Crafting the Tools of Knowledge: The Invention, Spread, and Commercialization of Probe Microscopy, 1960-2000

CRAFTING THE TOOLS OF KNOWLEDGE: THE INVENTION, SPREAD, AND COMMERCIALIZATION OF PROBE MICROSCOPY, 1960-2000

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This dissertation is an historical and ethnographic examination of the invention, replication, and routinization of scanning probe microscopy, a family of ultrahigh resolution surface characterization techniques found today in surface science, materials science, electrochemistry, biophysics, and nanotechnology, as well as in industrial reliability and quality control laboratories, in semiconductor manufacturing, in high school science fair projects, and even on the surface of Mars. The dissertation begins with the less-than-successful story of the Topografiner, a precursor of the scanning tunneling microscope (STM) at the US National Bureau of Standards at the end of the ’60s that failed to prove the concept of probe microscopy or win managerial approval. The STM itself originated at the IBM Research lab in Zurich. Its inventors committed themselves to a naïve experimental practice that allowed them to push past certain obstacles and to forge much-needed collaborations, particularly with surface scientists. Surface scientists were the first to bring the STM to the big North American corporate laboratories at IBM and Bell Labs. There, tunneling microscopy became a locus for training young researchers as well as for generating new surface scientific knowledge. Simultaneously, the STM was adopted by a handful of academic groups in California, who cultivated a more freewheeling way of integrating pedagogy and microscope-building. By 1990, building an STM had become easy enough that many people joined the community; this influx provoked a number of internal frictions, finally erupting in a controversy about whether the STM could atomically resolve DNA. This debate was resolved partly through the intervention of microscope manufacturers associated with the California

academic groups. As these manufacturers grew, they faced the problem of keeping themselves distinct from, yet close to, the experimental cultures of their customers. The probe microscopy community changed radically after it became possible to buy instruments from these manufacturers. Some of these changes have led probe microscopists to begin leading their community into the larger field of nanotechnology. This accords well with some nanotechnologists' vision of probe microscopy, but integrating the two cultures has proven difficult.

BIOGRAPHICAL SKETCH

Cyrus Mody was born September 25, 1974 in Omaha, Nebraska and spent the first year of his life in Rabindranath Tagore's dresser drawer at the Theosophical Society in Madras, India. After moving around until he was five, he spent his childhood in Lawrence, Kansas, where his interest in science was awakened by attending summer courses at the University of Kansas' Museum of Natural History. He graduated in 1997 from Harvard University with an A.B., *magna cum laude*, in mechanical and materials engineering. That same year, he enrolled in the Department of Science and Technology Studies at Cornell and began the work that led to this dissertation.

For my parents

C.M.S. and Janet Mody

and

in honor of

small, lopsided creatures

ACKNOWLEDGMENTS

Many people's voices can be heard in this dissertation; and though none of them bear any responsibility for the defects and errors of this work, they deserve many thanks for its broad outlines. No voices can be heard here more strongly than those of my committee members. Ron Kline was my adviser for my first three years at Cornell, and his kindness and eagerness to learn new things made it easy to enjoy studying the history of engineering and technology. For administrative reasons, Mike Lynch became my adviser just before I began the research for this dissertation, and his calm puzzling out of social analysis has pointed me to many of the social knots discussed here. Even when I could not quite explain what the project was, Mike had faith that it would come to something; and when I could explain it better he offered the encouragement and insight to make it better. Though never my adviser, Trevor Pinch offered an enthusiastic model for the spirit and methodology of qualitative research. Since Trevor so obviously enjoys his research, I've tried to follow his path; I hope some of the fun of meeting probe microscopists comes across in this dissertation. Finally, Peter Dear joined my committee at the very end as my field-appointed reader. My first acquaintance with science and technology studies was in Peter's classroom, though, and his probing attitude gave me the sense of what the field was about. One question from Peter can be worth hours of discussion with most people, and several of his questions can be seen in this study.

Once I started doing the research for this project, I began meeting scholars outside Cornell whose interests resonated with my own, three of whom deserve special thanks. Dave Kaiser at MIT was the first to give me a sense that what I was working on could be interesting to a wider audience, and the first to point me to thinking of the settings described in Chapters Four and Five as pedagogical environments. Arne

Hessenbruch at the Dibner Institute and MIT was the first person I met who agreed that the STM and AFM were fascinating enough to deserve study. Like Peter, one question from Arne can stir up many insights, and his ribbing has definitely made my analysis better. Finally, Davis Baird at South Carolina introduced me to the nascent community of historians, philosophers, sociologists, and anthropologists of nanotechnology. His generosity, and the discovery of this dynamic group of scholars, recharged my enthusiasm for the project in the final stages.

It goes without saying that this project would have been impossible without the help of numerous probe microscopists all over the world. A list of the people I interviewed is in the Appendix, but I would like to thank a few individuals for their support. Rowan Dordick, Mike Ross, and Martin Hug at, respectively, the IBM research labs at Yorktown Heights, Almaden, and Zurich helped me set up interviews, and provided me with names, documents, information, and encouragement. I had a uniformly positive experience meeting Big Blue's gang of STMers and AFMers. Also at IBM, Jane Frommer showed an abiding curiosity about my project and a willingness to point me in interesting directions. Similarly, Joe Griffith at Bell Labs offered me his unique insights and support both when I interviewed him and later when I took his and Phil Russell's microscopy course at Lehigh. From Digital Instruments, Virgil Elings and James Massie both gave me fascinating, if very different, meals. Also at DI/Veeco, Ken Babcock was an enthusiastic host, and he and Matt Thompson gave me invaluable leads on further sources of information. Two former Park Scientific Instruments employees, Mike Kirk and John Alexander, played a similar role in putting me in touch with the network of Park and Quate group veterans. Several representatives of smaller firms also made me feel at home: Klaus Weishaupt (WITec), Paul West and Gary Aden (Thermomicroscopes), John Green (RHK), Dave Farrell (Burleigh), Robert Sum (Nanosurf), Thomas Berghaus

(Omicron), Stuart Lindsay (Molecular Imaging and Arizona State), Eric Henderson (BioForce Labs), etc. Among academic microscopists, Paul and Helen Hansma of UC Santa Barbara were gracious hosts for an afternoon; Randy Feenstra at Carnegie Mellon was a generous interlocutor who sharpened several of the arguments in this dissertation; and Paul Weiss at Penn State was a helpful and familiar face at the 2001 STM Conference in Vancouver. At NIST, John Villarrubia and Joe Stroscio helped me find out whom I should talk to at an early stage in the project, and Russ Young gave me time and documents over an extended period. Jim Murday at the ONR gave me multiple interviews even when he was swamped with his role in managing the nation's nanotechnology effort. Last, but by no means least, Steve Sass, Uli Wiesner, and various students, professors, postdocs, and technicians in the Materials Science and Engineering, Chemistry, and Physics Departments kindly gave me access to their laboratories and answered my queries about their work. The probe microscopy community contains many different, fascinating yet contradictory, stories. I am sure many probe microscopists will find things to disagree with in my narrative. I hope they understand how delighted I have been to be associated with this field, and how hard I have tried to narrate the sum of their hopes, fears, and triumphs.

Numerous institutions supported this research over the years. The National Science Foundation, in particular, funded me for my first three years at Cornell, and gave me a Dissertation Improvement Grant to do most of the interviews listed in the Appendix. The American Institute of Physics also gave me funds for interviews, and allowed me to do research in their archives. Similarly, the Chemical Heritage Foundation gave me a travel grant to learn more about the history of surface science, as well as a place to work in the year after completion of my dissertation. The National Bureau of Economic Research and the Institute of Electrical and Electronics Engineers both gave me stipends while I was finishing up my research and writing my

thesis. Finally, the Lemelson Center at the Smithsonian's National Museum of American History let me do much-needed writing, reading, and research in Washington, D.C. for a summer. I'd particularly like to thank Maggie Dennis at the Lemelson for helping me navigate the museum. Also at the NMAH, Paul Forman's rigor combined with his boyish enthusiasm for nanotechnology gave me a much clearer sense of how to do the *history* of probe microscopy. Last but not least, the administrative staff of the Cornell Department of Science and Technology, particularly Deb van Galder, Judy Yonkin, and Lillian Isacks were extraordinarily helpful in securing these grants, as well as in making the most of being a member of the department.

For much of my time at Cornell, when I was not writing my dissertation I was researching it or cogitating on it. Nevertheless, despite all the distraction, I hope my friends and family know that their quirkiness and support was never far from my mind. I cannot name all of them here, but a few deserve special note. Dan Plafcan and Jamey Wetmore were both my roommates in the middle of my graduate career; their decidedly eccentric normalcy made tough times less dark and good times more pleasant. In the later stages of my stay in Ithaca, Christina Dunbar-Hester and Kevin O'Neill (and his cousin Jack) were great friends and deviants; without them, the analysis in this dissertation might have taken a much more paranoid tone. The Inkeles clan – Aunt Margery, Uncle David, and cousins Charles, Barbara, John, and Laura – gave me a refuge away from Ithaca and plenty of energy and love. Finally, my parents, Cawas and Janet Mody, always saw that writing this story was my path and calling. Their integrity, curiosity, and humanity can hopefully be heard on every page.

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Chapter One

Introduction to Probe Microscopy

In the summer of 1998, I began an ethnographic study of a small research group in the Materials Science and Engineering department at Cornell. This project continued, off and on, for almost three years before it became subsumed within my study of scanning probe microscopy. Although this ethnography began with an open-ended purview (I think my original vague intent was to examine interactions between different kinds of sub-disciplinary knowledge in a group researching composite materials), it very quickly focused on issues of dirt, contamination, purity, and (later) sound in the materials science workplace (Mody 2001; forthcoming-b). But I was also fascinated by the ways these materials scientists coordinated their use of instrumentation with their preparation of materials and their practices of interpreting data. In particular, their work entailed putting the "same" materials (their sameness diminished by the work of preparing them to be examined under different instruments) through two very different kinds of microscopes – the transmission electron microscope (TEM) and the atomic force microscope (AFM) – and using images from those microscopes to generate new knowledge and inflect future specimen preparation.

Because of my focus on contamination and purity, I was especially struck by how those issues surfaced in the differing practices of TEM and AFM. TEM is a very "dirt-aware" instrument – the microscope is housed in its own room, which is kept dark when it is running, with the door closed and a special curtain covering the door to keep out light and noise; the operator and any spectators must whisper and keep still, to avoid knocking the microscope console; there is a phone in the room, but the ringer is turned off to prevent jarring sounds; the sample is kept in a high vacuum, which takes half an hour to pump down; and, even so, most samples degrade very quickly, and the operator is in a constant battle with time and contamination to salvage a good image. On the other hand, the AFM was, at least among my informants, a very dirt-indifferent tool – the door was kept open, the lights were kept on, the noise of pumps and air conditioners was loud and constant, the sample was exposed to the air, and our habitus, as operators and spectators, was much more relaxed.[1]

These were the kinds of observations that initially drew my attention to the AFM. My interest in it as a dissertation topic, however, arose from other features. I was struck by: (A) how commonplace and useful it was, a quintessential tool of "small science"; (B) how surprising that routineness was, given that it was only thirteen years old at the time; and (C) how surprisingly invisible it was to historians, philosophers, and sociologists of science, given how remarkably, and rapidly, successful it was. I knew of some excellent histories of electron microscopy, especially Nicolas Rasmussen's *Picture Control* (Rasmussen 1997), and of Ian Hacking's marvelous explication of light microscopy in *Representing and Intervening* (Hacking 1983), but I had never even heard of the AFM before my ethnographic work, although I was aware of its more glamorous cousin, the scanning tunneling microscope (STM).

As I began to look more closely at the issues surrounding the AFM, I saw that it was, indeed, an exemplary case study of a modern laboratory technology. By studying a laboratory artifact like the AFM, I could work at what I saw as the rich intersection between the sociology of science and the sociology of technology. That is, I could analyze both the material practice of science, in which knowledge is made through the manufacture of "epistemic things" (Rheinberger 1997) housed inside highly specialized pieces of equipment; and I could analyze the technological

[1] See Douglas (1966) for the classic study on dirt and social organization. See Bourdieu (1990) for an explication of the concept of habitus.

marketplace of science, in which lab equipment is invented, developed, traded, bought, sold, and tinkered with by a variety of actors.

Thus, this dissertation was originally undertaken as a case study of a "typical" instrument, and much of the analysis here is likely to be relevant to understanding the development of other laboratory techniques. In taking a second glance at the AFM, though, I learned that this particular instrument had special features that made its story important on its own merits. First, I learned that the AFM was merely the most common member of a very large family of "scanning probe microscopes" – a family of thirty or forty different instruments each only slightly varying from the others, yet each with its own domain of samples and practitioners. From an earlier project (Mody 2000), I was interested in technological variation and hybridity, and I saw probe microscopy as an opportunity to explore this theme further.

Moreover, the oldest member of the probe microscopy family, the STM, had an intriguing history that invited analysis. Although, as we will see, it was not the first microscope to resolve individual atoms, it was the first to make images of the atom publicly notorious (particularly Don Eigler's manipulation of xenon atoms to spell out "IBM" – see Figure 1-1). It was, in some sense, atomic resolution that secured the 1986 Nobel Prize for the STM's inventors, Gerd Binnig and Heini Rohrer (along with one of the inventors of the transmission electron microscope, Ernst Ruska). Yet the *exact* sense in which atomic resolution was important was, on closer inspection, elusive. There was, as we will see, a story about the relationship between instrumentation and disciplinary community that underlies the simple notion, much loved by philosophers of science, that the STM can see atoms.

This is all the more apparent in contrasting the STM and AFM. Though Gerd Binnig was central to the invention of both instruments, he won the Nobel Prize for the STM, and philosophers and pop historians of physics and nanotechnology give

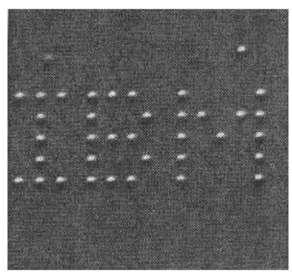

Figure 1-1: "IBM" in atoms. Don Eigler's famous use of a low-temperature STM to position and then image individual xenon atoms on a nickel substrate to form the letters "IBM." From Eigler and Schweizer (1990).

much more epistemic weight to the STM. Yet there are thousands more of the *lower*-resolution AFM at work today, across a much wider range of research disciplines and industrial settings. By studying the full spectrum of probe microscopes, in both academic and corporate contexts, I hoped to say something about how scientific instruments take root – and to show that the routine utility of the AFM is at least as epistemically interesting as the atom-resolving éclat of the STM.

Indeed, probe microscopy has had an enormously complicated journey from maverick, Nobel Prize-winning basic science, to corporate and academic research at the elusive boundary between science and technology, into shoe-string garage start-up companies, and finally into the multimillion dollar worlds of both semiconductor manufacturing and nanoscience research. In trying to do justice to this complex story, this dissertation will follow the STM and AFM into all of these contexts; indeed, it is one of the distinguishing hallmarks of this study that it traces a laboratory technique not just through the traditional stages of invention and replication, but on to the equally intriguing phases of routinization and commercialization. Moreover, the rapid diffusion of probe microscopy into a variety of sectors offers interesting insights for both the sociology of science and technology. As we will see, the diffusion and routinization of a technique is not an undifferentiated process. Because probe microscopy has been taken up (and, in some cases, spurned) by so many communities, its story nicely demonstrates that different subcultures have different ways of judging and appropriating instruments, and that instruments often need to be reinvented in order for routine use to go forward in new contexts.

Relevance to Nanotechnology

Finally, one reason why the probe microscopy story merits special attention did not emerge until I was well into my study. When I began my ethnographic work in 1998, neither I nor my informants had much inkling of the changes soon to be wrought on the AFM community by the advent of coordinated governmental support for nanotechnology. The AFMers I knew did occasionally interact with local nano institutions – they would, for instance, occasionally prepare samples in the clean rooms at the Cornell Nanofabrication Facility – but they did not consider themselves nanotechnologists, even though most of their work centered on nanoscale features (i.e., features with characteristic dimensions on the order of 1-100 nanometers or 10^{-9} to 10^{-7} meters). Moreover, the leading nanotechnologists at Cornell were not probe microscopists, and most of the probe microscopes on campus were not paid for, managed, or even used under the "nanotech" umbrella.

After the Clinton administration's founding of the National Nanotechnology Initiative in 2000, though, it became more difficult for my informants (and myself) to be unmindful of the nanotechnology phenomenon. AFMers at Cornell began to vie for NNI funding, they became more vocal in the organization of nano research on campus, and, most interestingly, research groups with a marginal interest in probe microscopy nevertheless began sharing AFMs and using the instruments to coordinate their work so that their joint projects would more closely resemble the NNI's vision of interdisciplinary nanotechnology. As I began supplementing my ethnographic work with oral histories of old-time STMers and AFMers, I found that almost none of them had had any engagement with nanotechnology discourse before the very late '90s, yet by 2001 many of them were clearly gearing up to become nanotechnologists, even if they were also quite skeptical about what nanotechnology was.

At the same time, in reading some of the early writings of nano-visionaries like Eric Drexler and interviewing some of the leaders of the NNI, as well as meeting some of the first historians and philosophers of science to engage with nanotechnology, I learned that no instruments were more symbolically crucial to this highly instrumented field than the STM and AFM. Probe microscopes were *the* tools people like Drexler and Mike Roco pointed to in heralding the coming of this "next industrial revolution" (Anonymous 2002). Clearly, STM and AFM were enormously important for nano, and nano was becoming enormously important for STMers and AFMers. There was, though, an unusual time lag of almost 15 years in making the interests of the nano and probe microscopy communities mutual.

Almost all of this study focuses on events before or during the onset of that time lag; thus, nano is largely an off-stage presence in this story. Readers will find characters making occasional gestures toward nano – talk of nanometers, Nanoscopes, nanotubes, NANO Conferences – but these made for very loose ties between the nano and probe microscopy communities until the turn of the century. There are, however, important lessons to be drawn about nano throughout this work. Readers who are interested in nanotechnology should take this as a history of an important family of nano instruments; and, as I will make clear in the epilogue, the way the probe microscopy community approaches nano has much more to do with its own complicated history than with any self-evident relation between SPM work and nanotechnology. That is, seen from within the probe microscopy community, nano is only an obvious direction to pursue because it has become a convenient device for easing frictions brought on by the commercialization of the instruments and the segmentation of STM and AFM research.

More generally, nanotechnology provides a fascinating case study in discipline formation, and the story of STM and AFM provides insight into how nascent disciplines incorporate or appropriate instrumental subcultures. We do not often have the chance to watch a large social movement in science get underway, particularly one that, like nanotechnology, proceeds by patching together numerous pieces of "small science" to form a "big science" undertaking. In many ways, nanotechnology is more a community of communities than a discipline. In this, it resembles other postwar transdisciplinary constellations, some of which (e.g. materials science) eventually became disciplines, and others (e.g. cybernetics) did not.[2] The role of instruments and other laboratory artifacts in seeding disciplines and catalyzing scientists' professional identities is still in need of analysis, and this dissertation seeks to contribute to discussions of these processes. Thus, though nanotechnology is only implicitly important to much of this text, the need to understand the role of STM and AFM in the formation of communities (including nano communities) and technical identities (including researchers' identification as nanotechnologists) has strongly shaped this study's methodology and analytic frame.

Methodology and Analytic Perspectives

This dissertation was carried out in a Department of Science and Technology Studies, so the tools and questions I have used to understand the development of probe microscopy are those most closely associated with the S&TS community.[3] Three strands of work in S&TS, though, were most influential in guiding what questions I asked and what methods I used to answer them. The first is the actor-network theory (ANT) framework associated with Bruno Latour, Michel Callon, John Law, Madeline Akrich, and others (Latour 1988b; Callon 1986; Akrich 1992; Law 1987). ANT pictures a world in which knowledge and credibility are generated through the construction of densely interconnected networks of people and things (or, collectively, "actants"). To understand science and technology, Latour says, analysts must "follow the actors" through these networks. Thus, this study attempts to follow a large number of the actors relevant to probe microscopy through a wide cross-section of the settings in which STM and AFM have taken form.

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[2] For some analyses of other postwar umbrella disciplines, see Reardon (2001); Bensaude-Vincent (2001); Pickering (1995).

[3] For a more or less comprehensive review of the field in the early '90s, see Jasanoff, et al. (1995). For more recent compendia of, respectively, the science and technology sectors, see Biagioli (1999) and Mackenzie and Wajcman (1999).

Although Callon and Latour always regarded humans and non-humans as equivalent agents in their networks, the version of ANT presented in *Science in Action* leaves room for agnosticism about non-human agency. Indeed, some science studies scholars outside the ANT school only realized much later how central non-human agents were to Callon and Latour's vision.[4] Later instantiations of ANT (Latour 1996; 1999b) have put more epistemic weight on the theory of non-human agency and the politics of "things". This dissertation concerns itself much more with the methodological recommendations and ontological ambivalence of *Science in Action*. One way to do so, I believe, is to read ANT in the light of the work of Michel Foucault. Though it is problematic to lump Latour uncritically with Foucault, the aspects of ANT that are most relevant to this dissertation are those shared with the Foucault of *The Order of Things* (1994b), *The Birth of the Clinic* (1994a), and, especially, *Discipline and Punish* (1977a). Latour's idea that scientific knowledge is accredited through the transformation of non-laboratory settings into laboratory-like fields is akin to Foucault's notion of the "capillarity of power," in which regimes of discourse and knowledge propagate through ever more physical spaces, molding ever more of the behavior, thought, and self-conception of knowing subjects within those spaces.

