. At Stanford, former Quate students and postdocs founded Park Scientific Instruments in 1989; while at Santa Barbara, Virgil Elings, a physics professor with experience commercializing instruments, started Digital Instruments (or DI as it is usually called) in 1987; smaller companies also emerged at Berkeley, Caltech, and Arizona State. Several aspects of lab group culture encouraged commercialization: the need and desire to enroll new kinds of users (who now became *buyers*); the quick production of surplus images, instruments, and microscopists; and an outward orientation that encouraged collaboration with many different disciplines. Ironically, the inward-looking corporate labs commercialized virtually nothing. Belatedly, and largely unsuccessfully, IBM did a small-scale commercialization of the SXM, an industrial instrument developed for in-house use – and, significantly, its inventor was not a surface scientist but a former Quate postdoc.

Initially, start-ups and academic labs lived symbiotically and mostly harmoniously. At DI and Park, company culture was an extension of lab culture. Many students left the lab groups to work for the start-ups, and once in the company, they took up projects that resembled what they had worked on as graduate students. Moreover, the companies mirrored the academic groups' division of labor by setting up in-house applications labs to pioneer the technique in new areas. Often, former Quate and Hansma postdocs cycled through these applications labs after their stint with the academic groups.

One concern for the start-ups in the early 1990s was to find new "7x7s". That is, these companies were looking for new samples to which the microscopes could bring experimental realism ("nanopresence") and thus demonstrate the technique to new disciplines and markets. When STM moved into air, half this problem was solved by showing it could resolve atoms of graphite. Highly-oriented pyrolitic graphite (HOPG) had many advantages for STMers: it was a well-known substrate used in several disciplines; techniques were available for putting down different objects, especially biomolecules, onto an HOPG surface; and the HOPG itself was easily bought from lab supply companies.

There were two (related) problems with graphite, however. First, there was no discipline in which seeing the atoms of graphite was epistemically significant in the way seeing the 7x7 had been; there was no ongoing debate in which STM images of graphite made any difference. Besides, images of graphite were (unlike the 7x7) visually banal – just a close-packed surface with no defects. This lack of defects itself became a point of contention. Defects and contamination are a key part of the reality effect of STM images (Mody 2001). Thus, the consistent absence of defects in images of graphite indicated that "atomic resolution" of graphite was more artifact than reality (Mizes *et al.* 1987, Pethica 1986). Also, STM images did disagree with what was known about HOPG – they showed an extremely high atomic corrugation – but in the absence of any *disciplinary* debate about graphite the STMers were left to construct one on their own. This meant that, for those who cared about the anomaly, it was taken exclusively to indicate a quirk of the instrument rather than a new phenomenon. For the *surface science* STMers, in particular, this issue was taken to indicate the fallibility and non-rigor of doing STM in air.

Nevertheless, putting molecules on graphite and looking at them with an STM proved extraordinarily attractive in the early '90s. Hundreds of researchers joined the STM community just to do this, many by buying the new commercial instruments. A kind of "experimental vertigo" set in – it was too tempting not to look at biomolecules with an atomic resolution instrument; yet it was disconcerting to use a microscope and a substrate with so many unresolved ambiguities. These were the gold rush days of probe microscopy, as newcomers flooded in and articles and images flooded out, and the 24-karat experiment was atomic resolution of DNA. DNA was a well-studied molecule, but the possibility of seeing the helix, and perhaps even sequencing genetic material with an STM, had a magical grip on the probe microscopy community. The high stakes inherent in making the STM a routine tool of biophysics and genomics were a draw that rapidly expanded the field.

The boom and bust of air STM (particularly the drive to image DNA) says much about how the probe microscopy community was organized. The Quate and Hansma groups valued the quick production of microscopes, images, and articles. Newcomers had to compete with this flow, while also learning to use the instruments. The result was a flurry of experimental activity of uneven quality. Former members of the Quate and Hansma groups are the first to admit that some of what they published in this period was questionable; but error was basic to their lab culture. Hansma's great proverbs, for instance, were: "do everything as poorly as you can" and "make as many mistakes as you can as fast as you can". Sometimes this produced smashing successes. Sometimes – particularly when other groups tried to mimic this style – it could bring glaring failures.

Thus, there was tremendous excitement, but also intense skepticism, when a few groups published atomic resolution images of DNA (Driscoll *et al.* 1990, Beebe *et al.* 1989). This ambiguous reaction was partly due to difficulty in satisfying the contradictory demands of the various fields with an interest in STM – on the one hand, surface scientists found biological systems "dirty" and ill-defined, and, on the other hand, biologists found the whole language of electron tunneling obscure and unhelpful in interpreting these strange images. Skepticism was also a product, though, of tension between the pioneers of air STM and newcomers to the field. Binnig and other old-timers pointed out that you could see "DNA" even on graphite surfaces that were ostensibly free of DNA molecules (Lindsay 1990, Heckl & Binnig 1992). Most people with experience in the area knew that graphite defects could easily mimic DNA in an STM image – the significant problem was not seeing "DNA", but rather sorting DNA from defects. Crucially, this required much more meticulous experiments and much tighter ties to biology and biophysics than the instrument-building groups were willing to take on at the time.

Thus, air STM evaporated almost overnight (at least in Europe and North America). Groups like Quate's and Hansma's were content to try a technique out on the samples particular to various disciplines, and to close up shop if doing so proved too contentious. Their interest was in enrolling various communities, not in debating them. Thus, it was still *possible* to do air STM, even on DNA, and a few marginal groups continued working on the problem with eventual success (Guckenberger *et al.* 1994); but, unlike the 7x7, with DNA there was no community with the resources and the desire to make air STM a fluent biophysical tool. Rather, with so many different kinds of probe microscopes available, it made little sense to dwell on a technique that so many communities found problematic. The outcome of the DNA crisis was a reorganization of the probe microscopy community that eased many of the frictions brought on by the initial differences between the California academic groups and the corporate surface scientists, as well as by the influx of newcomers attracted by air STM on graphite. Henceforth, the dichotomy between STMers and AFMers became much more pronounced. STM was consigned almost exclusively to surface scientists and electrochemists, and commercialization of the microscopes proceeded more slowly and cautiously in those fields. The rest of the probe microscopy community migrated quickly to AFM, and the vast majority AFMers began buying, rather than building, their microscopes. Thus, the task of welcoming newcomers to the field, ensuring their practices fell in line with community standards, and further innovating the technique fell to the microscope manufacturers and to a very small, elite residue of groups (such as Quate's and Hansma's) that continued to build all or part of their instruments.

4. Problems of Role

The DNA and 7x7 stories provide a glimpse of the social organization of probe microscopy that can help us understand the field's relationship to nanotechnology. Who, we should first ask, are the actors in these stories? The Hansma group, for one, divided itself into "builders" and "runners". "Builders" included group leaders like Quate, Hansma, and Binnig, as well as their students and technicians. Builders formed the core of the group, to which runners were added later; when they graduated, builders joined (or founded) the start-ups, or went to places where they could continue developing instruments (*e.g.* IBM Almaden or manufacturers like KLA-Tencor). A few took academic jobs in physics or engineering departments where they founded their own "builder" groups. *Runners* were the postdocs or junior professors who passed through the builder groups and helped integrate the microscopes with the practice of various disciplines – geology, surface science, biology, biophysics, electrochemistry, etc. After collaborating with the builders, runners usually went to academic jobs in their respective disciplinary homes – chemistry or physics or engineering departments, medical schools, etc. Others joined the start-ups as applications scientists, continuing the work of incorporating the microscopes into new fields.

At the corporate labs, builders and runners were less distinct categories. Building an instrument was a test of a young scientist's abilities, but it was skill as a runner – generating credible, intelligible, disciplined surface science knowledge from the microscope – that made a corporate researcher's career. Still, building instruments was key to these people's identities and most of them have continued to build their own instruments through their careers or have only recently transitioned to commercial microscopes.

As commercial instruments became widely available, a new category emerged – "users". These people never worked directly with builders, but rather learned probe microscopy by interacting with manufacturers' representatives. Their ties to STM and AFM were generally loose – some bought the instruments with leftover research funds, or just to see what they could do, or chipped in to share with other groups or even whole departments. Learning to use a probe microscope is relatively easy, so such users risked little by assigning one technician or student to add the technique to their portfolio and provide SPM services to the whole lab. Relatively few of these users would go to an STM Conference, or privilege their AFM or STM over any of the other instruments they relied on. Rather, probe microscopes became routine, taken-for-granted tools – maybe less routine than a centrifuge or a light microscope, but not much so.