The diffusion of knowledge and artifacts is a central concern of this dissertation, and I have looked at this process in terms of the Latourian "enrollment" of actors whose interests are manufactured to intersect with those of network-builders; and I see this as a simultaneously Foucauldian process of manufacturing and disciplining subjects whose technical identities (Haring 2002) take their shape from the networks of things and people surrounding them. We will closely follow actors

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[4] See, for instance, the debate in Collins and Yearley (1992a); Callon and Latour (1992); Collins and Yearley (1992b), as well as Bloor (1999) and Latour (1999c).

around the networks of probe microscopy as they attempt to enroll each other for various projects; we will also watch as the *circulation* (Latour 1999a) of materials and people through networks helps to transform “ideas” into “facts” and idiosyncratic experiments into black-boxed instruments. This story is not incompatible with, but does not explore, the non-human ontology of ANT; if readers wish, they may see certain kinds of probe microscopists as acting as “proxies” (Callon 1986) for the microscope in advancing its interests within the network. Certainly, probe microscopists do occasionally speak of the microscopes as having a will of their own that bends human action around them.

The second S&TS framework implicit in my methodology draws on Peter Galison’s work on so-called “trading zones” (Galison 1996; Galison 1997). I take the trading zone to be a setting where different *types* of actors (often people from different disciplines) come together and create knowledge not by generating “facts” that are acceded to by all, but by allowing their various knowledge sets to remain differentiated yet generatively overlapping. Members of a trading zone find *ad hoc* pieces of knowledge, practice, and materiel that they can exchange, without needing to arrive at a fully mutual understanding of the tokens of exchange or even the exchange event itself. Rather, they hammer out “pidgins” and “interlanguages” that suffice to allow their local mix of cultures to remain coherent. With time, this local mix may even become structured enough that its pidgin will become an autonomous “creole,” a patois which new members can learn without knowing any of the parent dialects.

The trading zone concept can, at times, be abstractly metaphorical in ways that invite analytical misuse and allow it to stretch to encompass an unmanageable number of situations.[5] For these reasons, I have tried to offer criticisms where appropriate, and to supplement the concept with tools from other S&TS literatures, particularly works

[5] E.g. Fuller (1996).

in the philosophy of technology and instrumentation (Ihde 1991; Baird 1993; Hacking 1992; Dupré 1993), especially with respect to the relationship between laboratory artifacts and embodied knowledge, and the history and sociology of engineering (Layton 1971; Constant 1983; Kline 2000a) with regard to the autonomous ways of knowing of artifact-creating subcultures. The basic trading zone idea, though, can be enormously fruitful, especially in describing the modes of instrument development seen with STM and AFM. Some local cultures within the probe microscopy community (particularly those described in Chapters Five and Seven) seem to be tailor-made trading zones; indeed, as the STM and AFM gradually moved out of academic labs and into small start-up manufacturers, the once metaphorical "trading" zone underwent an exquisite literalization.

Thus, the methodology of this study was structured to make features of local trading zones conspicuous, and to take advantage of distinctive features that would nuance the trading zone concept. I have tried to pay attention to the disciplinary backgrounds of many of the actors in this story, and the traditions of experimentation and instrument-building in which they were trained; but I have also highlighted what Galison calls the "thick border regions between disciplines," places where disciplinary identity is important but where it is also constantly being remade. Two such settings are most important to this study: pedagogical environments, where the creation of knowledge is inextricably tied to the creation of new knowing subjects (Kaiser 2002; 2000); and contexts of commercialization, where participants eagerly throw different kinds of actors together in order to expand the reach of their trading zones.

Finally, this dissertation has most been influenced methodologically and analytically by the sociology of scientific knowledge (SSK) tradition and its variant, the social construction of technology (SCOT) program (Bloor 1991; Collins 1981; MacKenzie 1990; Bijker and Pinch 1987). SSK views the formal, symbolically encoded, textbook knowledge beloved of positivist philosophers of science as an incomplete picture of the knowledge generated by and used in scientific settings. Formal knowledge is only intelligible when it is accompanied by some local, embodied, "tacit" knowledge that cannot be entirely symbolically encoded (i.e. written down) and which is judged less by "correspondence to reality" than by the standards of communities of practitioners. In particular, the brand of SSK associated with Harry Collins focuses on the replication of scientific experiments, using Wittgensteinian epistemology to show that there is no absolute standard for deciding whether an experiment has been correctly replicated, but only standards that are infused with the sociological understandings of the participants. Indeed, as Steven Shapin and Simon Schaffer show, the social order of a "core set" of experimenters emerges along with the knowledge generated by the experiment (Shapin and Schaffer 1985).

The SCOT program takes the model offered by SSK and applies it to understanding how the shapes of various technologies come to seem as intuitively "factual" as most scientific knowledge. As with SSK, SCOT zeroes in on variations in artifacts that are presented as solutions to the "same" technological problem and shows how various "interpretations" of a technology (its design, its use, who should be using it, what kind of social organization should be arrayed around it) are associated with relatively homogeneous "relevant social groups." With time, processes of negotiation between (or disenfranchisement of) the various relevant groups winnows down the viable interpretations of a technology, until, as in SSK, "closure" is achieved and the basis for the outstanding technology is usually reconstructed as the rational, practical one, rather than a matter of contingent and socially constructed negotiation.

Thus, both SSK and SCOT find the best sites for their analysis in moments of dispute, uncertainty, negotiation, and transition. Since such moments are hard to catch in the act through ethnographic methods, the historical case study has been one of the mainstays of this style of sociology. Yet because the features that SSK and SCOT emphasize are intangible, unformalized aspects of practice, an ethnographic sensibility is often necessary. While such a sensibility can be maintained even in studies which rely exclusively on textual materials and archival records – Shapin and Schaffer's *Leviathan and the Air-Pump* being the best example – most practitioners of SSK and SCOT choose to highlight the role of interpretive flexibility and tacit knowledge by interviewing participants in relatively recent scientific and technological controversies, and by achieving a comprehensive, actors-eye understanding of the relevant technical material, often through participant-observation and other ethnographic techniques.[6]

Several of the core concepts of SSK and SCOT serve as benchmarks for the analysis in this study. Above all, this dissertation takes seriously the analytical connection between SSK and SCOT. I will try to show that researchers whose work revolves around scientific instruments are very skilled at pushing their work practically and rhetorically back and forth between "scientific" and "technological" registers. Also, tacit knowledge is an almost inevitable phenomenon in any discussion of probe microscopy – both for analysts and, intriguingly, for the actors themselves. Previous studies in SSK and SCOT have shown the *importance* of tacit knowledge in the transmission of scientific knowledge and the diffusion of technological artifacts. This study aims to elaborate some of the *mechanisms* of transmission and diffusion and the consequences of those mechanisms for the communities involved. Finally, this study supplements the project in both SSK and SCOT of deconstructing the boundaries between production and consumption. As we will see, "audiences" for scientific knowledge, and "users" of laboratory technologies strongly shaped the production and development of facts and artifacts.

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[6] Again, though this kind of participant-observation is usually occasioned by studies of recent science, it is not strictly limited to controversies that took place within living memory. See Sibum (1995).

This dissertation will use these three analytic perspectives to frame one central question: how do some laboratory technologies go from being idiosyncratic, home-built tools used by a small group of researchers to being mass-produced devices indispensable to the work of researchers and manufacturers throughout the world? The language of actor-network theory, with its vocabulary of "networks," "enrolling," and "black-boxes," particularly lends itself to thinking about the standardization and diffusion of artifacts. In this case, artifacts embody knowledge made at one node in a network, carry that knowledge to other nodes, and by so doing require the new nodes to reproduce the work of the original. The trading zones concept, too, contains an inherent notion of diffusion (signaled by the very metaphor of "trading"). Actors within trading zones necessarily impart some of their intellectual and material wealth, which then can move far from its original context. Importantly, this kind of diffusion necessarily involves the reworking and transformation of the tokens of trade as they are appropriated by new communities – as we will see, diffusion is not a simple matter of "technology transfer," but a complicated process of adjusting a technology and the practices surrounding it to local circumstances (and *vice versa*).[7] Finally, SSK and SCOT have been fascinated by diffusion from the beginning. This dissertation will particularly use Harry Collins' (1974) analysis of "tacit knowledge" (i.e. knowledge that cannot be fully written down or encoded formally) as a starting point for examining the spread and routinization of probe microscopy. Interestingly, we will see that (as might be expected from the trading zones literature) replication and diffusion of laboratory technologies is accomplished through a *two-way* exchange of tacit knowledge, and that (as might be expected from actor-network theory) a few

[7] For this reason, Kaiser (forthcoming-a) and Jordan and Lynch (1992) prefer the term "dispersion." Diffusion, as Latour (1987) argues, can carry the unintended connotation of an inexorably physical process (like smoke diffusing through a room) in which the item being diffused is unchanged by its spread. Readers should know that this is not the interpretation I assign to "diffusion."

"centers of calculation" (or, here, centers of experimentation) are vital for regulating the simultaneous flow of instrumentation and tacit knowledge.

In addition to demonstrating the cogency of existing treatments of diffusion and tacit knowledge, I will use the probe microscopy case to supplement and critically reëxamine these widely-used concepts. Take, for example, the concept of the "black box" that is central to many analyses (including Latour's) of the diffusion of technology. We will see in Chapter Eight, for instance, how notions about black boxes are not just analytical tools that allow us to make sense of what probe microscopists do; a version of that very concept is a subject of enormous debate among probe microscopists themselves. Laboratory technologies are not inexorably transformed into black boxes; rather, the *possibility* of black boxing is a political point of contention among researchers, which accompanies and affords their efforts to draw particular kinds of internal and external social boundaries. In Chapter Seven, we will see how the process of commercializing instruments offers some new twists on the traditional notion of the trading zone; we will examine how a particular kind of commercializer of instruments (the figure I have called a "boxwallah") inhabits dynamic trading zones. In order to live at this margin, the boxwallah must learn not just one interlanguage, but rather dozens of 'pidgins' – the partly overlapping 'languages' of materials science, electrochemistry, biophysics, molecular biology, mineralogy, catalysis, electrical engineering, etc. (not to mention even more arcane, and sometimes proprietary, jargons of various industrial processes). Finally, throughout the dissertation, we will see that the original conception of a "relevant social group" that is so central to the SCOT approach is too 'flat' to account for the diffusion of many technologies; particularly the kind represented by the family of probe microscopes discussed here. All social groups are internally differentiated, and those grouped around technologies are no different; indeed, the complex array of members' roles *within* a social group can give a technology its form and enable it to diffuse. We will encounter social groups that include members who are also affiliated with external communities (some of which become 'internal' communities); often, these people are brought in specifically to *manufacture* the relevance of the technology to their home communities – that is, not. or not only, helping with the concerete manufacture of technological components, but also forging links and weaving together 'languages' and skills in an extended community; thereby thickening ties between groups that allow for the spread of the technology. Indeed, not only the relevance, but the social group itself, must sometimes be molded into existence in the course of hammering out the form of the technology. Groups are not simply there to take up a technology; rather, groups and subgroups (e.g. "AFM-using biophysicists" or "STM-using electrochemists") take shape with the technology, and change form as the technology matures.

I will also develop a number of novel themes and analytical that will supplement these three bodies of S&TS literature and further strengthen the relation of those literatures to this study of the diffusion of laboratory techniques. Chapters Two and Three will focus on *canonical materials* and their importance in making lab technologies viable; one of the fastest ways to make a technique relevant to a new community is to link it to the epistemic materials considered interesting and generative. Chapter Three introduces a particular notion of *naïveté* that was an important vehicle for developing and diffusing probe microscopes; science and technology studies have long been concerned with performances of expertise (Collins and Evans 2002; Jasanoff 1992) in the accreditation of knowledge, but performing a naïve style can be just as crucial in aligning support for a technology. Chapters Four, Five, and Seven trace the importance of *pedagogy* in the creation of new knowledge and techniques; I will show that the production of new microscopes was rarely separated from the task of training new microscopists. Chapters Six and Eight follow the often-neglected disruptions and anxieties that can attend the successful diffusion of a technique. When an instrument moves too far too far it can induce an *experimental vertigo* that results in controversy; and when it has been *too* successfully routinized it can provoke role anxieties among its adherents. Finally, the entire dissertation is concerned with the nexus of commerce and science in all its forms. Commercial interests are sometimes thought of as sullying the "purity" of knowledge-making; and at other times, commercial interests are thought of as rationally selecting from the fruits of research to produce better (more cost-effective, efficient) technologies. I will show that neither of these pictures applies to probe microscopy. Rather, we will see that commerce is not a foreign, impure instinct for researchers, nor is it a way to rationally maximize profit. Rather, it is usually a way to solve exigencies brought on by the local culture of experimentation; throughout I will try to illustrate these local cultures and show how commercialization of research could be a culturally viable adaptation.

In explaining the mechanisms of diffusion, I have adopted a methodology inspired by actor-network theory and the notion of trading zones, but which most closely resembles those of classic studies in constructionist SSK and sociology of technology. To begin with, I drew on my ethnographic acquaintance with users of AFMs to come close to an actors' competence in the operation of these instruments. Indeed, I was aided in this by the fact that no participant in the wider probe microscopy community can demonstrate full competence across the astonishingly wide range of uses of these instruments; there are simply too many variants, too many different modes, and too many different kinds of samples particular to too many different "communities of practice" (Wenger 1998) for anyone to fully comprehend them all. In trying to accommodate my analysis to this fact, I took as my model key kinds of mediators in the probe microscopy community – such as grant officers, postdocs, microscope builders, and instrument manufacturers' representatives – whose jobs entail quickly coming to terms with local variations on the design and use of the microscopes.

Next, I began interviewing long-term participants in the probe microscopy community, trying to draw out thick actors' descriptions of the mechanisms of transmission and diffusion, the operation of trading zones, and the building of networks. These were semi-structured, face-to-face oral histories, revolving around the interviewee's personal experiences working with the microscopes and being a member of the probe microscopy community.[8] By digging through the literature, I was able to isolate a few key locales which had been instrumental in the early development of the STM and AFM – the National Institute of Standards and Technology, the Naval Research Lab, Stanford University, the University of California at Santa Barbara, Digital Instruments, Park Scientific Instruments, Bell Labs, and the IBM research labs at Almaden, Yorktown, and Zurich – and I put those interviews at the top of my list. For any given excursion to see people who had been involved early on with the STM or AFM at those places, though, I also had many opportunities to talk with people who had come to probe microscopy much later, or whose involvement with it centered much less on design and engineering and much more on routine use. The former group provided the richest oral histories; but the issues that interested me focused not on one group or the other, but on the sites in which each were thrown together (or sought each other out), the ways in which they

[8] A list of interviewees is in the Appendix. Throughout the dissertation, interviews will be referenced, in square brackets , by the alphanumeric code given in the Appendix plus the date of the interview – e.g. [VE1, 3/20/01], unless interviewees have asked that their names be withheld (in which case a rough descriptor of the interviewee will be supplied with no date– e.g. [DI executive]). Some of the interviews will be publicly available in the near future. Readers interested in reading transcripts should contact the author.

traded representations of the instruments, and (as I came to realize) the types of people who catalyzed interactions between the two.

I also managed to fit in some more participant-observation at key sites, such as lab groups at Cornell that used the AFM, training courses for users of industrial AFMs, and big professional conferences for the probe microscopy and nanotech communities. These dimensions of my research were most relevant to the closing chapters of this study; for earlier chapters, which focus on participants who are more difficult to locate or whose memories are fading, I supplemented my interviews with archival research, particularly at the National Archives, the Smithsonian, and the American Institute of Physics. Other textual materials – journal articles, advertisements, email listservs, manufacturers' applications notes, manuals, and lab notebooks – were also crucial in making sense of my interview data.

Ethnographic Scene-Setting

In order for my readers to similarly make sense of the narrative presented here, it may be useful for me to describe what a probe microscope looks like and how it is typically used. There is, of course, no "average" SPMer – indeed, the variations among probe microscopists, across time, discipline, and setting, are what drives this story – but the practices of most STMers and AFMers bear enough of a "family resemblance" that it is worthwhile picking out some of the distinctive features of the clan. One important characteristic is that most of the practice of using a probe microscope takes place away from the instrument itself. Many users spend most of their time *preparing* samples to then quickly "characterize" with the microscope. Indeed, both in academic and industrial settings, sample preparation *is* the experiment at stake; users only turn to the STM or AFM when they believe the instrument can inform them about the results of a change in sample preparation procedures – does a new process step created the desired nanoscale properties, does it make the surface hotter or bumpier or stickier, does it introduce new kinds of defects, etc.[9]

Indeed, for many users their STM or AFM may be just one stop in the "career" of the samples they prepare.[10] My informants at Cornell, for instance, had a kind of peripatetic epistemology – they would move from spot to spot around campus, using a variety of tools to prepare and characterize their samples, hoping in the end to coordinate data generated from different characterization tools in order to learn something about the effects of different sample preparation techniques.[11] Other probe microscopists – especially surface science STMers – tend to do sample preparation close to the microscope itself (often in the same vacuum chamber). In general, sample preparation is the part of probe microscopy most specific to the practitioner's discipline – it is usually the product of a long tradition in their community or institution of manufacturing materials in order to make them amenable to the gaze of the microscope, ways of carving out specific entities from the sensible flux of the world in order to generate particular kinds of knowledge about them.[12] The types of practices involved in specimen preparation vary widely, but they include things like cleaning or heating materials, exposing them to various chemicals, or growing crystals or biological specimens under specific conditions. For probe microscopists working in industrial laboratories, "specimen preparation" may refer to the practice of bringing

[9] Although most SPMers use the microscope as a characterization instrument, some also use it to manipulate samples – i.e., to "intervene" as much as to "represent." SPMs are unusual among microscopes in that the same mechanism that allows a user to look at a surface can also allow them to modify it – to drag tiny objects (even atoms) around, or to scrape and inscribe features (like a nano-chisel) into the sample.

[10] I draw on Becker (1963, 25ff.) and Goffman (1961, 125ff.), as well as conversations with Mike Lynch, for the notion of the "career" of a sample.

[11] An instance of what Bruno Latour has called "circulating reference" – i.e., the *generation* of a correspondence between object and knowledge *through* the movement of objects and inscriptions around a network (Latour 1999a).

[12] The notion of unrefined reality as "sensible flux" comes from James (1996). I draw heavily on Rheinberger's notion of "epistemic things" (Rheinberger 1997) in analyzing how research communities appropriate particular materials and rework them to make them more amenable to generating knowledge.

in defective or randomly selected products and examining them to understand whether/why they are broken, or how experimental process steps affect them.

Once specimen preparation is done, probe microscopists bring their samples to the microscope itself. Since an AFM is (relatively) cheap – about $200,000 for one with most of the bells and whistles – and relatively easy to learn, even many small academic research groups (at least in Western Europe, Japan, and North America) have their own microscope. Others share one with one or two other groups, or send their students and/or samples to campus microscopy centers. Also, unlike an electron microscope, an AFM is quite small – the microscope "itself" is only about the size of a coffee can, although it is often surrounded by more bulky apparatus for shielding it from vibrations. Often, vibration isolation equipment is home-made from unusual materiel – a message on a widely-read list-serv describes pails of sand, blank headstones, disused refrigerators, old acoustic hoods for noisy dot-matrix printers, inner tubes, and, probably the most popular, bungee cords or surgical tubing, used to hang the microscope from the ceiling or a stand or even the legs of an upturned table as possible ways of protecting a microscope from stray vibrations.[13]

These can give the microscope a messy, cobbled-together appearance, and SPMers sometimes express disbelief that such an *ad hoc* system can "see" nanoscale features. In more high-end laboratories, particularly in industrial settings, AFMs are often kept on very expensive optical tables or housed in special vibration isolation shields supplied by the instrument manufacturers; to the casual eye, such microscopes have a much more polished appearance. Also, STMs are generally not as exposed as AFMs. Most STMers are electrochemists or surface scientists, and keep their microscopes in bulky electrochemical cells or ultrahigh vacuum (UHV) chambers. Most STMers work in universities or do basic research at corporate and national labs,

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[13] From R. McLeary, 11/26/95, on the spmlist@di.com listserv.

though, so their microscopes have much the same *ad hoc* look as academic AFMs – especially among surface scientists, who have a penchant for covering key parts of their UHV chambers in wadded-up pieces of aluminum foil.

In general, this *ad hoc*-ness has decreased over time. The earliest STMs and AFMs were ramshackle devices connected to enormous racks of electronics with an ever-expanding array of knobs, dials, and switches [BH1, 5/9/01; BD2, 10/18/01]. Later, companies that manufacture STMs and AFMs saw it as their duty to eliminate many of the stray wires, (literal) rough edges, and proliferating knobs and switches and to present a cleaner, ostensibly more user-friendly interface [DB3, 4/3/01]. Some SPMers who built microscopes back in the '80s complain that this cleaning up has eliminated certain kinds of subtle, virtuosic control of the instrument, though most ordinary users of commercial AFMs feel they have all the control they can handle. Also, both for the instrument manufacturers and people who build their own microscopes, most of the clumsy interfaces and electronics associated with the older STMs and AFMs are now packaged inside sleek, compact personal computers.

When an AFMer today goes to use their microscope, they first put the sample into a small holder, then place the holder inside the microscope "head." Then, after some fiddling with the head to align lasers and cantilevers and so forth, they turn their attention away from the microscope "itself" and concentrate on a personal computer (often with two monitors) that they use to control the microscope. With the wonders of software, many of the tricky parts of operating an STM or AFM are today automated, though in the past it took a great deal of embodied skill and tacit knowledge to get a microscope to begin working properly. Once the microscope begins scanning and producing images, the operator has the somewhat monotonous task of monitoring outputs (today in dialog boxes on the computer monitor; in the past on an oscilloscope or voltmeter), adjusting parameters (today by typing in numbers on the keyboard; in the past by adjusting various dials and knobs), and watching as images form (today, images unveil themselves in real time on the computer screen; in the past, much cruder representations would have appeared on oscilloscopes, chart recorders, or even TVs) while making tactical decisions about where the image looks "good" or "interesting," whether to zoom in or out or reposition the probe, when data is valuable enough to save and analyze later, and when the data is so bad that the machine must be fixed or the sample must be scrapped. A microscope "run" lasts for as long as the operator can pay attention or the sample stays clean or the probe/tip stays in good condition – usually two or three hours.