There were, in addition, "exceptional users" with special ties to the manufacturers and a special place in the SPM community. Many were early adopters of the commercial instruments, and, like the runners, they were often the first to push the microscopes into a new community. Indeed, in the earliest days, one way to get bumped up long waiting lists for a microscope was to name the manufacturer's employees as coauthors on papers generated with their AFM – *i.e.*, to form a typical exceptional user relationship. Exceptional users are the type of people who write applications notes (which the manufacturers distribute widely), supply manufacturers with interesting samples (which manufacturers use in their advertising), beta test commercial instruments, develop add-ons and modules for the commercial models, act as references for potential customers, and train graduate students who then join the companies' applications labs. In return, exceptional users get cheap instruments, free publicity, jobs for students, research funding, and a privileged position within the scanning probe community.

One privilege over which exceptional users and manufacturers bargain is knowledge of the inner workings of the commercial instruments. DI (the largest SPM company) built its reputation on tightly black-boxed instruments. Users were told little about the electronics, and serial numbers were even filed off of controller chips. Being an exceptional user for DI, however, could mean access to the secrets of the Nanoscope controller. It could also mean a right to tinker with the controller and still have a guarantee of customer support (where, for unexceptional users, tinkering with the controller meant writing off the warranty). Other companies like Topometrix and RHK created a market niche selling instruments with a more open, flexible architecture somewhat more conducive to tinkering. For these companies, the line between ordinary users and exceptional users is more blurred, and the constant chatter between company and user created by an open architecture often turns up innovations to fold into the next generation microscope.

There are difficulties, though, in *maintaining* the roles of builder, runner, and exceptional user. The existence of microscope manufacturers, and of a large user base for them to supply, destabilizes the positions of these people. For instance, people who formerly built their own instruments now have to deal with the existence of (relatively cheap) commercial instruments that (in most cases) can do everything a home-built instrument can. Some have chosen to go over to the commercial products, but this means ceding hard-won expertise and an investment of technical identity (Haring 2002) as an instrument-builder. Some have used their special knowledge and long-standing acquaintance with the manufacturers to become "exceptional users". Others continue to build instruments, but now have to produce more elaborate justifications for doing so.

For groups like Quate or Hansma's, the justification is that they are engineering the next generation of probe microscopes (and derivative technologies). There is such disincentive to build one's own microscope at this point, though, that unless a new advance is seen as orders of magnitude better than what the manufacturers are selling (or as easy to adapt from a commercial instrument), few research groups follow up. So the audience then becomes the manufacturers themselves, and symbiotic relationships between builder groups and companies like DI spring up to extend and commercialize builders' innovations. If DI doesn't incorporate an innovation, though, then the builder group may decide to spin off its own company to sell its idea (or, alternatively, if DI is interested then the group may still spin off a company to handle the commercial relationship with the larger manufacturer). So groups that have traditionally built their own microscopes and still want to do so are one source of the dozens of little startups that are populating the probe microscopy community.

Alternatively, builder groups may depict the commercial instruments as inadequate in some way. They describe a "mass" marketed instrument as necessarily built for the needs and skills of what the manufacturers perceive to be the average microscope. Certain modes of operation may be elided from such instruments, and for some research groups those modes may then become experimental niches. Such groups often say they are doing high-end, sophisticated science – and careers can be made quickly with such machines. The design, construction, and care of such microscopes take up much of the time of such groups. Thus, the language of craftsmanship and artistry surrounds their work – both in terms of the instruments and the images they produce. For instance, where users of commercial instruments usually rely on the default color settings and image rendering algorithms, groups that build their own instruments also invest time and energy in making their images highly rendered and aesthetically pleasing. As artisans, some builders have positioned themselves as a craft elite within the probe microscopy community. Through spectacular images (such as Eigler's atomic corrals), they generate much of the publicity the community receives; and they take on leadership roles in the planning of STM Conferences, in editing volumes and journals related to the field, and in founding companies to market to small niches within the SPM community.

For "runners" and "exceptional users", the existence of relatively cheap, high-quality, widely available commercial instruments presents other difficulties. Bringing probe microscopy into a discipline can make a career, but it can rarely sustain it. Once manufacturers figure out how to sell and market to that discipline, and a wide cross-section of the field follows a runner's lead and figures out how to use the instruments, then being the first is no longer as helpful in getting new grants accepted or articles published. Such people walk a difficult line, as illustrated vividly for them by the history of electron and light microscopy. On one side lies the danger of being tied to the technology, of producing nothing new of one's own by, for example, running a microscopy core to which other groups bring samples or send students – as has happened with light and electron microscopy. On the other side is the danger of distancing oneself from the technology and losing an important tool in the struggle to compete and survive within one's subdiscipline – runners made their reputations with the STM or AFM, and are loathe to loosen their ties *either* to the instrument or to their home community. Runners and exceptional users face the choice of developing the technology, often by *constructing* modifications and taking on the role of a builder, or focusing more on the samples specific to their subdiscipline and treating the STM or AFM as one tool among many.

Interestingly, the solution to this problem frames differences between runners and exceptional users (who, in other ways, are quite similar), particularly in terms of how they view nanotechnology. The old runners who worked with Quate and Hansma in the early '90s tend to be more cautious about nano than exceptional users. Runners expended such effort translating probe microscopy into the terms of their home communities that they don't have any desire to further disturb the structure of those disciplines. Runners are most skeptical of nano's rhetoric of a revolutionary interdisciplinarity that will dissolve the traditional disciplines. Some runners, and some builders, yearn for the early '90s as a charismatic golden age – as evidenced, for example, by a revolt at DI in 2000, when former runners and early DI employees formed a startup, *Asylum* Research (pun intended) to recapture the free-form instrument-building culture of the early '90s. "Exceptional users" though, are more opportunistic about nano – they've tied themselves strongly to the manufacturers who have cultivated the growth of the nano community. Thus, exceptional users tend to be some of the most enthusiastic nanoists in the probe microscopy field and, more than anyone, have done the work of figuring out how to implement the manufacturers' products in ways that would count as nano – that is, they aren't building new nano instruments themselves, but they've been most successful at taking products that are already there and using them in canonically "nano" ways.

5. Nanoscience and Probe Microscopy

For many ordinary users, meanwhile, AFM has become an easy entrée to the nano. I have done ethnographic work, for instance, with materials scientists at Cornell who are using very old techniques to produce small structures that they can now call "nanohills" or "nanoropes". Before the AFM, these structures would have had very little "nanopresence" – it would have been very difficult to visualize them and make plausible arguments about their usefulness. By buying an off-the-shelf AFM, training a student to use it, and producing very ordinary nanoscale images, though, these groups are able to connect themselves to a community of people who work on other entities which have a similar nanopresence (which, at Cornell, is quite a large community). The AFM is hardly central to their work – their knowledge of it is fairly circumscribed, only one or two of their students may be adept with it, most of their group's time is spent preparing samples that will very quickly be put through the AFM *as well as* other instruments, and they have little intention of ever tinkering with the instrument. It does, however, help them lay claim to the money, attention, facilities, and community that are growing up around nanoscience.

For those with deeper roots in probe microscopy, responses to nanoscience are shaped by the centrifugal and centripetal dynamics of routinization and commercialization, and the role instabilities they create. As a community, probe microscopy is disintegrating in places. SPM no longer has the instant appeal it did in the gold rush of the early '90s, and most users see it as a benchtop tool rather than as their primary locus for innovation and research. At the same time, many builders and runners have made STM and AFM the center of their work for so long that they feel detached from their original disciplinary homes. Thus, many of these people have rallied to "nano" as a way to shore up some of their role instabilities and to make the existence of a probe microscopy *community* more palpable. For those who have positioned themselves as a craft elite, "nano" provides the framework for a community to be the elite *of*. For those who have positioned themselves as mediators between users and the manufacturers, "nano" can be both carrot and stick in their relations with the manufacturers. The carrot is the lavish, coordinated funding the government is pumping into nanoscience. The stick is that this funding helps coalesce a nanoscience community that still puts a premium on *making* epistemically interesting artifacts, many of which derive from probe microscopy techniques (*e.g.* molecule pullers, artificial noses, millipede storage devices, nanotube transistors, nanomanipulators, etc.). Builders and runners can direct their work to the nano community as much as to the manufacturers; and if the nano community likes what they hear, then these groups have an added leverage in their relationships with the manufacturers.

For SPM manufacturers, too, nano has many attractions. For DI, in particular, nano is a tool to regulate the centripetal and centrifugal dynamics of an instrument-oriented community. By virtue of its size and influence and tremendous number of users, DI sits at or near the center around which probe microscopy revolves. Nano means funding and publicity and interest that DI sees as pulling more people into that orbit. Perhaps more importantly, nano provides a unifying rhetoric that keeps people who are already using AFMs from drifting back into their own disciplines. As long as DI can convince these people that they are *nanotechnologists* (rather than just physicists or chemists or biologists) then it can hold their attention for an ever-expanding line of nanotechnology instrumentation. Thus, it's interesting to notice that in the past few years, DI's new products have been much more oriented to sensing and force-pulling and nanomanipulation (*i.e.*, a whole family of nano-oriented products) rather than just microscopy.