After data has been taken, it must be digested and communicated. Again, this task looks very different today, when images are stored on a computer as electronic files and can be quickly and radically processed using software sold either separately or with the microscope. There is some debate about how much rendering is permitted, but SPMers today can very easily produce images of their samples as seen from all kinds of angles, in all kinds of colors, with all kinds of filters and exaggerated or processed features. Once the images have been massaged sufficiently, they are stored permanently on a disk or printed out on a color printer (in the past, they would have been stored on video tapes, or a Polaroid would have been taken of the image on an oscilloscope). Once printed out, images become Latourian "immutabile mobiles" – objects that help make facts simply by being durable as well as transportable (Latour 1988a). SPMers paste them into lab notebooks or put them into three-ring binders, email them to colleagues or manufacturers' representatives for advice, put them on overheads and discuss them in group meetings and conferences, and eventually insert them into journal articles and textbooks and advertisements, spinning elaborate interpretations that connect these images to local and disciplinary debates.

Technical Prologue

So how do SPMers think these images are being made, and what kinds of information do they think they provide? Basically, probe microscopes operate on a very simple principle, often analogized to that of a finger reading Braille or a phonograph needle riding over a record. That is, SPMs work by bringing a small, solid probe down very close to a surface and allowing the probe to skim along recording variations in the height of the surface. In order to obtain ultrahigh resolution, the parts of the probe that actually interact with the surface should be roughly the same size as the smallest distinguishable features on the surface – this is the same as saying that a finger is much larger than a Braille dot, but because the finger has very fine papillary ridges that are smaller than a single dot, one dot can be distinguished from another. Similarly an STM or AFM probe may be quite large compared to the features of a nanoscape, but if the probe ends in one atom or a handful, then the microscope will be able to see features in the 0.1 to 10 nanometer range. This is extraordinarily fine resolution – the diameter of a hydrogen atom, after all, is only about 1 Angstrom (0.1 nanometers).

The Braille analogy is a little limiting in that it implies that the probe can only sense heights if it is actually pushing against the surface. This was, indeed, the case for a predecessor to the AFM known as the stylus profilometer, invented in the 1950s, that worked by scraping a sharp probe over a sample and recording how the sample pushed back on the probe. The diversity of the probe microscopy family is made possible, though, because probes can interact with surfaces in a variety of ways and feed back off of any number of properties without having to be in contact with the material. Again, a macroscale analogy to the "ways of the hand" (Sudnow 1978) may be helpful. If you brush your hand over a piece of paper, you may be able to tell where on the paper there are Braille dots; but if you move your hand a few inches above a stove, you can also tell where on the stove the burners are without touching them. If you move your hand a few inches above an air hockey table you can tell where the vents are, and if you move your hand a few inches away from a stereo speaker, you can tell where the speaker cones are. The key in all of these examples (as in probe microscopy) is moving your hand (the probe) down close enough to the surface to sense whatever properties interest you, and then moving your hand around the surface to build up a spatial map of those properties.

The STM (the oldest probe microscope) and the AFM (the most common) will be our primary interest in this study, so their modes of operation bear special explanation (see Figures 1-2 and 1-3). The *tunneling* microscope works by bringing a sharp metal probe to within a nanometer (10^{-9} meters) of a metal of semiconductor surface, placing a voltage difference between probe and sample, and measuring the number of electrons that this voltage causes to "tunnel" between sample and tip. Tunneling is a quantum mechanical phenomenon that defies many of our intuitive understandings of the macroscale, yet which can be quite common at the nanoscale. In rather esoteric terms, tunneling allows for the movement of a particle across an energy barrier in ways that are forbidden by classical physics. That is, electrons at the surface of a metal or semiconductor are bound to the material in various empty or filled energy "bands." The interface between the surface and the vacuum or air beyond presents a large energy barrier to the electrons – they "want" to remain bound to the material rather than to move off into empty space.

A common macroscale analogy for the electron's situation is that of a lion sitting in a room surrounded by a very high brick wall. Classically, the lion has little chance of scaling the wall and freeing itself unless someone comes to its aid by offering a ramp or creating a hole in the wall. Yet quantum mechanics says that because both the lion and the electron are simultaneously particles and waves, there is

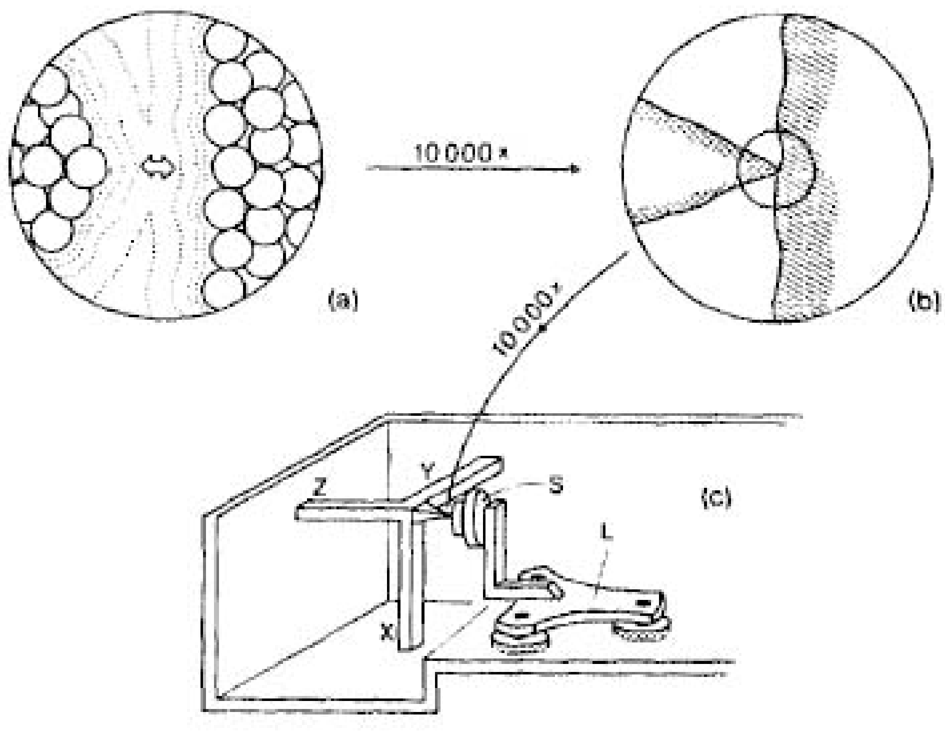

Figure 1-2: Scanning tunneling microscope. Diagram showing the design and operation of the scanning tunneling microscope. In the lower part of the diagram, the piece labeled “L” is the so-called “louse” used for coarse positioning by walking the tip down toward the sample. “S” points to the sample; “X”, “Y,” and “Z” are the three orthogonal piezoelectric scanners. The inset labeled “a” shows how atomically rough both the tip and surface are, and why this allows almost all of the tunneling to go through the outermost atom on the tip (thus giving atomic resolution of the surface). From Binnig and Rohrer (1984).

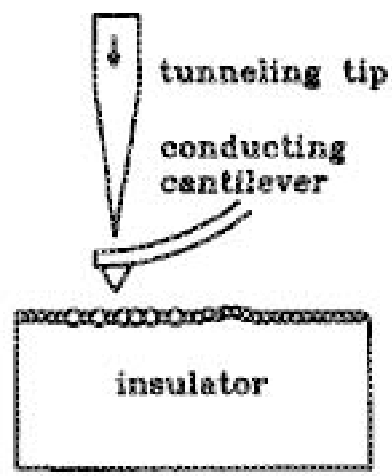

Figure 1-3: AFM with STM detection. Diagram of an early atomic force microscope. Early AFMs put an STM on the back of the cantilever to detect the cantilever's deflection (i.e. the height of the surface). This image shows the cantilever in so-called "contact mode" (i.e., with the tip touching the surface). From Binnig, et al. (1986a).

a finite (though very small) probability that their wave functions will creep over the barrier (or "tunnel" through) and they will suddenly stop being on one side and start being on the other – the electron will fly off into empty space and the lion will be free to gobble up zoo-goers. With regard to the electron, this process can be made more likely by, for example, lowering the temperature of the system or putting a very high voltage on the surface, or by putting a smaller voltage on the surface and offering the electron someplace to tunnel to – e.g. a metal probe with its own energy bands where the electron can take up residence.

In the latter case, the number of electrons that actually tunnel from the surface to the probe (or vice versa if the voltage is reversed) is exponentially dependent on the distance between probe and surface – thus, extremely small variations in the tip-sample distance will result in enormous variations in the tunneling current. This allows a tunneling microscope to resolve the height of features on a surface very accurately. It also allows the microscope to distinguish closely-spaced *lateral* features on the surface. Because of the distance-dependence of the tunneling current, virtually all the current goes through the outermost bit of the probe; and because most materials are somewhat rough at the nanoscale, that outermost point is likely to be only one atom wide. Thus, the stream of tunneling electrons is less than an atom wide, meaning that the STM can "see" where individual atoms are on a surface – imagine that if your fingertip ridges were only an atom wide, you could read atom-sized Braille.

Thus, tunneling electrons can be used to let the STM "see" very small features on a surface. The trick is bringing the probe down close enough to a surface to allow electrons to start tunneling, and then to move it around in a controlled way to build up a spatial map of the surface, all without letting the tip crash into the sample every few nanometers. This necessitates a mechanism for precisely controlling the position of the probe, and moving it in very fine increments in all three dimensions. Controlling its height above the surface (the z dimension) is often thought of as a separate problem from controlling it laterally (in the x and y dimensions). Also, both vertical and lateral movement is separated into two components, coarse motion and fine motion. Vertically, there is a need to bring the probe down to the point where electrons begin tunneling, using "large" enough steps (coarse motion) that the "approach" process takes place in a reasonable amount of time. Today, the approach is usually automated, but in the past STMers would first eyeball the probe to within a micron of the surface, then delicately bring the tip into the "tunneling regime" manually, using differential screws and levers that would take small finger movements at one end and de-amplify to translate the probe by much smaller increments at the other end [BD2, 10/18/01].

Once the probe is close enough that tunneling starts, it needs to be controlled much more finely in all three dimensions. Usually this is done with materials called piezoelectric crystals. Piezoelectrics are crystals that emit an electric current when they are put under pressure (as, for example, in the sensors in the door opener at a grocery store that signal for the door to open when someone steps on the footpad). Conversely, if a *voltage* is put on a piezoelectric crystal, it will change its shape; by very finely controlling the voltage (something which is easy to do), one can control the dimensions of the crystal at the atomic scale (something which is very hard to do by other means). Piezoelectrics have been known for a long time (the Curies did much of the original research on them) and are easy to obtain (for example, sugarcane is a common piezoelectric; many early STMs and AFMs used piezos from doorbell ringers bought at Radio Shack [BJ2, 6/27/01]). Early probe microscopes worked by having separate piezos for each axis, stacked orthogonally to each other – one to lower the probe, one to scan in x, and one to move linearly in y. This allows the probe to "raster" back and forth, forming an image (in the same way pixels are lit up on a TV or computer screen), while the probe quickly changes its height to avoiding hitting features on the surface.

The actual image is formed by sensing how the tunneling current changes as the probe changes its position in x and y; this tells the STM (via a feedback circuit) to adjust the voltage being fed into the z-piezo so that it will be at the right height. The voltage on the z-piezo is then fed into the imaging mechanism, whether a computer program or an oscilloscope. In computerized renderings, a color or grayscale scheme matches different z-voltages (corresponding to different heights) with different shades; usually lighter colors for higher features and darker ones for lower features (which gives a nice shadowing effect if there is a trench or dip in the surface).

Thus, the basic ingredients of a probe microscope are: a way for the probe to sense one or more properties of the surface; a way to move the probe around the surface in a controlled way; and a way for the probe to tell the microscope where it is and output a map of the surface. Of these, only the first significantly varies from one type of SPM to another – motion and imaging are fairly standard between types. In the AFM, the probe feeds back not on tunneling electrons, but on the strength of the interatomic forces between the probe and surface. These can be *repulsive* forces if the probe is in contact with the surface; or they can be *attractive* forces such as electrostatic or van der Waals forces that cause the probe to be pulled toward, or even stick to, the surface. To sense these forces, the AFM uses a small, flexible cantilever – a thin strip of silicon nitride or (in the early days) aluminum foil that bends in proportion to the strength of the interatomic forces. Usually, there is a small pyramid of material at the end of the cantilever that provides most of the mass that interacts with the sample; AFMers try to make this tip as sharp as possible, but even under the best circumstances the strength of the interactions between tip and sample are more blurry and less distance-dependent than in an STM. Thus, *in general*, AFM has a lower resolution than STM; the only exceptions are high-end AFMs, custom-built by a handful of academic researchers. Some of *these* instruments can actually make out *subatomic* features such as dangling s-orbitals jutting out of a silicon surface (Giessibl, et al. 2001).

Most other members of the probe microscope family build out from the STM or the AFM, whether by using a cantilever made from materials that sense particular properties (as in magnetic force microscopy) or by tweaking the movement of the cantilever or reading different kinds of information out of its motion (as in friction force microscopy) or by using an AFM or STM as the feedback mechanism to keep the probe close to the surface (as in near-field scanning optical microscopy). Some of these variants (such as magnetic force microscopy) have their own dedicated communities and markets, some (such as friction force microscopy) are just operating modes ("bells and whistles") included with a standard microscope, and others (such as near-field scanning optical microscopy) are still only associated with a handful of people doing basic research. The diversity of designs and users in probe microscopy makes for an extraordinarily complex (and colorful) social organization.

Outline of Chapters and Themes

In detailing the co-construction of this community with the development of instruments, designs, and practices, we will follow the relevant actors through a range of settings (primarily in Europe and North America) over four decades. Along the way, we will see three major themes play out. First, the invention, replication, and routinization of probe microscopy took place in an unusual historical moment in which large, regulated-monopoly capitalism, typical of the early to mid-twentieth century, slowly gave way to a more segmented, deregulated, globalized economy (Chandler 1977; Castells 2000). The practice of science and engineering were deeply implicated in this transformation, in both academic and industrial settings; the story of

STM and AFM provides an unusual lens on these shifts. The actors in this story faced many challenges in adapting themselves and their microscopes to the shifting demands and audiences of corporate and academic research.

Secondly, there is a long tradition of seeing academic research as "pure" or free from aberrations brought on by the commercial imperatives of corporate science. Deconstructing this conception has been one of the major achievements of the science and technology studies tradition. Recent works have taken the opposite tack, and described corporate research as more intellectually free and less disciplinarily hidebound than academic science (Rabinow 1996; 1999). I have tried, however, to remain agnostic about whether and where truly "free" research can be done. Instead, I take disciplined and undisciplined practices (and representations thereof) as their own research site. Some STM and AFM work, in both corporate and academic settings, was chaotic, personal, and loosely structured. Other work was seen as contributing positively to disciplined bodies of knowledge, and was constructed to accord with stricter communal standards. Crucially, *switching* between these registers at key moments was an important mechanism for diffusing probe microscopy.

Finally, we will explore one other set of registers relevant to the development of probe microscopy: those of "science" and "technology." In their earliest days, the STM and its predecessor, the Topografiner, were envisioned as industrial surface characterization tools; they were seen as technologies necessary in the manufacture of goods like ball bearings and integrated circuits. Almost immediately, though, tunneling microscopy migrated into fields whose members pictured what they were doing as "basic research." In Chapter Two we look at the invention of the Topografiner and its relationship to the new discipline of surface science at the National Bureau of Standards in the late 1960s. In Chapter Three, we shift to IBM, where the inventors of the STM forged a local experimental practice that they represented as "relaxed" and undisciplined; at the same time, they saw the need to draw on the practices, knowledge, equipment, and audiences supplied by more disciplined communities in order to popularize their microscope. In looking for the right community to foster the STM, they eventually forged more lasting ties with surface science.

Chapters Four and Five examine the first replicators of the STM in North America; Chapter Four looks at young postdocs and junior staff scientists at IBM and Bell Labs trying to make the STM part of surface science, while Chapter Five tells the story of academic microscope builders in California who took the STM as the starting point for the development of a whole family of instruments. By 1991-2, the efforts of both corporate surface scientists and academic instrument-builders had expanded the probe microscopy community dramatically, resulting in the frictions and controversies that are detailed in Chapter Six. In the corporate labs, STMers had to make their work interesting both as basic research, relevant to the wider surface science discipline, and as potentially relevant, on a long time scale, to the technological interests of companies like IBM and Bell Labs. As these institutions fell on hard times in the early '90s, these STMers found they needed to radically shorten the horizon of that relevance, and recast what they were doing as more overtly "technological."

Academic STMers and AFMers, meanwhile, always tacked skillfully between repertoires of "science" and "technology" – any piece of work could be represented one minute as "scientific research" and the next as "technology development." In particular, when their scientific claims were occasionally questioned, these early academic probe microscopists could fall back on the position that they were innovating the technology and inspiring further work by more disciplined practitioners. This way of doing things worked extraordinarily well through the 1980s, but with the disputes of Chapter Six, and the commercialization of the microscopes detailed in Chapter Seven, this style became more difficult to sustain. Yet even after the commercialization of the instruments, probe microscopists still needed to switch fluidly between repertoires of “science” and “technology.” Chapter Seven details the many different ways various microscope manufacturers dealt with this dilemma; while Chapter Eight examines the changes in role experienced by probe microscopists after commercialization, and the ways they have adapted the registers of “science” and “technology” for the post-commercial world. Finally, I end with an epilogue on the relevance of STM and AFM to nanotechnology (and vice versa). Probe microscopy has been crucial symbolically for the emergence of nanotechnology; and nano is a peculiar proto-discipline in that it fuses the scientific and technological repertoires to an unusual degree. Nano is all about making *things*, but also about generating a new kind of knowledge to surround those things. Given these qualities, and their own post-commercial role dilemmas, probe microscopists are beginning to make nano their home, though not without frictions. Hence, as we will see in the epilogue, we can use the emergence of nanotechnology as a way to once again highlight and understand many of the processes of social organization at work in this complex community.

Chapter Two

Surface Science and the Topografiner

Today, the scanning tunneling microscope and the atomic force microscope are the multimillion dollar darlings of the nanotechnology boom. The glamorous STM is central to basic nano-oriented surface physics and provides the spectacular images at the imaginative core of nanotechnology; while the workaday AFM is in a variety of academic science and engineering laboratories, in industrial reliability and quality control labs, on the process line in semiconductor fabs, and even on the surface of Mars.[1] In a field fascinated by instrumentation, no instruments have been more symbolically crucial than these scanning probe microscopes.

Slowly, scholars in science and technology studies are realizing the importance of these instruments in the constitution of nanotechnology. Yet the *spectacle* of probe microscopy, especially STM, often diverts scholarly attention to a narrow range of topics. In particular, the STM's ability to "see" and manipulate *atoms* makes it a golden goose for philosophers and historians of science.[2] The atom's long journey from Democritus to Dalton to Bohr is a central story in histories of physics and chemistry; and its transformation from a fictive heuristic of *fin de siecle* positivism to an undisputedly "real" particle signposts the past century's upheavals in philosophy of science.[3] The story of microscope development, too, is often taken as a paradigmatic

[1] For an explication of the centrality of the STM to the mythology of nanotech, see Baird and Shew forthcoming.
[2] I am thinking in particular of Buchwald (2000); Barad (1999); Hacking (1992). The latter has been enormously influential on this dissertation. All three of these pieces, though, treat the STM only in passing, taking its lessons for science studies as self-evident and as deriving primarily from the ability to see atoms.
[3] See, for example, Pullman (1998); McDonnell (1991); Heilbron (1981).

narrative of science's inexorable approach to the minute fabric of reality.[4] So the STM, in seeing atoms, lies at a tempting intersection for philosophers and historians.

Seeing single atoms *is* fascinating, and that fascination did stoke the rapid growth of STM in the 1980s. Yet to concentrate on the atom or the STM or even imaging is to miss the complex array of practices surrounding probe microscopy. This dissertation will present STM and AFM as multi-dimensional artifacts whose intricate story is bound to a diverse cast of people, institutions, and communities. Imaging (including imaging atoms) is part of this story, but so are practices of tinkering, playing, presenting, marketing, and teaching. Above all, we will examine how STM and AFM are not simply means for seeing the very small, but also foci for training researchers, establishing enterprises, and growing communities.

The rest of this dissertation details how the STM and AFM achieved such success. This chapter, though, will examine an alternative reality in which imaging atoms aroused little interest and the STM's forerunner – the Topografiner – died an unnoticed death. This is partly an historical curiosity, but it also elucidates the institutional and disciplinary transformations that fostered STM in the early '80s. The Topografiner did not evolve directly into the STM, yet the context in which the Topografiner withered did evolve directly into the environment that nurtured the STM. To understand where the STM came from, and why its success was context-dependent, rather than an inevitable outcome of seeing atoms, we need to examine the milieu and life story of its predecessor.

Electron Physics and Field Ion Microscopy

As far as is known, the first microscopic images resolving single atoms both vertically and laterally were produced in Erwin Mueller's physics group at Penn State

[4] See Wilson (1995); Ruestow (1996); Rasmussen (1997).

in 1956 (Mueller 1956).[5] Despite this claim to fame, the instrument that achieved atomic resolution, the field-ion microscope (FIM), is virtually unknown to historians and philosophers of science. Its compelling images did not capture the scientific or public imaginations in the way the STM has, nor has FIM become central to any "big science" like nanotechnology. Yet Mueller and the FIM community did indirectly influence the birth of STM. Though the two instruments' imaging mechanisms are quite different, the physics behind them is related; also some FIMmers contributed to the invention and development of STM, and the early STMers saw FIM researchers as a crucial resource and an analogous community.