For the smaller manufacturers, nano holds the promise of a ready-made community with niches and wrinkles where DI doesn't compete and in which they can survive. These manufacturers, for instance, have helped maintain the annual STM Conferences while steering them toward nano. In part, these meetings represent the densest concentration of probe microscope users and innovators – the people from whom manufacturers draw their patents, people, and ideas. Yet manufacturers realize the tensions inherent in prolonging the existence of a dedicated STM Conference when the number of builder groups is dwindling, so SPM manufacturers have been key in transforming these meetings into "NANO Conferences". They see nano as transcending the connection to any particular instrument, and therefore as providing a more sustainable basis for the ongoing existence of an expanding community of practitioners who will be interested in their wares, and who will still have enough builder groups to yield up commercializeable innovations.

So the incorporation of instrumental communities such as the STM/AFM field is one mechanism for the growth of nanotechnology. For several reasons this is a highly attractive method for nano leaders. First, one nagging problem of nano at this early stage is that there is little that connects together all of its disparate parts. By identifying instruments such as the AFM that are common to different patches of the nano quilt, nano leaders, particularly at the National Science Foundation and NSF-sponsored university nanocenters, can encourage coordinated work across the nano community by funding the purchase of microscopes that are shared across interdisciplinary groups. Since probe microscopy manufacturers and elite builder groups have a long tradition of encouraging the passing of these instruments across disciplinary boundaries, nano can incorporate tried and true pathways for tying together disparate subcultures through common instrumentation.

This process, in turn, encourages those probe microscopy elites and manufacturers to seek out the nano community. "Nano" provides a way out of the role dilemmas arising from commercialization of the microscopes. That is, by encouraging ordinary users of commercial microscopes to build networks through shared instrumentation, nano offers STM and AFM manufacturers a much larger market than they have seen before. At the same time, nanotechnology attracts probe microscopists because its canonical activity centers on making knowledge-generating *things* – whether macroscale artifacts like microscopes and diffractometers, or nanoscale artifacts like nanotubes and quantum dots. Closure has not been reached on what counts as nanotechnology (whereas closure has, more or less, been reached on what counts as a good STM or AFM), so *builders* have much more room within nanotechnology to continue building, tinkering, and modifying a wide range of instruments. In the meantime, those who routinely use commercial instruments can concentrate on creating novel nanoscale artifacts and characterizing them with their store-bought STM or AFM. Indeed, this process leads to new rounds of collaboration – as has happened so often in the history of STM and AFM, *builders* are looking to work with people who know how to make novel nanoscale objects, and instrument *users* are looking to work with people who can build instruments that will make novel measurements.

There are also other mechanisms for the growth of nanotechnology. In particular, nano leaders have seeded their discipline by appropriating whole subdisciplines. STM and AFM have played an important role in this process, especially in the conversion of surface science discourse into nano discourse. Surface science has changed dramatically since the introduction of STM and AFM. To some extent, the new microscopes reoriented the important problematics of the field – in the late '70s, for instance, you could get a Ph.D. for proposing a new model for an unsolved reconstruction, whereas in the '90s that would only be one chapter of a surface science dissertation. More important to the cohesiveness of the discipline, though, was the massive scaling back of corporate research in the early '90s. Though they were by no means the entirety of the field, Bell Labs and IBM were the centers around which the field revolved. With their decline, the set of activities that count as good surface science has become broader and more diffuse.

Indeed, surface science's institutional ties to probe microscopy were one mechanism for this blurring of the field's focus. When the STM first made inroads into surface science, it attracted the attention of the Naval Research Lab (an eminent center of surface science), the Office of Naval Research (the primary funder of non-corporate surface science), and the American Vacuum Society (the primary institutional home of surface science and the sponsor of the field's major journal, the *Journal of Vacuum Science and Technology*). Two people at the top of these organizations, Jim Murday and Rich Colton, made STM the baby of both the AVS and ONR. Thus, the ONR became a major funder of people like Quate and Hansma, and the AVS became the major sponsor of the annual STM Conferences, with most of the proceedings of these conferences appearing in JVST. As we have seen, though, the probe microscopy community included a lot of people who were no surface scientists; and especially after the advent of the AFM, the STM Conferences became filled with people with no interest in surface science or its traditional mainstays (vacuum technology, spectroscopic and diffraction techniques, metals and semiconductors, well-defined surfaces). Thus, the *Journal of Vacuum Science and Technology* found itself publishing vast numbers of articles on work done in air and liquid environments.