Mueller came to Penn State from Germany, where he studied under Gustav Hertz at the Technical University in Berlin, specializing in electron physics.[6] This subfield descended from studies of cathode tubes by J.J. Thomson and others leading to the discovery of the electron at the turn of the century; by the time of Mueller's training in the '30s, though, electron physics also took its inspiration from solid state physics, metallurgy, and crystallography.[7] Electron physicists were interested not just in the electron *per se*, but in using its properties to characterize crystals. For instance, before Penn State, Mueller pioneered the field emission microscope. In FEM, a high voltage is placed on a sharp metal (usually tungsten) emitter, causing electrons to tunnel out of the emitter into the surrounding vacuum (a process known as field emission). A phosphorescent screen is placed near the emitter so that field-emitted electrons light up patches on the screen. Different parts of the emitter – corresponding

[5] The resolution of a microscope is a much-contested term of art. For the purposes of this dissertation, the handy definition of "atomic resolution" is the ability of a microscope to distinguish two adjacent atoms as distinct entities, rather than present them visually as a continuum. It is important to distinguish lateral and vertical resolution. Under certain conditions, an optical microscope can image single atomic steps on a crystal (vertically resolution), but cannot distinguish two atoms in the same monolayer (lateral resolution). Thus, lateral resolution is generally a much more difficult proposition.
[6] For biographical details about Mueller and the FIM and FEM, I draw on Melmed (1996) and Melmed (2003).
[7] See the pieces in Buchwald and Warwick (2001).

to, for instance, different grains in the tungsten – emit more or fewer electrons. This yields an indirect, lensless image of the emitter on the phosphorescent screen. Thus, through FEM, electron physics can be used to answer crystallographic questions about the size of grains or the diffusion of adsorbates.

One canonical figure of this kind of electron physics was the American chemist Irving Langmuir. Langmuir's style of mixing basic and applied research set the tone for the subdiscipline, particularly in North American corporate and national laboratories (Reich 1983; 1985). Leonard Reich shows that Langmuir carved a space within General Electric for doing basic research that won him the Nobel Prize; yet Langmuir's "basic" work took its *inspiration* from the technologically oriented artifacts and materials with which he was familiar from his commercial research. Even when the most esoteric questions were at stake, the materials and artifacts used to answer those questions gestured toward or mimicked commercial technologies like light bulbs and vacuum tubes. By the '50s, this entanglement of commercial, technological means with basic electron physics problematics could be seen in, for example, the research leading to the transistor at Bell Labs (Riordan and Hoddeson 1997) or the mutual development of electron microscopy and television at General Electric (Kunkle 1995; Strick 1998).

Mueller's lab at Penn State focused on using the classical geometries of electron physics to develop instrumentation to probe the microscopic properties of metals. The central character of this chapter is not Mueller, however, but one of his graduate students, Russell Young.[8] Young arrived in Mueller's lab in 1953 on the heels of Mueller's invention in 1951 of the field *ion* microscope (for a diagram of the FEM and FIM apparatus, and a comparison of their images, see Figures 2-1 and 2-2).

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[8] Young's colleagues at NIST have been assiduous in writing about the history of his work. This chapter draws especially on Villarrubia (2001).

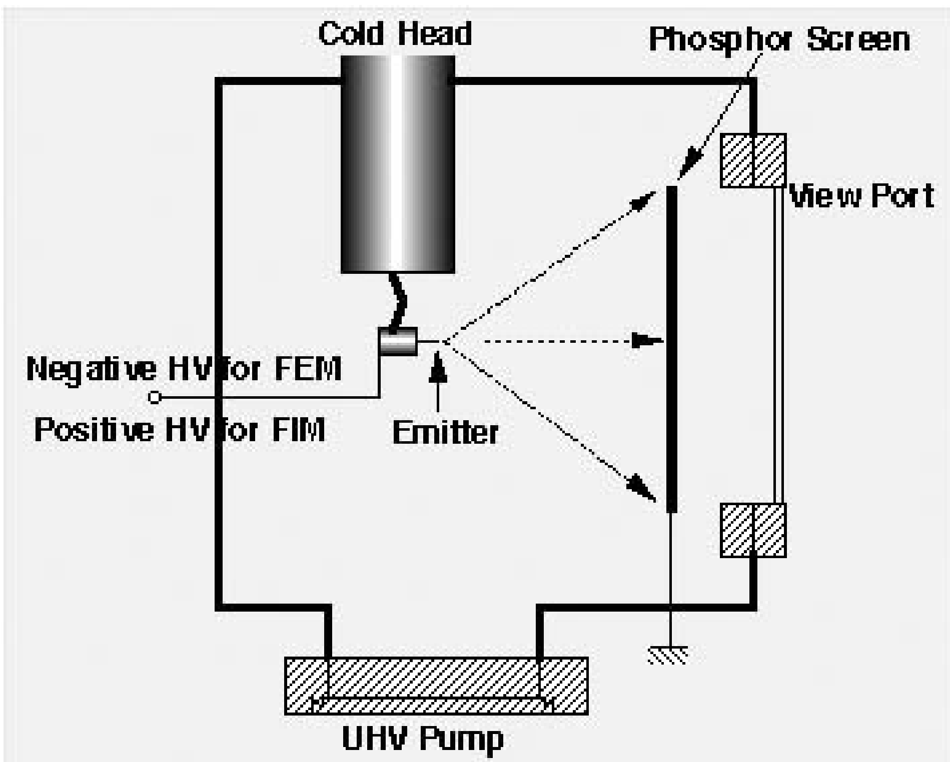

Figure 2-1: Field emission and field ion microscopy. A schematic of the field emission microscope (FEM) and the field ion microscope (FIM), both invented by Erwin Mueller. A sharp metal emitting filament is kept at low temperature, in a vacuum, with a high voltage placed on it. This causes electrons or ions (depending on the sign of the voltage) to fly away from the emitter toward a phosphor screen, where they light up bright patches. These patches correspond to points of extremely high voltage on the emitter (i.e. corners, atoms, grain boundaries, etc.). From www.nims.go.jp/apfim/FEM.html

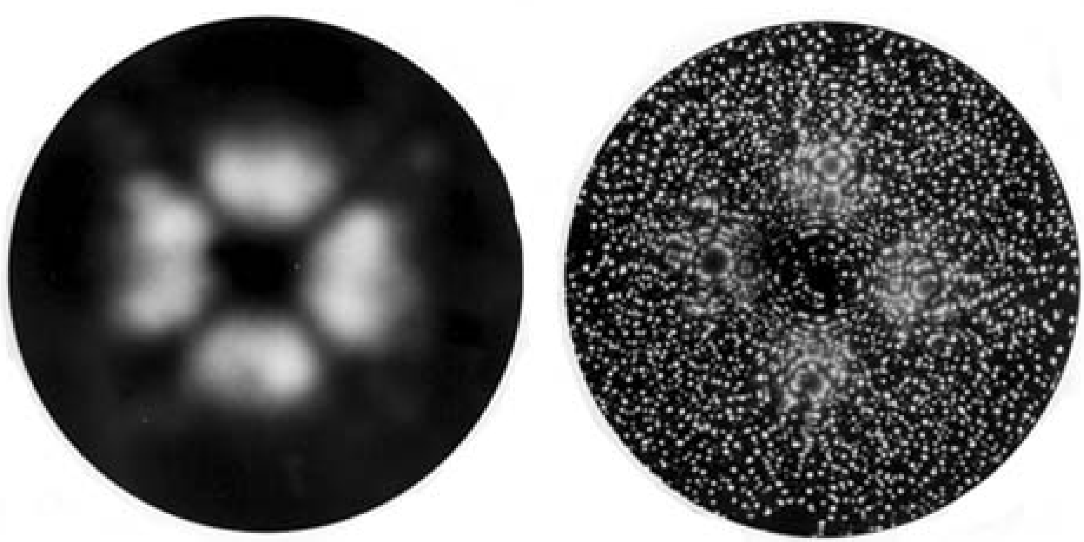

Figure 2-2: FEM and FIM images. A field emission microscope image (left) and a field ion microscope image (right) of the same sharp nickel tip. The point of the tip is at the center of each image. The bright patches in the FEM image are the grain at the end of the tip. The small bright dots in the FIM image correspond to individual nickel atoms. From www.nims.go.jp/apfim/FEM.html.

Like FEM, FIM consists of a sharp emitter, high voltage, and phosphorescent screen in a vacuum chamber. The major difference is that in the FIM, the voltage on the emitter is positive, so ions rather than electrons provide the imaging mechanism – as the ions left in the vacuum chamber approach the emitter they accelerate away from it toward the phosphorescent screen.

This provides higher resolution than FEM, because the wavelength of the ions is smaller than the electrons. Indeed, even with the first FIM experiments the instruments achieved a limited ability to "see" single atoms. The secret to atomic resolution lay in the non-continuous nature of the emitter. At sharp corners, edges, and points, the electric field at the surface of the emitter is much higher than the rest of the material, meaning ions will preferentially accelerate away from those areas. *Within* a corner, edge, or point, the electric field will be even higher around individual atoms; ions accelerating away from the emitter will be so narrowly confined that they when they hit the screen, the points of highest electric field (atoms at a corner or edge) can be picked out. Thus, as can be seen in Figure 2-2, individual atoms at the apex of the emitter show up on the phosphorescent screen as bright spots.

Atomic resolution would seem to be a holy grail of microscopy. Popular histories of microscopy present its development as progressively leading to higher resolutions. Yet, pragmatically, resolution is rarely the only, or even primary, factor for a user – contrast, perspective, ease of use, the presence and type of "artifacts" (distortions or spurious phenomena in the image), sample damage, and other considerations can all be more important than resolution. Most importantly, a microscope is only relevant to some scientific subculture if it can be made to tell something about the "epistemic things" (Rheinberger 1997) of that subculture. If features on the materials of a technical subculture are too large to be elucidated by a new microscope, then the microscope will face many challenges being adopted by that subculture. Even if the new microscope is adopted, lower resolution microscopes can still generate knowledge about the subculture's epistemic entities. Thus, optical microscopes continued to be used in many disciplines even after the arrival of electron microscopy, FIM, FEM, STM, and AFM, and instrument development is as intense as ever for certain kinds of optical microscopes.

Also, when new microscopes are first developed, they often have a lower resolution than instruments already in use. Microscope builders dedicate themselves to the proposition that, whatever its current defects, their instrument's capabilities will improve over time. For instance, the first transmission electron microscopes developed by Ernst Ruska and others were bulky and temperamental, destroyed samples, and yielded blurry low-resolution images; yet TEM's inventors pressed on, eventually developing instruments that outstripped optical microscopy in terms of resolution. This is a common theme in history of technology. To take examples from aviation, Ed Constant shows that early jet engines were, in most ways, technically inferior to propellers, yet jet proponents fostered a paradigm shift in which disadvantages of propellers counted against that technology, but drawbacks of jets were seen only as growing pains of a new technique (Constant 1980). Similarly, Eric Schatzberg shows that an "ideology of progress" surrounded metal airplanes; thus, resources were provided for developing all-metal planes, while they were diverted from improving knowledge about wood construction, leading to a retrospective evaluation of metal as the better material (Schatzberg 1994; Mody 2000).

Likewise, during development microscope builders can find themselves in an "iron cage of resolution," where resolution is the most important rationalized, means-ends measure of progress.[9] In looking for potential users, though, builders must attach

[9] Weber's ideas about rationalization and routinization (1992) are a largely implicit but central ingredient of this dissertation's argument.

high resolution to the needs of other technical subcultures. In this, FIM met limited success. Mueller did grow a community of FIMmers, largely by graduating students who founded their own FIM groups. Yet, partly because Mueller was a strong gatekeeper of this community, few users from other communities became FIMmers [CD1, 10/30/03]. Also, Mueller's electron physics community was evolving at this time. As we will see, FIMmers eventually formed their own insular subdiscipline well apart from the surface science community that succeeded electron physics.

Young and the National Bureau of Standards

While at Penn State, Russ Young became familiar with FIM and FEM, but his own thesis work did not deal directly with microscopy. Instead, he measured the energy distributions of electrons emitted from surfaces to much greater accuracy than had been done before – work that both illuminated the properties of field-emitted electrons and could be used to probe the characteristics of metal surfaces (Young 1959). In 1961, Young left Penn State for the National Bureau of Standards in Washington, DC. The Bureau, founded in 1900, was the United States' metrology institute, the equivalent of the German Physikalisch-Technische Bundesanstalt or the French Bureau des Poids et Mésures.[10] Its mandate was to keep and calibrate national standards of mass, length, time, etc.; to measure more accurately fundamental physical constants; and to develop codes for products such as fire retardant building materials. Also, the Bureau supplemented this mandate with work typical of any national laboratory – i.e., a mixture of basic and applied research oriented to national needs.

The Bureau was heavily involved in military applications in World War Two, and afterwards it benefited from the dramatic expansion of the physical sciences that swept American universities and national and corporate labs. In the 1950s, a large

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[10] Some institutional histories of NBS/NIST are useful: Cochrane (1966); Passaglia (1999); Schooley (2000).

fraction of the Bureau's people and resources were dedicated to basic research not obviously related to standards or measuring fundamental constants. This was especially true in the Atomic Physics Division where Young worked. Young entered the APD's Electron Optics group, led by Ladislaus Marton and noted both for its prestige in electron microscopy and for its cultural isolation from the rest of the Bureau and the Bureau's customers. From the start, Young worked at the periphery of Marton's fiefdom, since his research was only tangentially related to the group's main focus, improving the resolution of electron microscopy of biological samples [RY1, 6/29/00; BG1, 6/11/02].

So Young carved his own niche, in which he returned to the intersection of electron physics and the solid state. This meant measuring the work functions of metals like tungsten to new levels of accuracy (Young and Clark 1966). The "work function" is a solid-state term referring to the energy needed to move one electron from the Fermi level of a material an infinite distance from the material. The Fermi level is the highest occupied band of electrons in the material. Basically, then, the work function is a measure of the ease with which electrons can be stripped out of a material – think, for example, about how easy it is to use a balloon to strip electrons out of your hair and make it stand on end. As a characteristic constant, the work function is useful in understanding the electrical and optical properties of a material. Though this work rarely connected with the rest of the Electron Optics group, it began gaining recognition from outside researchers, which allowed Young to help recruit a cadre of fresh Ph.D.s similarly interested in the intersection of electron physics and material properties –Bill Gadzuk, Cecil and Ward Plummer, Ted Madey, and others [BG1, 6/11/02].

Surface Science

As they arrived, these people spread out across the full range of the Bureau's divisions. Their common research interests, though, sparked decades-long formal and informal collaborations. The mid 1960s were an exciting time to be doing this work. In forming a local community organized around their topic, they both contributed to and exemplified a larger trend in which traditional electron physics morphed into a new subdiscipline known as surface science. Various technical traditions have, of course, been concerned with surfaces for centuries: almost all chemical reactions happen at a surface or interface, most mechanical engineering is affected by friction and adhesion between the surfaces of moving parts, optical components gain many of their characteristics from their surfaces, rust and corrosion begin at surfaces, etc.

Yet the study of surfaces, as a scientific rather than a craft community, was marginal before the 1960s. Notably, the new surface science adopted many of the same forebears as the electron physics community had. For instance, Irving Langmuir's Nobel Prize-winning work came to be seen as the study of electron emission *from surfaces*, and his work on Langmuir-Blodgett films came to be seen as the study of self-assembled monolayers *on surfaces*. Similarly, Clinton Davisson and Lester Germer, Bell Labs researchers who won the Nobel Prize for studies of electron diffraction, now came to be seen as the pioneers of *low energy* electron diffraction, a technique in which electrons lightly graze a material and interact only *with the surface* (Gehrenbeck 1978). Also, surface science embraced the same institutions and ethic as electron physics – in North America, the forefront of surface science was located in large corporate and national laboratories and focused on materials and systems which bore a resemblance to commercially relevant artifacts [JM2, 7/6/00; JM2, 7/8/02; CD1, 10/30/03].

Even with such institutional support, surface science in the early ’60s was a marginal activity. Most physicists and chemists viewed surfaces as a chaotic, unintelligible, disordered mass, in pointed contrast to their view of bulk crystals as ordered, pure, and hence amenable to study. In general, surfaces and interfaces present boundary conditions that defy the assumptions needed to make solid-state theory workable. Moreover, surfaces are more prone to contaminants, impurities, and “dirt” than bulk solids. These difficulties with surfaces, and the social marginalization that attended them, provoked a creative double bind (Bateson 1956; 1962) for early surface scientists at large corporate and national institutions like Bell Labs, IBM, GE, and the Bureau of Standards. On the one hand, these organizations had enormous financial stakes in technological problems to which surface science could address itself. By the 1960s, for instance, both AT&T and IBM had pinned their futures to semiconductor chip technology for communications relaying and mainframe computing (Morris 1990, 86). Even from the time of the invention of the point-contact transistor at Bell Labs in 1947 (often taken as the event and the institution that founded surface physics), it was clear that surface phenomena were key to making semiconductor devices that could work reliably in electronic signal processing and logic circuits (Riordan and Hoddeson 1997). Transistors are essentially signal amplifiers; a very small change in voltage or current at one junction creates very large changes in the voltages at the other junctions. The amplifying characteristics of a transistor are best utilized (in, for example, repeating a long-distance connection so that there is no loss of signal) if it can work with very small input voltages or currents. The pioneers of semiconductor electronics saw, though, that this required using a very small volume of semiconductor crystal. This could best be achieved (as in early junction transistors) by building the semiconducting part of the transistor out of an extremely thin crystal, where the relevant physics are those of a *surface* (with very different assumptions made about boundary conditions and path lengths) rather than a *bulk* crystal.

Also, transistors depend on bringing together two different types of semiconductors – a p-type (which donates "holes" or positive carriers) and an n-type (which donates electrons or negative carriers to the flow of current). Usually these are crystals made from the same material (e.g. silicon), but "doped" with different trace amounts of other materials like phosphorus. To create a current with the proper characteristics, the p and n regions must be close enough that carriers with very short penetration depths can move from one region to the other. This means there must be an *interface* between them – a region where, again, the physical properties of the material must be approximated with very different boundary conditions than a bulk crystal. Since carrier charges often congregate near surfaces, and since a *surface* offers much more tractable approximations in modeling than an *interface*, surface scientists moved in the '60s to incorporate the study of interfacial layers in their work.

Finally, after the invention of the integrated circuit in 1959, surface science began to take on new importance. The first integrated circuits were planar transistors – devices made by growing very thin successive layers of semiconductor, insulator (usually the semiconductor's native oxide), and metal interconnects in intricate patterns (Zygmont 2003). It quickly became clear that improving the reliability of such devices depended on surface physics and chemistry. Surface physicsists began to study the myriad crystalline defects (dislocations, twins, pinholes, and other disruptions to an orderly, smooth crystalline lattice) that could impair the flow of carriers. Surface chemists began to study ways of more reliably growing such layers on top of each other and of "capping" or "passivating" surfaces to keep them clean, so that contamination would not cause the characteristics of devices to vary too widely.

The double bind arose because, in making their expertise relevant to these technologically important materials, surface scientists saw a need, and an opportunity, to professionalize their community and develop a disciplined body of practices and knowledge. Yet, while professionalization accorded well with the entry of surface scientists into place like Bell Labs and the Bureau of Standards, it also made demands that cut against the technological imperatives of those institutions. In particular, the contamination and disorder to which surfaces were prone made building a positive, disciplined body of knowledge difficult – experimenters struggled for years in the '40s and '50s to make surfaces clean enough that results would be reproducible [JM2, 7/6/00]. This drove surface scientists to study only material systems that could be kept ultraclean and hence would yield more reliable experiments and mesh better with theoretical approximations. The problem was, such clean systems diverged greatly from the messy, real-world devices surface scientists' employers had staked their businesses on.

Institutionally, therefore, surface scientists had to find ways to combine corporate citizenship with professionalization. One way was to follow Langmuir and alternate basic research with work on technological artifacts. Surface scientists could contribute to engineering projects that often involved processing the same materials that they researched part-time in the new surface science idiom; their basic research might not directly forward their engineering work, but it generated prestige for their employers, it enriched their intuitive understandings of how to handle and prepare technologically relevant materials, and, occasionally, it opened serendipities that had immediate applications. Also, surface science became a training ground for young researchers; the rise of surface science coincided with the postwar expansion of the role of the postdoctoral position in American science, and many postdocs passed through basic surface science groups at corporate labs on their way to joining engineering groups within those corporations. One prominent surface scientist sums up all these ways of contributing:

> CD: The science of semiconductor surfaces has to best of my knowledge had zero impact on the microelectronics industry.... The semiconductor industry's process steps were empirically determined....
> CM: What did a company like Bell Labs or IBM see itself getting out of semiconductor surface science?
> CD: Training people.... Young Ph.D.s from a university would come and work with your basic science group and they'd work there for maybe two or three years and it would become clear that they needed to move on.... It was basically an elaborate recruiting and socialization scheme.... The people who ran these groups were people who made their living from delivering technology to the company. For them, science is an avocation. As an industrial scientist, I did my basic science on the side. I delivered technology options for my job, and the company was delighted to let me have a couple postdocs and to do my science on the side because they could hire the postdocs. My postdocs populate this place [Xerox Research].... Doing science was a hobby. Delivering the next generation of some kind of technology was your job. Students come into that world and they .... can see that if they want to stay around they're probably going to do one of these more applied things. [CD1, 10/30/03]

As we will see, surface scientists' oscillation between basic research and more technologically-relevant work, as well as their use of basic research to train protégés, profoundly shaped Russell Young's work at the Bureau and, later, the adoption of the STM at North American corporate and national labs.

Experimentally, surface scientists solved their double bind by building their expertise around systems that *resembled* technological artifacts, but which had been transformed in various ways to make them more amenable to disciplined research. This is an interesting wrinkle on old debates about the relationship between "basic" and "applied" science (Layton 1971; Kline 1995; Leslie 1993, 61ff.). In many ways, basic surface science research followed *after*, and was inspired *by*, engineering work; yet it rarely contributed directly to new products or applications with anything shorter than a several decade horizon. Instead, surface science contributed indirectly to applied work, through the generation of cultural capital, through pedagogy, and through the maintenance of a surface community that included corporate, national, and academic scientists and engineers, funding agencies like the Office of Naval Research and the National Science Foundation, and instrument and/or equipment manufacturers.

Interestingly, surface scientists made their preoccupation with cleanliness, and the production of more theoretically tractable materials, central to the growth of this community. Three contaminants were particularly relevant – impurities in the bulk that migrate to the surface; contaminating particles in the atmosphere that adsorb to the surface; and a ubiquitous layer of moisture covering almost every surface exposed to air. The consciousness of these contaminants affected the organization of surface science institutionally, communally, and practically. Practically, it led surface scientists to use vacuum chambers to eliminate contamination (Duke 1984). Vacuum technology had been integral to electron physics, since light bulbs and vacuum tubes depend on a low vacuum for their properties. In surface science, though, vacuum became a fetish. As improvements in solid-state theory made surface calculations more tractable, vacuum technology became the means to produce "well-defined" surfaces that meshed with theory more adequately. Vacuum made theory easier to produce, and it allowed experimentalists finer control over materials so they resembled the entities of theory.

Thus, communally, vacuum became a shibboleth. For theorists, vacuum defined the limits of discourse; vacuum at the surface was an axiomatic approximation. For experimentalists, building and operating vacuum technology became a mandatory part of graduate education. This meant familiarity with vacuum chambers, manipulators, pumps, and airlocks, an understanding of how to accommodate instrumentation to the vacuum environment, and how to prepare ultraclean, "well-defined" materials. Vacuum is an extremely demanding environment – materials behave in different ways, experimenters have limited bodily access to equipment, and the time to bring materials into and out of a good vacuum significantly slows the pace of experimentation. Surface scientists made possession of arcane, disciplined knowledge about vacuum and contamination a central part of the construction of their field.[11] Aspersions of contamination or unfamiliarity with specimen preparation and vacuum techniques became easy repertoires for policing the internal structure and external boundaries of their community.

This fascination with vacuum also shaped surface scientists' search for an institutional home. For those who came to surface science from electron physics or solid-state research, a natural move was to create a surface science section within the American Physical Society. The APS, however, still saw surface science as a marginal, technology-oriented area and insisted that surface scientists remain within the existing solid-state section [JM2, 7/6/00; CD1, 10/30/03]. Many surface scientists, though, were already members of the American Vacuum Society, and in the '60s these researchers swelled the ranks of the AVS, eventually taking over its leadership positions and much of its identity and making it the professional society of surface science (Schleuning 1973).

For the AVS, this influx brought rewards and drawbacks. The Society started as a small craft association, the Committee on Vacuum Technique, in 1953. Its members were engineers, technicians, and inventors from companies that made vacuum equipment or products with evacuated components. The CVT grew rapidly, and by 1957 it changed its name to the American Vacuum Society, with founding members from science and engineering, universities and national labs, medium-sized firms and giants like General Electric. This was an exciting time for vacuum. Just as higher resolution is a rationalized measure of progress in microscopy, lower pressures

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[11] See Douglas (19660 and for the classic analysis of contamination and social structure. See Mody (2001) for an extension of Douglas' work to lab culture.

can be a yardstick for vacuum technology – and the late ’50s saw the advent of *ultrahigh vacuum* or UHV.

The resolution of three reverse salients (Hughes 1987) crystallized the UHV revolution (Steinherz and Redhead 1962). First, new diffusion pumps allowed reliable generation of lower, cleaner pressures. Second, the vacuum community moved away from traditional glass bell jars to metal flanges and chambers, encouraging a wider range of auxiliary equipment (airlocks, manipulators, pressure gages, specimen preparation and characterization tools) within or at the boundary of the chamber. Curiously, the most vital contribution was the invention of better gages for measuring ultralow pressures. As early as the ’30s practitioners were achieving UHV conditions, but without a gage to measure those pressures that ability was useless. For surface scientists, gages were key both to obtaining UHV, and to knowing how long a specimen in UHV would stay “well-defined”; the higher the remaining pressure in a chamber, the greater the number of contaminant atoms that, over time, would impinge on a surface, stick to it, and make the surface less and less resemble its theoretical alter ego and make experimental results more difficult to reproduce reliably.

The ’50s and ’60s also saw improvements in lab technologies that surface scientists wove together into a mutually-constituting “instrumentarium” (Hacking 1992). Above all, practitioners came to better understand and reliably build low energy electron diffraction (LEED) instruments. The realization that LEED provided information about the topmost layers of atoms on a material, and that the structures of those layers differed significantly from the underlying bulk, opened new vistas of investigation. Also, through the ’60s, advances in computing power made it more tractable to provide theoretical analysis of LEED patterns. Thus, institutions with access to high-end computers and an interest in surface science – Bell Labs, IBM, and, to a lesser extent, Xerox and the Bureau of Standards – steered the field [JM2, 7/8/02].

This made LEED indispensable to surface science; as new technologies were developed, such as specimen preparation tools and an alphabet soup of spectroscopies and other analytic techniques, they were laboriously coordinated with LEED – for instance, a new specimen preparation technique could only be *seen* to be affecting the structure of a surface if it produced a change in the LEED pattern. Finally, as one participant has noted, the efficient use of these techniques, and the rapid growth of a surface science community, was aided by the commercial availability for the first time of large single crystals of the materials surface scientists were most interested in (Duke 2003). That is, one of the requirements for being a surface scientist – possession of the community's epistemic things – could now be had via catalog.

Thus, evidences of surface science's professionalization appeared in short order through the '60s. By 1960, the AVS' ranks swelled with enough people whose interests were wider than "vacuum technique" that it started a Vacuum Metallurgy Division, then a Thin Film Division in 1965 and a Surface Science Division in 1968. The creation of divisions was caused by the scientization of the society's membership. By 1970, the old vacuum engineers that founded the Committee on Vacuum Technique had become such a minority in the AVS relative to surface scientists that the Society formed a Vacuum Technology Division – i.e., a *division* with the same mandate as the original society (Schleuning 1973). Many of the surface scientists in this influx, by the late '60s, took leadership positions in the Society. Also, throughout this period the AVS repeatedly tried to become a member society within the American Institute of Physics (the umbrella organization led by the American Physical Society) but was turned away. Only when prominent surface scientists – especially Charles Duke, later president of the AVS – took the lead in the early '70s did the APS finally admit the Vacuum Society [JM2, 7/6/00; CD1, 10/30/03]. One reason the AVS sought membership was to have the AIP publish a journal of vacuum research, another mark of professionalization. By 1964, though, pressure for a journal became so great that the AVS began publishing under its own auspices the *Journal of Vacuum Science and Technology*. An independent journal, *Surface Science*, also started in 1964. In addition, the editor of JVST, Franklin Propst, was the founding vice-chair of the Surface Science Division – i.e., surface science considerations were foremost in editorial policy. Notably, from the beginning there were complaints from the old vacuum technology crowd that JVST catered exclusively to basic surface science research rather than the technical fundamentals of vacuum equipment.

Surface Science and Precision Engineering at the Bureau

In forming a surface science coterie at the Bureau of Standards, Young and his colleagues were in tune with the times. Moreover, their work, particularly in field emission, became central to development of surface science spectroscopies over the next decade. Several Bureau researchers – Young, Ward Plummer, Ted Madey – took leadership positions in the AVS and/or received the AVS' most prestigious career awards. Madey's description of the coalescence of this group nicely captures the era:

> The first people whom we would recognize as UHV surface scientists hired at NBS were all field emission microscopists, and it's logical because in those days field emission was the *only* technique where one could reproducibly and reliably generate clean surfaces. Russ Young was hired in electron physics, Ralph Klein was hired to establish the surface chemistry section, and then Allan Melmed [another Mueller student] was hired to do corrosion research.... Ralph was also responsible for an interesting and important cultural activity at the Bureau of Standards. In the early '60s he had a weekly lunch bunch meet in his office, the "field emission lunch bunch" to talk about exciting developments in field emission microscopy. Well, within a few years as more surface scientists came on board with different interests than field emission microscopy, this evolved into a surface science lunch bunch that was coordinated for many years by Russell Young.... This group is well-known for lots of lively and interesting discussions and being supercritical about issues and scientific topics, and it was occasionally a dismaying experience for visitors to be exposed to this group, but it was really kind of an exciting and exhilarating time. (Madey and Kendall 2001)[12]

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[12]. See also King (1994) for a description of the lunch bunch and surface science at the NBS.

This quote exemplifies the changes I've detailed: surface scientists' obsession with cleanliness, familiarity with UHV as a credential for membership, the early importance of electron physics at the core of the new subdiscipline, and the expansion of the community beyond electron physics in the '60s. One other characteristic emerges in Madey's story – an ethic of stringent, disciplining criticism performed semi-privately in institutional settings, rather than publicly through articles and conferences. In the 1980s, this characteristic of the new surface science would shape the designs of STMs and AFMs and the organization of the probe microscopy community as STM integrated into surface science.

So what did surface scientists at the Bureau work on? At the start, little distinguished this research from traditional electron physics. Even people in the Electron Optics group with little interest in surface science, like Arol Simpson and Chris Kuyatt, contributed studies that helped develop surface spectrometry (Kuyatt and Simpson 1967; Cashion, et al. 1971). With time, though, traditional electron physics faded into the background. The experimental materials and geometries of that field – nickel and tungsten filaments and tips, kept in moderate vacuum – were geared to the instruments, theories, and technological applications of the '30s and '40s. In surface science, semiconductors – especially silicon – became more prominent, particularly at IBM and Bell Labs. Also, as integrated circuits entered production, semiconductor researchers became more interested in the large, flat, clean silicon geometries needed to make ICs. This meshed well with the new emphasis on LEED, which was believed to operate best with a large, flat, clean sample.

Moreover, since LEED gave atomic-scale information, surface science oriented itself more strongly to atomic-scale phenomena. There are some larger-scale things that can be learned from LEED (e.g. growth patterns of adsorbed layers or the abundance of atomic steps), but, in general it offers clues to how electrons are deflected by individual atoms on a surface. When LEED showed that atoms are organized differently at the surface than in the bulk – especially for semiconducting materials – these "surface reconstructions" became the defining problematic of surface science. As new spectroscopies appeared, they were expected to contribute to this problematic by explaining how bonds between atoms in a reconstruction affect the surface's electronic signature. Reconstructions were useful organizationally to surface scientists because they were one of the best indications that surface phenomena were *ordered* (and hence amenable to study that other physicists would see as disciplined – i.e. capable of being represented through elegant equations and models), yet clearly different from bulk properties. Reconstructions were obviously a crystalline feature (because they yielded orderly, reproducible diffraction patterns), so they could be tackled using many of the basic tools and terms of crystallography.

Surface scientists built their paradigm from the basic building block of crystallography – the unit cell. In a perfect bulk crystal, the entire crystal can be represented as an infinite repetition of one cube containing 8, 9, or 14 atoms that is the characteristic "unit cell" of that material. Crystallographers use a three digit vector notation to describe the placement of atoms in the unit cell – (111), for example, is the vector pointing from one corner of the cube to the corner farthest away from it. Surface scientists also use this notation, but to describe the plane within the unit cell along which the crystal terminates to form the surface. For instance, (100) describes a crystal that simply terminates along one of the edges of its unit cell; a (110) surface bisects the bulk unit cell diagonally; and a (111) surface cuts along a plane that diagonally bisects three adjoining faces of the unit cell. Notably, surface scientists concentrated almost entirely on these three kinds of surface cuts – called "low-index" surfaces. Higher index surfaces like a (12-1-1) generally result in rough, complicated surfaces that are hard to produce and difficult to picture theoretically; thus, they have been almost completely neglected in surface science discourse until, very recently, the advent of nanotechnology has made rough surface features more interesting.

A unit cell in a bulk crystal is surrounded on all sides by other unit cells; so the atoms in one unit cell can form energetically favorable bonds with atoms in the adjoining cells. At the surface, though, some atoms will be left without partners. In air, or in a low vacuum, atoms with “dangling bonds” will simply pair up with contaminants, yielding a highly disordered surface. In ultrahigh vacuum, though, certain metals and many semiconductors will actually reorder their surfaces so that the number of dangling bonds will be minimized; this yields an ordered structure that is *different* from that of the bulk material. Since this new surface unit cell sits on top of the bulk crystal, it is usually an integer multiple in size of the bulk unit cell. The size and orientation of this surface unit cell is given by low energy electron diffraction, and provides the basic notation used for describing all surfaces. Take, for instance, Si(100)2x1. This is a (100) cut of silicon, where one surface unit cell forms a rectangle that is two bulk unit cells long and one bulk unit cell wide. Thus, its area is equivalent to that of two bulk unit cells.

In this way, LEED in UHV provided the starting point for surface science discourse. Importantly, though, LEED only describes the outline of the surface unit cell; it tells very little about how atoms are arranged within that cell. Some materials have very simple surface reconstructions that look just like terminated bulk unit cells; other materials, especially semiconductors, have intricate, beautiful surface structures that look very different from the bulk. Figuring out these reconstructions became one of the most dynamic and most organized activities of the new surface science. In the ’60s and ’70s, surface scientists began supplementing LEED with a long series of new spectroscopy tools, leading to an “alphabet soup” of available instruments.[13] Building

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[13] I will describe surface spectroscopy in more detail in Chapter Four.

new kinds of instruments, and bringing them into coordination with LEED and other tools, became one of the central experimental activities of the field.

One hallmark of surface science was the use of canonical "test objects."[14] As the field matured, it segmented into various subfields. In each of these, most work was referenced to one or two material systems that were thought to be key to understanding all the surface systems relevant to that subfield. Semiconductor people used something called the silicon (111) 7x7 (Ridgway and Haneman 1969; Best 1975); for those interested in putting metals on top of semiconductors, the silver-on-silicon (111) √3x√3 was a priority (Barone, et al. 1980; Gaspard, et al. 1980); and for those interested in molecules adsorbed on metals, articles about CO on nickel flooded the journals (Davis, et al. 1980; Sargent, et al. 1980). Again, we see here that these canonical materials were *inspired* by technologically-relevant systems, but that surface scientists transformed them to be more amenable to disciplined study and hence less directly relevant to engineering applications. Carbon monoxide research was easy to sell in the early '70s, given new auto pollution laws, but research on CO on nickel was only indirectly related to understanding CO's behavior in ceramic catalytic converters; metal on silicon research was vital in the era of integrated circuits since metals form interconnects between chip features and are used to protect chips, but silver was rarely used in this way; and silicon itself was the material of the moment, but the (111) 7x7 was a completely different reconstruction and crystalline orientation from the (100) 2x1 usually used in integrated circuit manufacturing. The (111) is easier to make, and the 7x7 was seen as more scientifically interesting (it was the "Rosetta Stone" of reconstructions [CD1, 10/30/03]); while the (100) is more difficult to make, but it is easier to use acids to etch it into technologically-important shapes.

[14] My thanks to Mike Lynch for the term "test object" and discussions on this subject.

Also, the new surface science, with its emphasis on reconstructions and other atomic-scale phenomena, moved away from microscopy. Since no optical or electron microscope (or FEM) of the day could image individual atoms, surface scientists found these instruments difficult to integrate into their new practices. The field ion microscope, with its atomic resolution capability, declined more slowly in popularity. Indeed, when the AVS began offering the Medard Welch career achievement prize in 1970, Mueller was its first recipient. FIM, though, relies on a limited number of sharp geometries and a small number of metals, none of which fit squarely within surface science. Surface science theory in the '60s became geared toward ultraflat geometries, whereas FIM uses sharp spikes and filaments. Under Mueller's gate-keeping, the FIM community became more autonomous and overlapped less and less with surface science. For surface scientists, FIM came to seem extraordinarily limited, while for FIMmers it remained a system with enough unexplored corners to maintain a dynamic research program. This kind of technological incommensurability – where one group sees an instrument as generative for endlessly ongoing body of work, and another group sees it as narrowly focused around uninteresting questions – will reappear again as we follow the STM and AFM.[15]

At the Bureau of Standards, Young began to find his own work diverging from the practical and intellectual core of surface science for complex institutional reasons. Under Marton, the Electron Optics group was repeatedly criticized in external reviews for being cloistered and uncommunicative with researchers both inside and outside the Bureau [BG1, 6/11/02]. After Marton's retirement, his successor, Arol Simpson, refocused the group's electron optics work while pushing Young to help overcome its deficit of contacts with other parts of the Bureau [RY1, 6/29/00]. In particular, he

[15] This type of "interpretive flexibility" is one of the basic observations of the social construction of technology literature. See Bijker and Pinch (1987); Bijker (1995a).

suggested Young make a foray into precision engineering. Like electron optics, this area of metrology was felt in the mid-'60s to be one of the Bureau's weaker areas, and Young and other researchers were imported to bring in new ideas. This was very different work from surface science and electron physics; where surface scientists were interested in the atomic structure of reconstructions or the vibrational energy of bonds in an adsorbed molecule, the Bureau's precision engineers were trying to find ways to standardize and calibrate industrial gears or measure the roughness of paints, varnishes, and surface finishes. The questions, length scales, and materials relevant to these subcultures were, at the time, incommensurable.[16]

Nevertheless, Young tried to think of ways electron physics could aid metrology of industrially-relevant artifacts and surfaces. Calibration of a gage block, gear, ball bearing, or diffraction grating (standard objects of metrological practice) involves having a known reference height and checking the distance between that height and a surface along a line or array of points; the greater the deviation from the mean distance between surface and referent height, the rougher the surface. There were some standard instruments – especially stylus profilometers – that could do this measurement in the '60s (by scraping a spring-loaded stylus – rather like a phonograph needle – across a surface and measuring the deflection of the spring). Notably, in metrology quantitative measurements are paramount, so electron microscopy (even today a largely *qualitative* technique) was marginal to this effort at the time [TV1, 6/29/00]. In analyzing the calibration problem, Young realized that, in theory, more sensitive measurements could be made using concepts from electron physics. Since the Fowler-Nordheim equations governing the field emission of

[16] It might seem odd to speak of length scales as "incommensurable"; after all, lengths should be easily interconvertible. Yet surface scientists were accustomed to thinking of the atomic scale through LEED, which gives images in reciprocal (or frequency) space; the trick of LEED is exactly that frequency space is *not* directly convertible to real space – one must apply an enormous amount of theory, calculation, and guesswork to approximate the conversion.

electrons contain strongly distance dependent terms, they could be used to measure very small changes in distance – surface roughnesses, for example. Young saw that if he used a sharp tip capable of measuring the flow of field-emitted electrons coming out of a surface, then very small changes in the gap between this probe and the surface would result in large changes in the measured current.

This led Young to his "field-emission ultramicrometer" (see Figure 2-3). As an institutional gadfly, Young had few resources to devote to the idea, so he attempted a crude proof of concept to win backing for further work. This consisted of a small, hand-blown, evacuated glass envelope containing a sharp tip (the field emitter) and a metal plate (a stand-in for the "sample" that would be calibrated by a fully-realized micrometer) (Young 1966). Young could put a voltage between the emitter and the "sample" and record the current of field-emitted electrons; but, since the emitter was stationary and the sample sealed in the envelope, this data was relatively useless. Certainly, no "calibration" of height could be done this way. Young realized, though, that he could change the temperature of the system and calculate how the gap between emitter and sample changed with temperature. Thus, he could correlate two *indirect* ways of calculating the distance between tip and sample and show that field emission was a sensitive measure of height.

The Topografiner

In 1966 Young published these results in the *Review of Scientific Instruments* with suggestions for how to use and improve the ultramicrometer. Nobody outside the Bureau followed up on this work, though. Ultramicrometer work became an un-funded sidelight that Young saw as a labor of love but his managers began to see as a distraction. By 1968, he was ready for another try, and approached his managers with a proposal to radically improve on the ultramicrometer [RY1, 6/29/00; BG1, 6/11/02]. Young had realized that he could generate *topographic* images of a sample using field

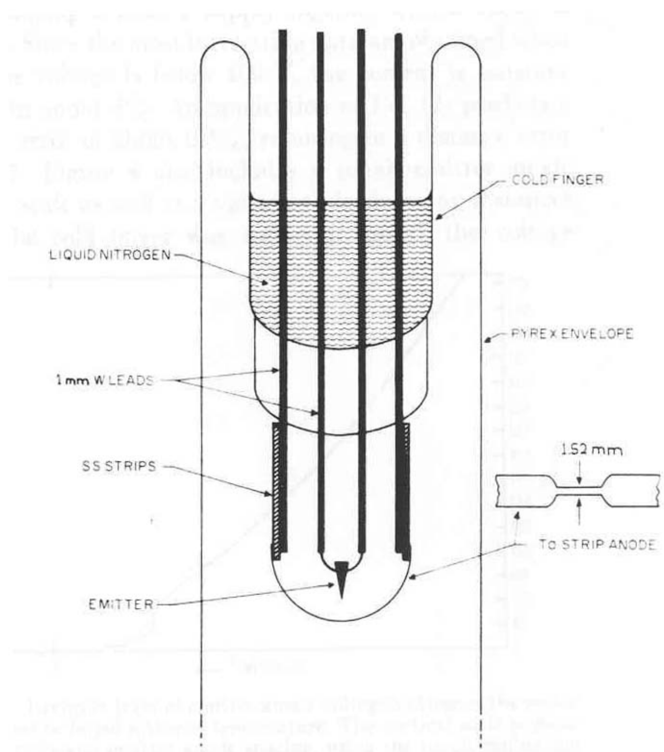

Figure 2-3: Field emission ultramicrometer. The first metrological device designed by Russ Young at the Bureau of Standards that used principles from electron physics. Shown is a sharp metal tip (the "emitter") enclosed in an evacuated glass envelope; the tip field emits electrons that strike a flexible metal strip (a substitute for what would be a gear or ball bearing in a real micrometer). The strength of the current of field emitted electrons striking the "sample" indicates the tip-sample distance. From Young (1966).

emission. If the emitter were moved over the sample in a regular way – say, scanned laterally in the x-direction and moved linearly after each scan in the y-direction in the same way a television or scanning electron microscope builds up an image – then at each point in the scan the emitter could measure the height of the sample. By presenting this data graphically, one could "see" the sample at high resolution. In proposing this design, Young has come retroactively to be seen as formulating all the essential ingredients of a scanning probe microscope: a piezoelectric scanning system, a sharp solid probe, and a means of monitoring, feeding back off of, and displaying the interaction between the probe and a sample (Villarrubia 2001). Young's managers, though, obviously had no way to see his contribution in this perspective. To them, the ultramicrometer mock-up had been an unconvincing demonstration of the value of field-emission height measurements in metrology, and the instrument Young proposed now – which he called the Topografiner (see Figure 2-4) – seemed much more complicated with few advantages in return.