For Murday, "nano" is a way to maintain the cohesiveness of both the surface science and probe microscopy communities. It was Murday and Colton, for instance, who hosted the 1990 STM Conference and changed its name to the STM/NANO Conference (and effected the switch from then on to alternating STM and NANO meetings). 1990 is very early in the takeoff of nano, a period when the word was still fairly disreputable, so it was a big step for a well-known scientist and grant officer to put such a stake in it. Today, this stake continues – Murday is now one of the key players in the National Nanoinitiative, and one of his pet projects is to transform the AVS into a Nanoscale Science and Technology Society. Now, in some sense this part of the story is the work of a very few individuals – Murday and Colton showed extraordinary vision in making STM their protégé early on, and they took a remarkable gamble in foreseeing how important "nano" rhetoric would become. At the same time, "nano" was in some ways a handy solution to a problem of their own making – by tying the AVS so strongly to an instrument that very quickly exploded out of surface science, they helped lay the grounds for the AVS' existential crisis. Given that they wanted to preserve both the nascent probe microscopy community and the well-established AVS, "nano" was an easy choice of a discursive means to do so.

So what are the lessons of the STM and AFM story for nanoscience? Well, from this last piece, we can see first of all the importance of institutions and subdisciplines that predate nano – nanoscience has had to wait for its opening, an opening afforded by changes in the status of (among others) the corporate research labs, the discipline of surface science, and the relationship between industry and the academy. We have also seen the importance of commercialization, and the tremendous problems and opportunities that commercialization presents for a wide range of researchers. In this case, nano has been an extremely flexible rallying cry for all of the parties to the commercialization process; for some, it may provide a way to smooth some of the fault lines created by commercialization. Finally, we've seen the extent to which "nano" is a moveable feast, a rhetoric that can be massaged and transformed to fit the needs of various parties in a community. These people have many orientations to nano, and many different emotions about it; and those emotions surround nano's role in defining their future practice and community. Whatever that role, it is clear that the nano they are trying to create is their own nano, deeply rooted in the traditions (of, *e.g.*, instrument-building or surface science) in which they have trained and worked. Many of these people are quite cynical, if not dismissive, of a grand nano rhetoric á la Drexler or Roco; but at the same time they project ways in which nano is their future, once it has been properly specified relative to their local practices. This transformation of a grand discourse into local practice is one of the most interesting parts of the nano phenomenon, and one that bears much further analysis.

References

Baird, D. & Shew, A.: 2004, 'Probing The History Of Scanning Tunneling Microscopy', in: D. Baird, A. Nordmann & J. Schummer (eds.), *Discovering the Nanoscale*, Amsterdam: IOS Press, pp. 145-156 (this volume).

Barad, K.: 1999, 'Agential Realism: Feminist Interventions In Understanding Scientific Practice' in: M. Biagioli (ed.), *The Science Studies Reader*, New York: Routledge, pp. 1-11.

Bassett, R.K.: 2002, *To The Digital Age: Research Labs, Start-Up Companies, And The Rise Of MOS Technology*, Baltimore: Johns Hopkins.

Becker, R.S.; Golovchenko, J.A.; McRae, *E.G.* & Swartzentruber, B.S.: 1985, 'Tunneling Images of Atomic Steps On the Si(111)7x7 Surface', *Physical Review Letters*, **55**, 2028-2031.

Beebe, T.P.; Wilson, T.E.; Ogletree, D.F.; Katz, J.E.; Balhorn, R.; Salmeron, M.B. & Siekhaus, W.J.: 1989, 'Direct Observation of Native DNA Structures With the Scanning Tunneling Microscope', *Science*, **243**, 370-372.

Binnig, G. & Rohrer, H.: 1985, 'The Scanning Tunneling Microscope', *Scientific American*, **253**, 50-6.

Binnig, G. & Rohrer, H.: 1987, 'Scanning Tunneling Microscopy – From Birth to Adolescence', *Reviews of Modern Physics*, **59**, 615-625.