For instance, Young calculated that the Topografiner's resolution was limited by the radius of curvature of its emitter (i.e., how sharp the point of the tip was); this made the average vertical resolution 3 nanometers (best projected resolution 0.3 nm) and average lateral resolution 400 nanometers (best projected resolution 20 nm). That is, the Topografiner's resolution would be little better (if at all) than existing instruments, yet only would have reached that resolution after years of costly development. Young had vague ideas about improving the Topografiner's resolution by operating in the tunneling, rather than field emission, mode, but this seemed even wilder and less certain.[17]

[17] In tunneling mode, the voltage between emitter and sample would be smaller, the tip closer to the sample, and electrons would tunnel directly from the emitter into the sample rather than tunneling from the emitter into vacuum and then ballistically striking the sample. The distance dependence of the tunnel current would be even greater than that for field emission, meaning the microscope would have

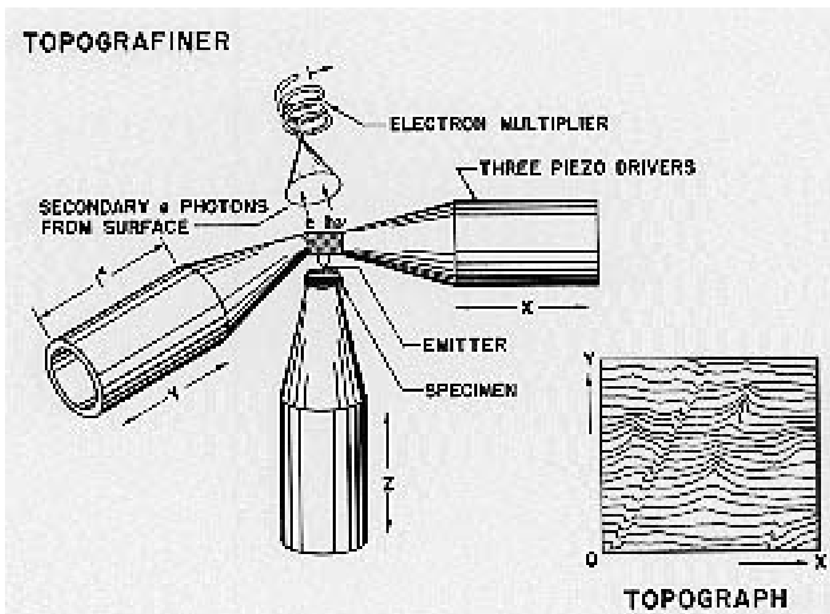

Figure 2-4: The Topografiner. A schematic of the Topografiner showing the three orthogonal piezoelectric scanners, the specimen, the probe (here called the "emitter") and an electron collector for forming an image from secondary electrons. From Young (1971).

Moreover, for Young's managers, the Topografiner seemed divorced from industrial metrology and changes at the Bureau. In the late 1960s, the largesse for fundamental research that floated the Bureau in the immediate postwar period was drying up. The change in presidential administration and looming economic downturn forced Bureau management to push researchers in more industrially relevant directions. Congressional overseers, for instance, began to demand that the Bureau accommodate industry to a much greater extent (Passaglia 1999; Schooley 2000). In particular, an embarrassing backlog of calibrations for industrial customers in 1970-1 became the focus of Congressional pressure. In this context, Young's proposal seemed out of step. After all, the Topografiner could only work in a relatively high vacuum – a major inconvenience for an industrial instrument – and could only operate with clean metal samples (where most industrially relevant surfaces are either non-metallic or have a contamination layer rendering them non-conducting in air).[18] Moreover, Young's talk of operating the instrument in tunneling mode made it sound to his managers more like a chase after one of the holy grails of electron physics than a program to develop a reliable metrology tool. After all, no one had even *seen* vacuum tunneling, much less used it to try to image things like gears and ball bearings. Besides, if the concept of tunneling/field emission metrology were significant, they figured, someone would have picked up on Young's initial ultramicrometer articles.

By going over their heads to an external review board, though, Young won from his managers a grudging two years of funding to prove the Topografiner concept [RY1, 6/29/00; BG1, 6/11/02]. His first priority was to show he could bring a tip into the field emission regime and scan it to produce relatively low-resolution images; at

[18] The Topografiner could also have worked with semiconducting samples, but, crucially, I can find no mention of anything other than metals in any of Young's lab notebooks or published articles. I draw on the "Field Emission Ultramicrometer" and "Topografiner" notebooks from 1965-1970 that Young kindly let me copy.

the same time, he also worked to show that a *stationary* tip could be brought into the *tunneling* regime, the hope being that these efforts could combine to make a scanning tunneling microscope. Initially, Young put his efforts into the scanning field-emission microscope – only as the difficulties building a fully-fledged Topografiner became apparent and his managers' opposition to the project became more pronounced did Young turn to tunneling as a way to salvage results. Today, with the success of the STM, Young credits the Topografiner's demise to the limited and often second-rate resources available to him. It is, indeed, difficult to deny this was a shoestring operation. Characterizations of resource-poverty can play both ways, though. If a phenomenon is "known" to exist, but cannot be produced with a certain instrument, then it is "known" that the group that built the instrument did not have access to the resources they needed; on the other hand, if the instrument does produce the effect, then it is "known" that the group has good "hands" and can overcome material deficiencies with bricolage and ingenuity. The appropriateness of resources available to build an instrument is co-produced with knowledge generated by the instrument.

In the Topografiner's case, the most important reputed shortage was a lack of good vacuum equipment. Young hoped the Topografiner would eventually work in air, since this would make it more palatable for metrology and precision engineering, so whenever he was able to secure a spare technician that person was assigned to experiments in air. As a first try, though, Young believed vacuum was necessary to make the signal clean enough to demonstrate the Topografiner was working. For surface scientists, this was the era of ultrahigh vacuum, an environment that demands complex, dedicated equipment. The Topografiner, though, operated in a less stringent vacuum; still, Young could not buy or borrow a vacuum chamber for the project, and instead, found a used naval gun barrel and converted it into an *ad hoc* chamber [RY1, 6/29/00; BG1, 6/11/02].

From the first, this chamber made worse one of the most demanding problems facing any probe microscope – vibration. When a probe is brought within nanometers of a surface, the most difficult thing is to keep the tip from jostling and "crashing" into the surface. Ambient vibrations couple through the tip-sample system, causing them to move relative to each other. If the average amplitude of that relative movement is greater than the intended separation between tip and sample, then the tip will crash continuously. Vibration isolation is important for many scientific instruments (see Collins (1975) for descriptions of the exquisite care taken to guard gravity wave sensors from stray vibrations), with an array of standard solutions. One expensive option is to buy a specialized, air-supported optical table; a cheaper route is to support the vacuum chamber on a low-tech combination of hard materials that filter out low frequency vibrations (e.g. concrete pillars) and pliable materials that filter out high frequency vibrations (e.g. inner tubes). All that is only a partial solution, though, if air-borne vibrations still impinge on the vacuum chamber, especially if the vacuum chamber amplifies those vibrations like a drum (as, apparently, the gun barrel chamber did). Thus, Young took special precautions to minimize sounds in the laboratory – by, for example, moving himself and his assistants and even the chart recorder used to display the Topografiner's output into an adjoining room.

Other shortages also bedeviled the project. Young was always chronically short of assistance, and he had to rely almost entirely on one technician, Fred Scire, for work that the early STM groups a decade later divided amongst two or three people. Late in the project, Young started taking work home to tinker with in his garage, where he enlisted the assistance of some of his daughters. Because of his shortage of personnel and shoestring budget, Young had to outsource some projects to local artisan; in some cases, such as the construction of the feedback circuit, this compromised the quality of the design.

Audience, Proof, and Afterlife

Though these shortages obviously contributed to the demise of the Topografiner, they were by no means determinative. Instead, to understand the Topografiner's short life and complicated afterlife, we need to see what various audiences made of it. Young had several audiences in mind, as can be read off from his design, use, and explanation of the instrument. For instance, he chose diffraction gratings as his first sample of choice (the clearest Topografiner images he published were of gratings – see Figure 2-5) since he was aiming at the precision engineering community (where gratings were a canonical test of precision) and at his managers (one of whom, Karl Kessler, was a spectroscopist used to dealing with gratings [RY1, 6/29/00; BG1, 6/11/02]). He played to much the same audience in his articles (Young 1971; Young, et al. 1972), where he described possible uses of the Topografiner on engineering surfaces (ball bearings, gears, surface finishes) and metrological materials (e.g. gage blocks – see Young and Scire (1972)), and gave a detailed account of how the Topografiner would complement, and scale down from, existing microscopes.[19]

This last point was also meant to make the Topografiner relevant (or at least palatable) to electron microscopists, including, of course, Young's immediate manager, Simpson. The Topografiner's origins in electron physics, and its intended relevance to electron microscopists, can be read in other details as well, such asYoung's labeling of the probe as the "emitter" and occasional references to the sample as the "anode." Most notably, he positioned an electron-collecting horn next to the sample as an attempt to gather secondary electrons from the probe-sample interaction. In the '60s, secondary electrons were tremendously important to electron

[19] "Traceability" of measurements from one length scale to a smaller one via overlapping instrumental regimes is *the* key concept of length metrology. For the Topografiner to find metrological applications, it had to overlap, and be coordinated with, instruments like profilometers and light and electron microscopes. For an analysis of the concept of traceability, see O'Connell (1993) and Lezaun (2003).

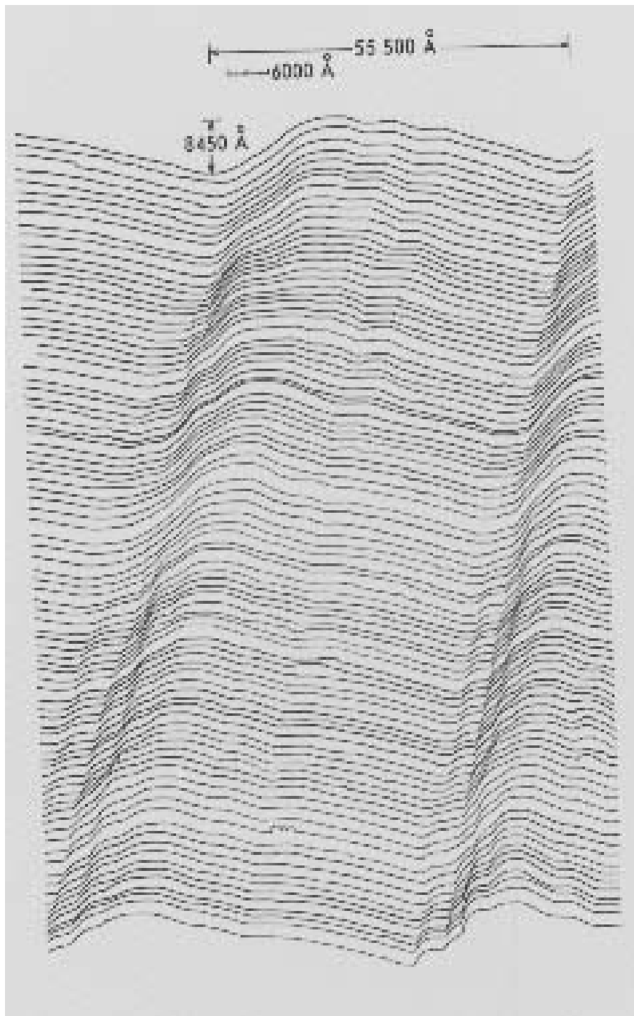

Figure 2-5: Diffraction grating. This was the most complete Topografiner image Young was able to make. It shows two different lines in a diffraction grating (a material with precisely ruled lines in it that cause incident light to diffract and separate into its constituent wavelengths). Because the spacing between lines must be exact, diffraction gratings are a traditional material of precision engineering. From Young (1971).

microscopists (particularly at the Bureau); a better understanding of how to collect secondary electrons and turn them into an image was central to the rapid rise of scanning electron microscopy (as opposed to the older transmission electron microscopy). Young thought it might be possible to form a similar image from secondary electrons associated with the Topografiner, and that such an image might interest Simpson and other EMers. Notably, this is the *one* feature of the Topografiner not present in later STMs, and, indeed, is only beginning to receive attention today.

Finally, there is one audience conspicuous by its absence – surface science. Given Young's ties to surface science, he might have been expected to sell the Topografiner to that community. Indeed, *after* the success of the STM, the American Vacuum Society awarded him its prestigious Gaede-Langmuir award for his invention in 1992. At the time, though, there was little to attract surface scientists to the Topografiner. Young concentrated almost exclusively on metals, rather than the more fashionable semiconductors; his samples of choice were all engineering materials like gratings and gage blocks, rather than the ultra-clean single crystals valued by surface scientists; the applications he talked about were metrological rather than fundamental; and the best resolution he thought he could get was hundreds of times larger than the atomic scales surface scientists had become preoccupied with. Moreover, surface science was exactly the kind of "high science" Young's managers discouraged him from following; to go in that direction would have curtailed the project even faster. Ironically, as we will see in Chapters Three and Four, when STM flashed onto the scene in 1983, it did so because its inventors made a lasting alliance with surface science. That alliance was the product of shrewdness, luck, and curiosity, rather than a natural conclusion from the instrument's capabilities; the fact that someone as central to the formation of surface science as Young could not convince that community to take up tunneling microscopy in the ’70s shows how illogical and unlikely such an alliance appeared at face value.

Young believed that if he made sufficient progress in a short time, he could demonstrate the relevance of the Topografiner *to* various communities *for* his managers, and they would let him continue work on the instrument until it could be turned into a reliable part of those communities’ toolkits. He worked to describe to his managers signposts along that road that he had achieved, and future signposts toward which he was working. For the Bureau management, facing budget cuts, scandalous backlogs for industrial customers, and increasing congressional pressure, the road to routinization of the Topografiner was simply too long. They viewed Young’s work as a proof of concept that such an instrument could work, a proof various communities outside the Bureau could follow up on [RY1, 6/29/00; BG1, 6/11/02]. More importantly, they saw the Topografiner effort as “proof” of the technical and management capabilities of Young himself. On a shoestring budget, against the (friendly) opposition of his managers, Young took an esoteric idea from electron physics and demonstrated that it could, down the road, have significant industrial and metrology applications. In an era when the Bureau was turning away from basic research, Young’s managers felt it was important to move someone with his expertise, management ability, and familiarity with precision engineering into a more applied area; since the best way to get someone off a project is to promote them, they told Young he could quit the Topografiner and accept a promotion or leave the Bureau. Thus, Young moved up to head the Bureau’s new Precision Engineering Division and never resumed work on tunneling microscopy until after his retirement in 1981.

Curiously, at the time practitioners outside the Bureau read the decision to halt Topografiner work in the light of Young’s and the Bureau’s past accomplishments rather than their then-current difficulties. As these corporate surface scientists (one at Xerox and one at Ford) put it:

> CD: If you look at the people who do the good experimental stuff [in surface science], they all still build their own [instruments].... The good stuff is home-made. You've got a group of people who really knows how to do good electron optics, and they do high resolution energy loss spectroscopy, they may do LEED, and they may do core level spectroscopy, but these are really specialty tools.... Not more than five or six people in the world can do this well. The vast majority of university types worked with bought gear....
> CM: If people found the Topografiner work interesting why didn't anyone replicate it?
> CD: [long pause] Well I think the real answer is that if you look at what was required to build that experiment, this was one of these homemade experiments. If the NBS group didn't do it, who else could do it? ... The people who built equipment were fairly rare by that time. These guys had all had their heyday 20 years, 15 years earlier. The NSF wasn't sponsoring any of this stuff. They weren't going to pay for people to build their gear. So the real answer is that the skill set had moved on in America. [CD1, 10/30/03]

> A number of people tried [vacuum tunneling] and didn't have very much success. I think the Bureau of Standards made the most elaborate attempt at doing it.... But they didn't quite get as far as anything really exciting because all they were able to do was build a machine that did topography, and that was on a relatively coarse scale.... You could build [a vacuum tunneling system] in the '60s. If you think about it, what's so conceptually difficult about it? But it wouldn't be easy. It'd be very expensive and hard and I don't think anybody conceived that it would work. The Bureau of Standards experience seemed to prove that out. They worked very hard and they put a lot of effort into it.... I figured if the Bureau wasn't able to accomplish anything more than that with all their resources well I sure couldn't. [BJ2, 6/27/01]

Potential replicators did not see the Topografiner's few accomplishments as an invitation to continue the work; rather, they believed that if *even* as experienced and renowned an experimentalist as Young, given the resources of an institution like the Bureau, could not get the Topografiner to work reliably or produce images at better resolution than existing instruments, then no one could. Their folk sociology of the Bureau, and of Young's place in it, led them to interpret the situation in exactly the opposite way from Young and his managers. This was trivially true of practitioners in industrial precision engineering, who would have been unlikely under any circumstances to follow up on as esoteric and unreliable an instrument as the

Topografiner except at the largest and most basic research-oriented companies like Bell Labs and IBM (where, indeed, the Topografiner concept was transformed into the STM a decade later). The "proof" of the unworkability of the Topografiner was probably most influential, though, to a subfield that Young had only tried to enroll very late in the game – the electron tunneling community.

From the earliest days of quantum mechanics, it was obvious that, given a high enough voltage and a small enough distance, an electron could be made to tunnel through a vacuum from one metal surface to another. Indeed, when theorists pictured the tunneling process, the imaginary apparatus they used to think through the problem usually consisted of two infinitely wide metal plates with only a (vacuum-filled) gap of less than a nanometer between them. For three decades, though, metal-vacuum-metal electron tunneling remained a theoretical certainty but an experimental nightmare. Because of the same vibration problems that plagued the Topografiner, no one could keep two metal plates separated by such a small distance. Finally, in the late '50s and early '60s, Leo Esaki at Sony (later IBM) and Ivar Giaever at General Electric came at the problem in a new way. What if the two surfaces were kept apart by a solid spacer rather than a vacuum? Since electrons were thought to interact only weakly during tunneling, the solid spacer would approximate nicely to the much more theoretically tractable vacuum, yet would be much easier to build (Giaever 1974).

From this insight, Giaever and Esaki founded a community centered on the metal-oxide-metal (or "sandwich") tunnel junction. To make a sandwich tunnel junction, they first deposited (inside a vacuum chamber) a thin strip of metal. Then they carefully grew a thin (one or two atomic layers thick) oxide film on the metal strip – usually the metal's native oxide. Finally, they grew another layer of metal on the oxide. By putting a voltage across the two metal strips, they could induce electrons to tunnel across the oxide from one strip to the other. At first, this work was especially prone to false positives – if the oxide layer had any pinholes in it, the two metal strips would meet and electrons would conduct directly from one to the other. As a result, when newcomers entered the tunneling community in the late ’50s and early ’60s, many reported “miraculously” clear spectroscopic signatures that later had to be retracted. Eventually, Giaever developed a test for the integrity of a junction involving detection of the superconducting band gap of the metal strips [BJ2, 6/27/01; CT1, 6/28/02]. This, in turn, led to a quick and reliable way of accrediting newcomers to the tunneling community.

By the late ’60s, sandwich junctions became the focus of a routine, if arcane, body of practice. Tunneling experts had a thriving business generating spectroscopic signatures for more and more combinations of metals. Yet the idea of true vacuum tunneling still piqued the interest of many in the field, and it was not unusual for dabblers to make quick tries at solving the problem. In the last days of the Topografiner effort, Young turned his attention to these people by building a stationary apparatus to bring a probe down into the tunneling regime. Indeed, he published current-voltage (I-V) curves that purported to show just that (Young, et al. 1971). Yet he was never confident of these curves, and those in the mainstream of the tunneling community who knew about the Topografiner interpreted their poor quality as meaning that if even Young could not crack vacuum tunneling then no one could. For those in this mainstream, sandwich tunnel junctions offered a generative path of experimentation that would last for many years; and with such an extensive and specialized body of practice surrounding their experimental system, they saw little need to latch onto something as limited and unreliable as the Topografiner.

For those at the margins of the sandwich junction community, though, vacuum tunneling was a way to cut through to the center of the field. This was the case for Clayton Teague, a graduate student at North Texas State University. Teague was trying to grow a tunnel junction using bismuth, but ran into trouble because of bismuth's lack of a native oxide [CT1, 6/28/02]. Having chipped away for several months with little progress, he ran across Young's articles on the Topografiner and vacuum tunneling and realized such an apparatus might offer a solution. In 1972 he contacted Young and came out to the Bureau, at first for a few weeks and then permanently, to complete his dissertation research by building a vacuum tunneling assembly. The instrument he built was a radical simplification of the Topografiner. Instead of using a tungsten emitter and a flat metal sample, Teague made two solid droplets of metal that he could both mechanically (with a differential screw) and piezoelectrically bring into the tunneling regime (see Figure 2-6). After fruitless attempts to form droplets of bismuth, he moved away from the original project and chose a gold-vacuum-gold system instead (gold is chemically inert and thus easy to keep clean). Also, unable to make the piezos work at the liquid helium temperatures necessary to observe the superconducting energy gap (a litmus test within the tunnel junction community), Teague decided to observe tunneling at room temperature instead. Indeed, as Young had done with the ultramicrometer, Teague used controlled temperature variations to adjust the size of the gap between the two droplets and thereby affect the tunneling current.

It is difficult to overstate how marginal this effort was. As with the Topografiner, Teague's work was done on the side, with a limited budget, and managerial skepticism directed at both Young (who was overseeing the work) and Teague (who was working part-time, then full-time, for the Bureau on other matters). Moreover, anyone who knew what they were doing in the tunneling community was not interested in metal-vacuum-metal systems; in fact, as it turned out, Jaklevic and Lambe (two premier tunnelers at Ford) eventually managed to make an ordinary sandwich junction with the same oxide layer on bismuth that had stymied Teague.

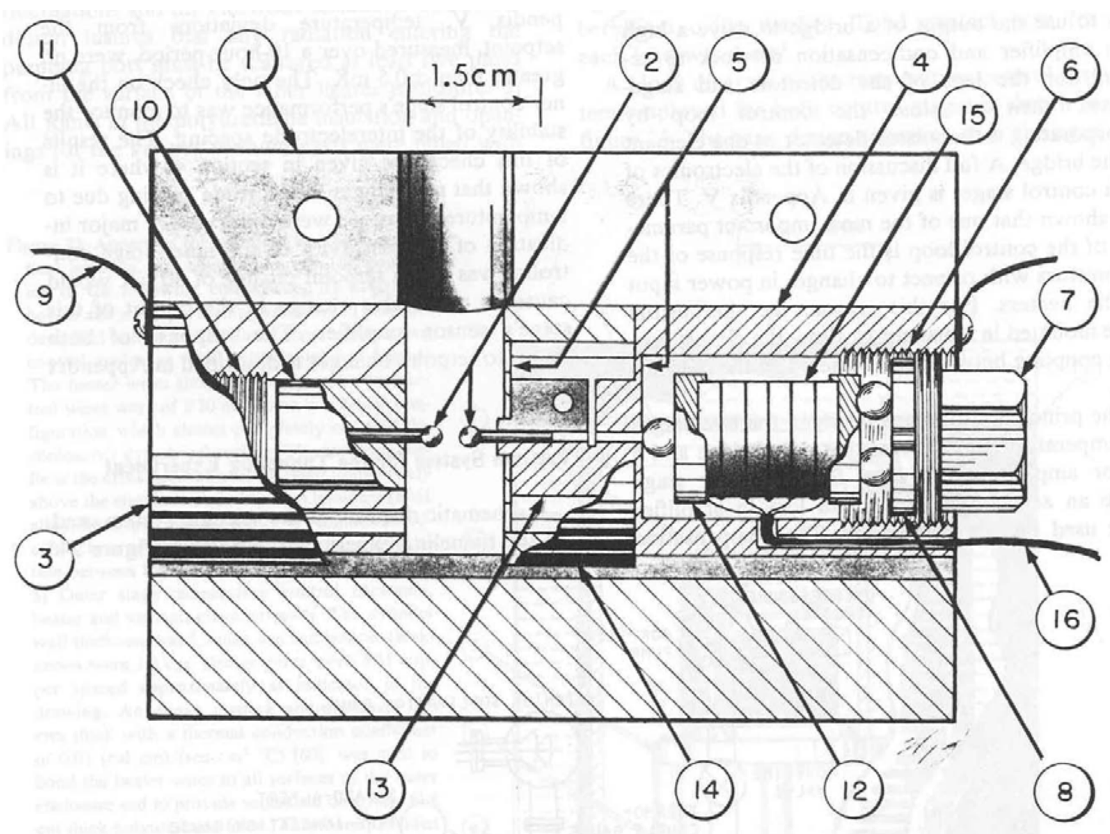

Figure 2-6: Teague's vacuum tunneling apparatus. A diagram of the vacuum tunneling instrument developed by Clayton Teague at the Bureau of Standards in the mid-’70s. The device worked by putting a voltage across two gold spheres (indicated by the number 2) and bringing them together using a differential screw and piezo set-up until electrons could tunnel from one sphere to the other. Teague discovered that as the spheres approached, interatomic forces between them distorted their shapes and added an offset to his tunneling measurements. This observation was later helpful in the invention of the atomic force microscope by Binnig, Quate, and Gerber. From Teague (1986).

Here and there isolated researchers were interested in metal-vacuum-metal systems – most notably Bill Thompson at IBM and U. Poppe at the Forschungszentrum in Jülich, as well as an even more obscure group in Russia that, it emerged later, had reported vacuum tunneling in 1966 (Thompson 1976; Poppe 1981; Lutskii, et al. 1966). Very few people, though, found vacuum tunneling worthy of notice; with the decline of electron physics and its splintering into surface science, field-ion microscopy, andsandwich tunnel junctions, no one found the "discovery" of vacuum tunneling exciting. What they wanted was tunneling that *told* something about a system – tunneling that could indicate the bond strengths of molecules placed in the oxide layer, for instance. This kind of information was seen as easy to obtain with a sandwich junction and difficult to obtain with the Topografiner.

Thus, when Teague reported his findings at meetings in 1975, he generated tepid interest, and so, other than his dissertation, his only published write-up of the experiment languished until well after the first STM publications (Teague 1978; Teague 1986). Not that it probably would have made much difference had he published before then; though the STM's inventors date its birth from their first (probably unwitting) replication of Teague's effort in 1981, their reporting of vacuum tunneling met exactly the same indifferent response (Binnig, et al. 1982). Tunneling *per se* was not the source of the STM's success; rather, an adventitious combination of institutional and disciplinary factors (strikingly similar to, yet crucially different from, those surrounding the Topografiner) fostered the STM. Curiously, once the STM began to spread, Teague's work enjoyed a rebirth. The inventors of the STM – despite their great openness –often had difficulty communicating all the essential instrument-building knowledge to their replicators. Thus, people like John Clarke and Cal Quate who built STMs in the mid '80s unearthed Teague's dissertation as a practical guide to constructing a tunneling apparatus. Indeed, copies of the dissertation circulated widely, especially in California, and some of Teague's comments on the force interactions between his gold droplets instigated the crucial insights behind the atomic force microscope [CT1, 6/28/02].[20]

Conclusion

Thus, this chapter is largely a counterweight to the story of the invention of the STM told in Chapter Three, a story that can sound inevitable and triumphalist. The STM and the Topografiner were grown in similar media, yet it was the subtle institutional, disciplinary, and interpretive differences between these instruments that contributed to their dissimilar careers. Still, the Topografiner was not merely a failed STM. Its proponents saw it in the '70s as a *success*, although one that, for institutional reasons, was not properly capitalized on; nor, indeed, would they have envisioned that capitalization as at all resembling what happened to the STM. The Topografiner is instructive as a comparison to the STM; but it can also offer some lessons of its own about the role of invention, institutional context, and disciplinary community at the boundary between instrument engineering and scientific experimentation.

The first lesson of the Topografiner is that a technology – what it looks like, what it can do, how it should be used – is often woven from the yarn of institutional imperatives. The Topografiner came into being because of a very complicated constellation of personnel, political pressures, discipline formation, and institutional evolution at the Bureau of Standards between 1965 and 1975. The basic conceptual and material components of the instrument – the field emission and tunneling equations, feedback circuitry, vacuum technology, and piezoelectric crystals – were all available for decades. The basic demand for the Topografiner to calibrate engineering surfaces was also around for a long time, though demand increased in the late '60s; indeed, given the institutional framework of the Bureau, it was that increased demand

[20] As referenced in, for example, Quate (1986) and Binnig, et al. (1986).

that led the Topografiner to be put on the back burner. At the same time, what made the Topografiner possible was the generative intersection (through individual researchers) of the Bureau and various technical subcultures (particularly surface science) at various stages in their own institutionalization. The Topografiner could not have come about based on the demands of the Bureau alone; it needed also to be part of various ongoing, wider communities.

Second, materials and the problematics associated with them – things like single crystals of silicon (111) or metal-oxide-metal sandwich tunnel junctions or diffraction gratings – anchor such technical communities. A new instrument exists by the grace of such problematics; if canonical materials, material-centered questions, and instrumentation cannot be molded to each other, then most of a community will lose interest in the instrument. This was the critical mass that Young never quite assembled – there were no materials upon which the Topografiner could either close or open up questions that were of interest to any community, especially not in a way that was not already possible with existing, more reliable instrumentation. As we will see in the next chapter with the scanning tunneling microscope, an instrument can stay in this state for a while if it is hidden and/or it is an individual obsession, but it will not be replicated or routinized unless it captures some wider imagination. Curiously, this provides a space for technologies to live on in unusual ways. The Topografiner, for instance, was able to provoke new lines of inquiry and new lines of development at the margins of established communities. Later, when the questions and materials of various communities had changed, and new private obsessions had been sparked by the STM, the Topografiner could be reinterpreted as being part of a new kind of problematic.

Chapter Three

Naïveté and the Invention of Tunneling Microscopy

Apart from Clayton Teague's work on vacuum tunneling, the Topografiner died childless. Those who might have found a use for it – surface scientists, precision engineers, the tunnel junction community – found nothing in the Topografiner to make them move away from the more proven instruments with which they were already working. The termination of Russ Young's project had "proven" to them that such an investment in this immature technology would only end in failure. Though in conception and design the Topografiner prefigured the basic elements of later probe microscopes, it never formed the kernel of a growing and innovative community.

The instrument that did form this kernel was the scanning tunneling microscope (STM), invented at the IBM Zurich research lab in 1981-2 by Gerd Binnig and Heini Rohrer.[1] This chapter will examine the local culture of innovation that grew up around Binnig, Rohrer, and the STM. By exploring both the institutional and individual characteristics of this culture, we can understand why Binnig and Rohrer succeeded in cultivating an STM community where Russell Young had not. Importantly, we will see that willfully turning a blind eye to past attempts at tunneling microscopy – indeed, a general embracing of naïveté and even ignorance – aided the Zurich team in pushing past the failure of Young's proof of concept.

Tunneling microscopy began as an auxiliary technology, rather than as a project in its own right. IBM Research – particularly the Zurich lab – was embroiled in the '70s in an effort to build a revolutionary new high-speed computer based on superconducting Josephson junctions, the kind of large-scale, all-or-nothing, high-stakes effort only a behemoth like IBM could envision, fund, and attempt to carry out.

[1] For a brief history of IBM Zurich, see Speiser (1998).

Josephson junctions are based on the theories of Brian Josephson, a British physicist who shared the 1973 Nobel Prize with GE's Ivar Giaever and Sony's (later IBM's) Leo Esaki, the founders of the tunnel junction community that we met in Chapter Two. Josephson observed in 1962 that a tunnel junction made from two superconductors separated by a very thin insulating layer would have interesting properties when kept at superconducting temperatures.[2] If no voltage drop were placed across the two superconducting layers, a current would actually flow between them; and if there were a voltage drop, then it would induce a very fast alternating current between the layers. By the late '60s, IBM researchers had become attracted by the speed and sensitivity of such "Josephson junctions" to try and incorporate them into an ultrafast supercomputer. IBM Research had put much of its money, reputation, and talent into the Josephson project in the late 1970s, in the hopes Big Blue could produce a computer that would leap-frog semiconductor-based micro-processors and alleviate competition from Silicon Valley firms like Intel. Thus, much of the future of the Research Division, and the company as a whole, rode on the outcome of the Josephson effort, and any employees whose expertise could be made relevant to Josephson computing found themselves contributing to the project.

The problem was, IBM Research began to find that Josephson junctions were an extremely finicky technology. Building one or two junctions to demonstrate Josephson's theory and perform very simple logic operations was easy enough, the kind of proof of concept IBM scientists excelled at. Scaling up from there to a full-fledged computer with hundreds of individual junctions, though, was the kind of task – at the interface between the manufacturing and basic research arms of the organization – that, as Russ Bassett has shown, IBM had problems with (Bassett 2002).

[2] Superconductors are materials that lose all resistance to electrical current below some (very low) critical temperature.

One difficulty, as with most ultrafast computers (such as the Cray-1), was with heat.[3] Since the superconducting materials of the day only became superconducting within a few degrees of absolute zero, all the Josephson logic elements had to be packed together in a bath of liquid helium, an expensive and finicky material. Peculiar to the Josephson project, though, were reliability issues in making the junctions themselves. The insulating film between the two superconducting layers must be extremely thin (on the order of a nanometer – in order to allow tunneling), yet it must also be perfectly homogeneous. In particular, there must be no "pinholes" that will allow the two superconducting layers to touch and short-circuit the junction. Even experts in the tunnel junction community had to use a whole tool-kit of arcane, tacit skills to make just one such junction; yet IBM was proposing to mass-produce chips with hundreds of junctions, all with no pinholes, at close to 100% reliability rates.

Ultimately, these issues might have been solved, and low-level research on Josephson computing is still continuing. At the time, though, a Josephson computer could only have been meant as a mainframe or (more likely) a scientific supercomputer. Yet as the IBM PC burst on the scene in 1981, the big computer market began to soften, ultimately resulting in Big Blue's disastrous losses and reorganization a decade later. Thus, in 1983, the plug was pulled on the Josephson project after only a few chips had been built. The quagmire of the Josephson effort is important for our story because it triggered the early development of the STM and also set the tone for the Zurich lab. The Zurich IBMers were heavily invested in the Josephson work, and its cancellation led to a period of uncertainty and low morale. Yet within a few short years, two separate Nobel Prizes, awarded to four different

[3] For some analysis of the technical challenges of making and selling supercomputers, and the competition between Cray and IBM, see Mackenzie and Elzen (1996). Seymour Cray's appropriation of the role of "charismatic engineer" is similar to the charismatic inventorship of Binnig and the instrument builders we will meet in Chapters Five and Seven.

employees, had been spun off from the Josephson effort. One prize, in 1986, went to Binnig and Rohrer for the STM; the other went to Georg Bednorz and Alex Mueller in 1987 for their discovery in 1986 of high temperature superconductors. Bednorz and Mueller's interest in unusual superconducting materials in the early '80s has to be seen in the context of the technological problem of making reliable superconducting Josephson junctions that could operate at higher (hence cheaper and more reliable) temperatures. Similarly, while the STM could be interpreted as arising from Gerd Binnig's interest in basic questions about vacuum tunneling, Binnig was "interested" in an almost infinite number of "basic" research questions. In fact, his development of technologies to explore these basic questions came about because he saw a way to attach them to the Josephson effort and receive institutional support for his work. Indeed, early on Binnig and Mueller collaborated on the search for superconductivity in oxides, though, as Bednorz puts it, "Gerd then lost his interest in this project and with deep disappointment I realized that he had started to develop what was called a scanning tunneling microscope" (Bednorz and Mueller 1993). Both high T_c superconductors and the STM, though, survived the demise of the Josephson project in part because they were at the margins of the effort; in an organization as large and complex as IBM Research, small groups could carve out and maintain niches long after their original reason for being had vanished.

The original basis for the STM cropped up in 1978, as the Zurich Josephson team began to realize the importance of pinhole defects in their insulating layers as a cause of junction unreliability. Thus, one part of the team set out to perfect a process for growing defect-free films. To do so, though, they saw they needed a better way to characterize films to better understand how different growth processes affected the size, shape, and quantity of defects. They asked a senior scientist/manager at IBM Zurich, Heini Rohrer, to explore various ways of characterizing thin films (Binnig and

Rohrer 1987). Rohrer saw this as a perfect assignment for a junior scientist, so he cast around for a recent Ph.D. to hire and oversee. Eventually he settled on Gerd Binnig, who was just completing his dissertation work on tunneling and superconductivity. Rohrer knew Binnig's thesis adviser, had seen Binnig present at conferences, and believed the film characterization project needed someone with Binnig's particular expertise. So he brought Binnig out to Zurich for a visit, explained the project to him, and together they discussed the ideas that germinated in the STM.

The core of their initial conception was the need for a device with an extremely localized sensitivity to electrical properties. The pinholes in the oxide films were *very* small, yet they significantly affected the electrical characteristics of the whole film. To understand how defects were being created, and what processes might ameliorate them, Binnig and Rohrer needed to see exactly where and how big the defects were and how they were disrupting the Josephson effect. There were traditional methods – e.g. electron microscopy – that could tackle part of this problem, but, in their view, no technique could give the whole package of spatial resolution and electronic characterization. Their brainstorming led them to ask: what effects are so highly localized that they could probe the electrical characteristics of the pinholes? Before long, they focused their discussions on tunneling, probably because both of them had a background in that area. Both realized that, under the right conditions, tunneling can be extremely localized – the cross-section of a tunneling event is less than an angstrom wide – and that a tunneling current coming out of an area covered by oxide should look quite different from that coming out of a pinhole [GB1, 9/26/00; HR1, 11/13/01]. The instrument they envisioned, therefore, would bring a sharp metal probe close enough to the film to witness electrons tunneling between surface and tip. The tip could move around the surface, finding locations where the strength of the tunneling current indicated the presence of a pinhole. From this, statistics about the number, location, and size of pinholes could be obtained.

A comparison between this instrument and the Topografiner is instructive. Both instruments were initially seen as highly applied, industrially relevant tools; both emerged from large, corporatized laboratories, where their inventors worked to insinuate them into the larger projects of the organization. Crucially, both tried to finesse themselves into a niche that overlapped older, more proven instruments. The resolution of Binnig and Rohrer's device was, like the Topografiner's, calculated to be highly dependent on the radius of curvature of the probe; thus, the two instruments should have had resolutions roughly comparable to an electron microscope. When the Bureau of Standards ran into financial and organizational difficulties, therefore, it dropped the Topografiner on the grounds that it could do little that other instruments could not do. When IBM Research ran into similar difficulties with the Josephson project, on the other hand, it pressed ahead with Binnig and Rohrer's instrument.

This is emblematic of the cultural differences between the organizations. Where the Bureau of Standards was a government institution, constructed to serve and be responsible to the public, IBM Research was responsible (tenuously) only to Big Blue's stockholders. The Bureau stood on the razor's edge of public scrutiny (during the Topografiner period, the NBS had to deal with budget cuts and harsh Congressional oversight), whereas IBM Research lived off the fat of the land (the company made it a point of honor to plow 10% of its budget into research - see Speiser (1998)). Big Blue's huge size and market dominance meant (A) it could pour copious money and resources into any project, particularly a make-or-break one like the Josephson effort; and (B) its near-monopoly in its field meant it had to develop technological solutions for problems no one else would face. As Bill Leslie and others point out, in certain technological areas (such as chip manufacturing), IBM and companies like it (particularly AT&T) developed idiosyncratic technologies and progressed down culs-de-sac no one else could imitate, yet which served the needs of these giant organizations (Pugh 1995; Leslie 2001; Knowles and Leslie 2001; Bassett 2002). The Bureau of Standards needed to develop technologies that a wide variety of companies and industries could draw on; whereas IBM spent enormous amounts to develop technologies only IBM would use.

Thus, at first no one flinched at the idea of Binnig and Rohrer focusing their efforts on making an instrument that would offer only marginally better information than devices that could be bought off the shelf. IBM was willing to pay for any extra advantage to help the Josephson effort; and, besides, this kind of innovation was at the core of the corporate research mission. So, exactly the kinds of obstacles that blocked the Topografiner in the culture of the Bureau made Binnig and Rohrer's instrument plausible at IBM. Plausible, but not probable –vacuum tunneling was something that, as far as anyone at Zurich (probably) knew, had never been done even in a highly-controlled laboratory environment, much less in the highly-applied industrial context Binnig and Rohrer envisioned. Many of Binnig's colleagues found his boldness foolhardy for a young, unestablished researcher:

> When we started they were very skeptical and people approached me, – I was a youngster here, I had just started – they said "are you crazy, just starting with such a risky project right from the beginning? That's very dangerous. I wouldn't do that." [GB1, 9/26/00]

Binnig, though, cultivated an image of himself as a "crazy" maverick who pursued directions others "knew" would not work.[4] As he puts it, "will and self-trust are important. Sometimes naïveté does not hurt. When one knows too much, one can become dispirited" (Binnig 1989, 89). Importantly, though, Binnig has almost always worked as part of a team, where he could position himself in a maverick role within

[4] Much has been written about the cultivation of scientific persona in the past few years. Most useful for me are Shapin (1991); Browne (1998); Biagioli (1993).

the group in such a way that, collectively, the group could sow enough disciplined expertise in with the naïveté in order to succeed.[5] On the few occasions when Binnig has worked alone, or as part of teams that could not support his maverick role, he has come off as marginal and out of his depth. When he has been part of the right kind of team – as he was in the early years at Zurich – he has seemed more like a brilliant and inventive, if off-beat, researcher.

This can be seen in the make-up of the original Zurich STM group. Binnig tended to be the idea man, and the gifted, if makeshift, experimentalist. A technician, Christoph Gerber, supplied much of the technical know-how and meticulous craft work to realize many of Binnig's ideas (another technician, Eddie Weibel, was also involved early on). Rohrer, meanwhile, gave the project occasional guidance and acted as a mediator between Binnig and Big Blue. As a group manager, Rohrer could shape the group's efforts so they would be seen as contributing to IBM Research's overall aims; but he could also disguise the group's work and make it invisible to senior management when Binnig's goals and those of IBM were too hard to reconcile. This soon proved to be the case with the team's contribution to the Josephson effort.