Binnig, G.; Rohrer, H.; Gerber, C. & Weibel, E.: 1983, '(111) Facets As the Origin of Reconstructed Au(110) Surfaces', *Surface Science*, **131**, L379-L384.

Binnig, G.; Rohrer, H.; Gerber, C. & Weibel, E.: 1983, '7x7 Reconstruction On Si(111) Resolved in Real Space', *Physical Review Letters*, **50**, 120-123.

Buchwald, J.Z.: 2000, 'How the Ether Spawned the Microworld' in: L. Daston (ed.), *Biographies of Scientific Objects*, Chicago: University of Chicago Press, pp. 203-225.

Chiang, S. & Wilson, R.J.: 1986, 'Construction of a UHV Scanning Tunneling Microscope', *IBM Journal of Research and Development*, **30**, 515-519.

Cuberes, M.T.; Schlittler, R.R. & Gimzewski, J.K.: 1996, 'Room-Temperature Repositioning Of Individual C_{60} Molecules At Cu Steps: Operation Of A Molecular Counting Device', *Applied Physics Letters*, **69**, 3016-3018.

Drexler, K.E.: 1990, *Engines of Creation: The Coming Era of Nanotechnology*, New York: Anchor Books.

Drexler, K.E.: 1992, *Nanosystems: Molecular Machinery, Manufacturing, and Computation*, New York: Wiley.

Driscoll, R.J.; Youngquist, M.G. & Baldeschwieler, J.D.: 1990, 'Atomic-Scale Imaging of DNA Using Scanning Tunneling Microscopy', *Nature*, **346**, 294-296.

Duke, C.B.: 1984, 'Atoms And Electrons At Surfaces: A Modern Scientific Revolution', *Journal of Vacuum Science and Technology A*, **2**, 139-143.

Eigler, D.M. & Schweizer, E.K.: 1990, 'Positioning Single Atoms With a Scanning Tunneling Microscope', *Nature*, **344**, 524-526.

Elrod, S.A.; Bryant, A.; Delozanne, A.L.; Park, S.; Smith, D. & Quate, C.F.: 1986, 'Tunneling Microscopy From 300-K to 4.2-K', *IBM Journal of Research and Development*, **30**, 387-395.

Feenstra, R.M. & Stroscio, J.A.: 1987, 'Reconstruction of Steps On the Si(111)2x1 Surface', *Physical Review Letters*, **59**, 2173-2176.

Feynman: 1999, 'There's Plenty of Room at the Bottom: An Invitation to Enter a New Field of Physics' in: (ed.), *The Pleasure of Finding Things Out*, Cambridge, MA: Perseus, pp. 117-139.

Francoeur, E.: 1997, 'The Forgotten Tool: The Design And Use Of Molecular Models', *Social Studies of Science*, **27**, 7-40.

Galison, P.: 1996, 'Computer Simulations and the Trading Zone' in: P. Galison & D. J. Stump (ed.), *The Disunity of Science: Boundaries, Contexts, and Power*, Stanford: Stanford University Press, pp. 118-157.

Guckenberger, R.; Heim, M.; Cevc, G.; Knapp, H.F.; Wiegrabe, W. & Hillebrand, A.: 1994, 'Scanning-Tunneling-Microscopy of Insulators and Biological Specimens Based On Lateral Conductivity of Ultrathin Water Films', *Science*, **266**, 1538-1540.

Hacking, I.: 1992, 'The Self-Vindication of the Laboratory Sciences' in: A. Pickering (ed.), *Science as Practice and Culture*, Chicago: University of Chicago Press, pp. 29-64.

Hamers, R.J.; Tromp, R.M. & Demuth, J.E.: 1987, 'Electronic and Geometric Structure of Si(111)-(7x7) and Si(001) Surfaces', *Surface Science*, **181**, 346-355.

Haring, K.: 2002, *Technical Identity in the Age of Electronics*, Cambridge, MA: Harvard University doctoral dissertation.

Heckl, W.M. & Binnig, G.: 1992, 'Domain-Walls On Graphite Mimic DNA, *Ultramicroscopy*, **42**, 1073-1078.

Hessenbruch, A.: 2001, 'History of Recent Science and Technology: Materials Research', Cambridge, MA: Dibner Institute [http://hrst.mit.edu/hrs/materials/public/STM_intro.htm].

Hoddeson, L.; Braun, E.; Teichmann, J. & Weart, S.: 1992, *Out of the Crystal Maze: Chapters from the History of Solid-State Physics*, Oxford: Oxford Univ. Press.

Isaacson, M.S.; Langmore, J.; Parker, N.W.; Kopf, D. & Utlaut, M.: 1976, 'The Study Of The Adsorption And Diffusion Of Heavy Atoms On Light Element Substrates By Means Of The Atomic Resolution STEM', *Ultramicroscopy*, **1**, 359-376.

Kaiser, D.: forthcoming, 'Making Tools Travel: Pedagogy And The Transfer Of Skills In Postwar Theoretical Physics' in: D. Kaiser (ed.), *Pedagogy and the Practice of Science: Producing Physical Scientists, 1800-2000*, Cambridge, MA: MIT Press.

King, D.: 1994, 'Chemisorption On Metals: A Personal Review', *Surface Science*, **299-300**, 678-689.

Knorr-Cetina, K.: 1981, *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science*, Oxford: Pergamon Press.

Knowles, S.G. & Leslie, S.W.: 2001, ''Industrial Versailles' – Eero Saarinen's Corporate Campuses For GM, IBM, and AT&T', *Isis*, **92**, 1-33.

Lagally, M.: 2003, 'Transition From Reciprocal-Space To Real-Space Surface Science – Advent Of The Scanning Tunneling Microscope', *Journal of Vacuum Science and Technology A*, **21**, S54-63.

Leslie, S.W.: 2001, 'Blue Collar Science: Bringing The Transistor To Life In The Lehigh Valley', *Historical Studies in the Physical and Biological Sciences*, **32**, 71-113.

Lindsay, S.M.: 1990, 'Can the Scanning Tunneling Microscope Sequence DNA?', *Abstracts of Papers of the American Chemical Society*, **200**, 96-ANYL.

Melmed, A.: 1996, 'Recollections Of Erwin Mueller's Laboratory: The Development Of FIM (1951-1956)', *Applied Surface Science*, **94/95**, 17-25.

Mizes, H.A.; Park, S. & Harrison, W.A.: 1987, 'Multiple-Tip Interpretation of Anomalous Scanning-Tunneling-Microscopy Images of Layered Materials', *Physical Review B-Condensed Matter*, **36**, 4491-4494.

Mody, C.: 2001, 'A Little Dirt Never Hurt Anyone: Knowledge-Making and Contamination in Materials Science', *Social Studies of Science*, **31**, 7-36.

Mody, C.: forthcoming, 'Instruments in Training: The Growth of American Probe Microscopy in the 1980s' in: D. Kaiser (ed.), *Pedagogy and the Practice of Science: Producing Physical Scientists, 1800-2000*, Cambridge, MA: MIT Press.

Pethica, J.B.: 1986, 'Interatomic Forces in Scanning Tunneling Microscopy – Giant Corrugations of the Graphite Surface – Comment', *Physical Review Letters*, **57**, 3235-3235.

Pinch, T. & Trocco, F.: 2002, *Analog Days: The Invention and Impact of the Moog Synthesizer*, Cambridge, MA: Harvard University Press.

Scheel, H.J.; Binnig, G. & Rohrer, H.: 1982, 'Atomically Flat Lpe-Grown Facets Seen By Scanning Tunneling Microscopy', *Journal of Crystal Growth*, **60**, 199-202.

Sonnenfeld, R. & Hansma, P.K.: 1986, 'Atomic-Resolution Microscopy in Water', *Science*, **232**, 211-213.

Star, S.L. & Griesemer, J.R.: 1989, 'Institutional Ecology, 'Translations' And Boundary Objects: Amateurs And Professionals In Berkeley's Museum Of Vertebrate Zoology, 1907-1939', *Social Studies of Science*, **19**, 387-420.

Villarrubia, J.: 2001, 'The Topografiner: An Instrument for Measuring Surface Microtopography' in: D. Lide and D. Stahl (ed.), *A Century of Excellence in Measurements, Standards, and Technology – Selected Publications of NBS/NIST, 1901-2000*, Boca Raton: CRC Press, pp. 214-8.

West, P.; Kramar, J.; Baxter, D.V.; Cave, R.J. & Baldeschwieler, J.D.: 1986, 'Chemical Applications of Scanning Tunneling Microscopy', *IBM Journal of Research and Development*, **30**, 484-491.

Young, R.; Ward, J. & Scire, F.: 1972, 'Topografiner – Instrument For Measuring Surface Microtopography', *Review of Scientific Instruments*, **43**, 999-1011.