Originally, Binnig and Rohrer thought their instrument would take readings of the tunneling current at one spot, then another, then another, yielding *statistics* about the frequency of pinholes [GB1, 9/26/00; HR1, 11/13/01]. Within a few weeks of sketching out this idea, though, Binnig realized that a sufficient density of such samplings would yield a real-space *image* of the film. By scanning the probe in the x-direction while feeding back on the tunneling current, the instrument could give line traces of the electrical characteristics of the films; and by translating the probe in the y-direction after each scan, a set of such traces could be collated together – much as

[5] This echoes Howard Becker's observations about how mavericks such as Charles Ives need more mainstream helpers to facilitate the acceptance of their "discoveries" (1982). For an application of Becker's ideas in science studies, see Fujimura and Chou (1994).

rastered pixels in a television or scanning electron microscope form an image on the screen. The size and location of pinholes could be made *visible*.

As we saw with the Topografiner and with Clayton Teague's tunneling apparatus, making an instrument that could tunnel seemed difficult enough to most experimentalists; yet quick calculations showed that that that was a much simpler prospect than making one that could tunnel *and* scan. The mechanical instabilities involved in keeping a probe that close to a surface while also moving it around in x and y would cause the probe to continually crash. Building such an instrument could have seemed like an enormous and fruitless investment of time, people, and resources – especially since its original inspiration, the Josephson project, was coming to an unsuccessful end. Thus, Binnig and Rohrer shaped their work to combat skepticism of the science and economics of the instrument (which by then they were calling the scanning tunneling microscope or STM). First, after some initial attempts at building a full-fledged STM, Binnig and Gerber decided to tackle the much simpler, more basic problem of tunneling without scanning. By building a stationary apparatus they could prove that a vacuum tunneling current was possible, without incurring the extra mechanical difficulties of scanning. Second, since the Josephson effort could no longer be used as a pretext for their work, Rohrer arranged to partially hide what they were doing. He assigned Binnig and Gerber to other tasks on which they could be seen to be making progress, but which would be easy enough that they could pursue their STM work part-time.

> CM: What was the STM group at Zurich like?
> OM: What it was? Hidden. As far as I know Gerd Binnig was hired officially at least to do mixing cryostat measurements in the millikelvin range. There was actually a cryostat like that. Well, he always was pretty impatient.... Heini Rohrer was working [on STM], let's say on a 10% basis probably at that time. Gerd Binnig probably on a 60% to 70% basis. [OM1, 11/16/01]

From one perspective, Rohrer was giving Binnig the space to pursue an interesting, if esoteric, problem. From another perspective, though, he was drawing resources away from work that could more directly contribute to IBM's aims. The latter can be seen as the view of managers at the center of IBM Research in Yorktown Heights, New York (just down the road from corporate headquarters).

> I was kidding one of the research managers whom I know pretty well from Yorktown Heights and said "here IBM Zurich is just this little operation of a research lab, to what do you attribute the fact that they've come up with two Nobel Prizes over the past two years?" He said "poor management." At the National Bureau of Standards, where Young had better management the project was stopped, and at IBM Zurich where they were pretty much leaving those people alone it went ahead and they made these important discoveries. [DH1, 2/28/01]

Binnig and Rohrer, though, saw themselves as at the periphery of IBM. Indeed, much of their work displays a distrust of corporate supervision of research and a willingness to subvert directions from Yorktown. This, of course, plays well to Binnig's careful construction of persona, and over the years he has carved out a niche as the colorful maverick who dances around the fringes of the buttoned down corporate world.[6]

As with the Topografiner and Clayton Teague's tunneling apparatus, vibration isolation appeared to be the greatest hurdle to building a successful STM. Here, Binnig's Rube Goldberg-like imagination came into play. Over several generations of STMs, he devised a variety of complicated, bulky, esoteric systems for vibration isolation – eddy-current damping, instruments levitating in bowls of superconducting helium, four stage spring systems surrounded by intricate, beautifully hand-blown glass scaffolds, etc. (see Figure 3-1). Arne Hessenbruch has written about this period of STM development as a time when Binnig and Rohrer faced great skepticism from their colleagues within the Zurich lab.[7] He shows that these complicated, high-physics

[6] This is, surely, a well-worn trope. We can see it, for instance, in Paul Rabinow's (1996) evocative descriptions of Kary Mullis' antics at Cetus.
[7] Some of this argument can be found in Hessenbruch (2001). My thanks to Arne for conversations that elaborated his argument in more detail.

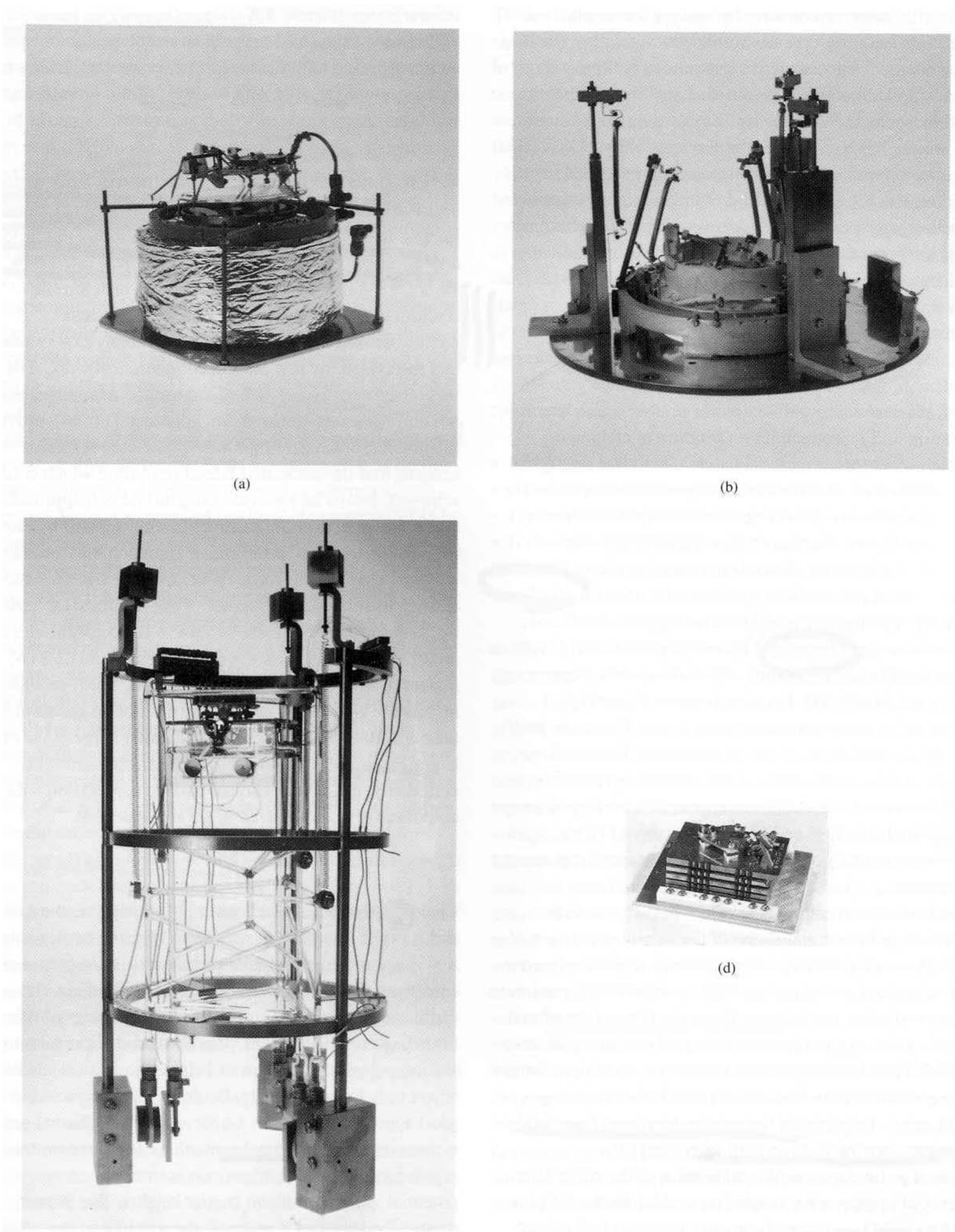

Figure 3-1: The Zurich STMs. The first four generations of STMs in the IBM Research lab at Zurich, differing mostly in their vibration isolation systems. The first levitates the STM by putting it on a bowl of helium-cooled superconducting lead; the second and third use complicated multi-stage spring suspension systems. All three also use eddy-current damping, with permanent magnets placed in an electrical field, in which they resist movement. The fourth is the "pocket STM." From Binnig and Rohrer (1986).

solutions to the vibration problem were a way to display to those colleagues that the STMers' results could not be written off. Just as important, though, is the role of these solutions in helping Binnig carve out a maverick scientific persona. Each of these systems constituted a possible avenue for development that would display Binnig's inventive genius and might prove more interesting than the STM itself.

The Ethic of Naïveté

Thus, both because of the need to disguise the STM work and because of Binnig's tendency to rebuild much of the device on completely new principles every few months, progress came extremely slowly. From the first conception in 1978, it took three years to get a well-defined vacuum tunneling signature (Binnig, et al. 1982). Another reason for the slow development, though, may have been the team's steadfast refusal to learn from other groups working in the same area. Not only did Binnig radically revise his own work every few months, but he repeatedly reinvented technical solutions that were readily availabe in the relevant literature. For him, *everything* about the STM was unprecedented and novel. This was made easier by a cultivation of blithe unawareness of Teague and Young's work at the Bureau of Standards, not to mention the Soviet vacuum tunneling work in the mid-'60s. At this early stage, Binnig and Rohrer seem not even to have paid special attention to, much less contacted, people in the tunnel junction community such as Bob Jaklevic and Paul Hansma. The one partial exception, for institutional reasons, was Bill Thompson, a researcher working on squeezable vacuum tunnel junctions at Yorktown – here again, though, what might have developed into a lasting collaboration fizzled early, and today Binnig and Rohrer have difficulty even remembering Thompson existed [GB1, 9/26/00; HR1, 11/13/01].

My point is not some Mertonian normative (Merton 1972; Mitroff 1974) claim that they *should* have sought out precedents and collaborations. Rather, I'm making a descriptive claim that insouciance about such precedents was a central part of the Zurich STMers' construction of a local experimental culture. Later on, Binnig and Rohrer had no difficulty collaborating with other groups and using the STM to develop a wide and heterogeneous network. These collaborations, though, were done to expand the STM's range of applications, and Binnig and Rohrer's partners were from fields that could in no way be seen as precedents for probe microscopy. People whose work *could* be seen as a precedent – Russ Young or Bill Thompson or the surface profilometry community – were kept outside the group's ken. This "ethic of naïveté" was both generative and problematic for the group. *Not knowing* about precedents allowed Binnig and Rohrer to see the STM as a blank slate, an instrument with an unknown array of attributes. *Not knowing* allowed them to be unaware of what others had found to be "impossible," which in turn allowed them to proceed as if everything were possible (and push through the same "impossibilities"). *Not knowing* allowed them to be unaware that forerunners such as Teague or Young had sparked little or no interest in the communities to whom they presented their results. By *not knowing*, Binnig and Rohrer could represent their work as novel, rather than as the replication of something no one else thought needed replication.

Not knowing became problematic, though, when the STM made its way into the wider world. Some people who heard about the Zurich instrument in the early '80s *did* know about its precursors, and questions arose about how much Binnig and Rohrer owed to their forerunners. In Binnig and Rohrer's writings about their invention process they often use phrases like "after we were done we encountered the similar work of" X or Y. This way of putting things could seem disingenuous, and some, especially Russ Young's friends at NIST, made it a personal project to point out Binnig and Rohrer's citational lapses and criticize what they saw as misrepresentations of the field's history.[8] These lapses became even more acute when the Zurich team started patenting STM innovations. Their first application for an American patent in 1979, for instance, was rejected (in 1980) on the grounds that almost all of their claims were mere replications of what Young had done. This pattern became typical for Binnig – a brilliant idea, a quick implementation, followed by the belated and half-hearted acknowledgement of forebears, resulting in a botched patent application.

In general, this naïve style of work at Zurich was centered on Binnig's experimental persona; Rohrer and Gerber seemed to have enjoyed Binnig style of work at times, and to have provided much-needed structure at other times. It is difficult to tell when exactly Binnig evolved this naïve approach to research; it is clear, though, that by 1984, his collaborators and emulators in North America were already commenting on his maverick persona and his refreshing, if sometimes undisciplined, experimental style. For the period covered in this chapter, we need to be more circumspect in thinking about naïveté; it is possible that the Zurich team were not quite as naïve as they remember being. Certainly, there are reasons (given the questions of priority surrounding the invention of the STM) why it might be more comfortable, post-Nobel Prize, for the Zurich team to look back on their early work as isolated, undisciplined, and naïve.

Yet what can be pieced together of Binnig's early career at IBM Zurich – his tendency to lose interest in projects and move on to new ones, the designs of his STMs, the samples he characterized, etc. – shows that, from the beginning, he was working in a way that could easily be represented as a naïve style. Indeed, the ethic of naïveté should be seen as a way of doing things that incorporates a tendency to broadcast representations of itself *as* a way of doing things. Binnig, more than anyone

[8] For instance, when Cal Quate published his history of the invention of the STM, Bill Gadzuk at NIST fired back an angry letter to the editor accusing Quate, Binnig, and Rohrer of intellectual dishonesty (Quate 1986; Gadzuk 1987).

else in the probe microscopy community (except perhaps Virgil Elings, whom we will meet in Chapter Seven) is known widely as a *character*, someone about whom there are a variety of eccentric stories that circulate widely. Most of these stories traveled down through the same network as the STM in the period when the tunneling microscopy community was expanding. Even before then, though, the Zurich team was using representations of its naïve, insouciant style in order to navigate the institutional politics of corporate research; and it was crafting its work to make those representations easier to create. For instance, embracing naïveté pushed Binnig and Rohrer toward a more playful, undisciplined style of work. As they say at the start of their Nobel lecture:

> We present here the historic development of Scanning Tunneling Microscopy.... Our narrative is by no means a recommendation of how research should be done.... However, it would certainly be gratifying if it encourage a more relaxed attitude towards doing science.... For scanning tunneling microscopy, we brought along some experience in tunneling and angstroms, but none in microscopy or surface science. This probably gave us the courage and light-heartedness to start something which should "not have worked in principle" as we were so often told. (Binnig and Rohrer 1987)

Binnig, especially, developed a working style in which he tried not to be overly informed on a subject when he started to tackle it, to leave a subject when he found he knew too much about it, and to jump from one project or technical approach or idea to another playfully and quickly. This dovetailed nicely with his self-presentation as an outsider and maverick. In his autobiographical moments, Binnig describes how "I had always the feeling when I talked to people that I'm the only one who thinks like I think, or the others seem to think differently" and, hence, he often ran afoul of more disciplined institutions such as the military and mainstream university physics.

> While studying physics, I started to wonder whether I had really made the right choice. Especially theoretical physics seemed so technical, so relatively unphilosophical and unimaginative. In those years, I concentrated more on playing music with friends in a beat-band rather than on physics.... [U]nder Dr. E. Hoenig's guidance I realized that actually *doing* physics is much more

> enjoyable than just learning it. Maybe 'doing it' is the right way of learning, at least as far as I am concerned. (Binnig 1993)

The ethic of naïveté, therefore, should be seen as a Švějk-like means for Binnig to subvert such institutions by appearing unaware of their rules.

This may sound like a psychological description of Binnig's persona and, indeed, there may well be psychological explanations of maverick behavior. Binnig's wife is a psychologist, and he often references her as the only person who, through her training and affection, can understand him. Later, after Binnig tired of STM, he began writing in a more psychological tone about creativity (Binnig 1987; 1989a; 1989b; 1995). Yet we should not lose sight of the social coordination of this role. Locally, Binnig needed the help of Rohrer and Gerber in supporting his maverick style of work. Rohrer provided managerial cover for Binnig's unorthodox ideas, and, as Binnig says, "his humanity and sense of humor fully restored my lost curiosity in physics;" while Gerber brought instrumental form to Binnig's more undisciplined ideas, and circumvented the ethic of naïveté to bring in outside knowledge at key points (Binnig 1993). Also, Binnig's style was so closely associated with the STM that it was often replicated along with the instrument. As other groups (particularly those in Chapter Five) built their own STMs, some of them also adopted the ethic of naïveté as a useful tool in instrument-building.

Thus, through personal contact, through published articles such as their Nobel lecture, and through word of mouth stories about Binnig's maverick exploits, the ethic of naïveté was crucial in building a network around the STM. It was most important in the early process of filling in the STM's array of attributes. By presenting the instrument as completely novel, and themselves as ignorant about what it could do, Binnig and Rohrer were able to draw in more disciplined collaborators who brought with them key knowledge that could be made relevant to tunneling microscopy. Having shown that their stationary apparatus could produce a vacuum tunneling current, Binnig and Gerber built a new machine with scanning capability; the question now was, what to look at with it? For this, they needed samples – the actual, physical specimen they wanted to look at – and they needed knowledge about those samples. Housed, as they were, in one of the largest corporate research organizations in the world, they quickly found they could obtain both from their local colleagues.

Binnig and Rohrer needed two things from early collaborators. Unlike many of the instruments classically analyzed in science studies, the STM was not purpose-built for any particular scientific problem. Its ostensible reason for being, the Josephson project, evaporated as a pretext as the years went by. By 1981, Binnig and Rohrer had a tool, but no idea of what to use it for – the STM was an instrument in search of a problem.[9] Thus, the STMers looked for samples central to open scientific debates. Early on, though, it was also important to find samples to tell something about the microscope, rather than the other way around. Binnig and Rohrer sought samples with known, unusual properties that, in theory, would appear distinctive in the STM and could be used as a metric of its capabilities. Thus began a series of ephemeral and often fruitless collaborations between Binnig and Rohrer and various IBM colleagues who supplied them with samples and knowledge. Here, we see the beginnings of a division of labor that would crop up repeatedly in the history of probe microscopy – a division between microscope builders with the expertise to construct and operate an STM and sample providers with the expertise to choose and prepare specimens that might generate useful STM images.[10]

> The first sample was simply chosen because Dick Gambino, was visiting for a year from IBM Yorktown. He was a materials scientist, he had this calcium iridium tin 4.... It was very shiny. The hope was, since it's already shiny [i.e. flat and clean], we can do something.... [On that material] we just saw the [atomic] steps. And the steps were in line with what you would expect from a

[9] My thanks to Mario Biagioli for comments that were helpful in this section.
[10] Much the same dialectic is present in Nicolas Rasmussen's (1997) account of the entry of electron microscopy into biology.

> step. You needed to calibrate the piezos – the calibration was different from all the numbers you got because the numbers you had were usually the numbers for applying large voltages. [HR1, 11/13/01]
>
> [One early sample] was a material built by Hans-Jörg Scheel in our department. He's a crystal grower and he said "this is a good material, it's very inert so you should see the atomic steps, and the atomic steps are relatively high so you should resolve them very nicely...." If you have huge terraces of steps, then you actually can see them with an optical microscope.... "Take this sample, if your instrument really works you will see atomic steps," that's what he said. We tried it and we saw the atomic steps. [GB1, 9/26/00][11]

More important than calibrating the instrument, though, Binnig and Gerber were accruing tacit knowledge about how to operate an STM. Just by looking at *something* (particularly something with an ordered surface), they could tell when they were and were not getting images, what kinds of operations improved the images, and what kinds of operations resulted in STM "accidents" (accidents which they could begin to assign labels, such as "crashed tip" or "gunk on tip" or "sudden reversal of contrast") [GB1, 9/26/00; CG1, 11/12/01].[12]

Moreover, they were learning intuitive facts about what STM landscapes looked like and how to interpret them. Interestingly, despite their corporate affiliation, Binnig and Rohrer manufactured their STM images entirely without the aid of computers. As part of Binnig's construction of himself as a maverick within IBM Research, and as part of the whole group's representation of itself as quietly defying the received truths of the American corporate center, both Binnig and Rohrer dismissed computers as useless [GB1, 9/26/00; HR1, 11/13/01].[13] Visualization was accomplished by analog means. Individual line traces were outputted to an x-y recorder or an oscilloscope, one trace at a time. Binnig and Gerber learned to "see" a three-dimensional image by collating these line traces in their heads; or, it is more

[11] Results of characterizing this sample are in Scheel, et al. (1982).
[12] There is a nice resonance here with Polanyi's discussion of the blind man's probe and the gradual transition from "knowing how" to "knowing what" (1962, 56).
[13] For an article that chronicles part of the entrenchment of scientific computing at IBM, see Akera (2002).

accurate to say they learned how to interpret line traces as indicative of "features" and sites of interest on the surface. Occasionally, they used a storage oscilloscope (which could keep more than one trace visible at a time) to build up crude pictures of the surfaces by off-setting a collection of line traces; and, on one famous occasion, they physically collated a collection of traces and sculpted together a three-dimensional representation of the surface (see Figure 3-2):

> The data acquisition was via chart recorders. That's nice, it works fast, but if you have to analyze the data later on, that's a problem. It got aggravating when the results really got good.... I mean IBM is a computer company – can you just present chart recorder data with nothing else? So for this PRL paper on the 7x7 ... they took the chart recordings, copied them 50 or 60 times, and then Christoph Gerber had to cut out the first sheet of paper for the first line, the second sheet of paper for the second line, and so on. He was gluing cardboard on the back so that the things would stand and then everything was packed together in the right arrangement, put on a piece of wood, and then fastened with nails. [OM1, 11/16/01]

Binnig (especially), but also Roher probably saw computers as representative of a more bureaucratic, formal kind of research – indeed, we shall see in Chapter Three how it allowed the bureaucratization of certain aspects of STM. In counterpoint, the Zurich team offered themselves as representatives of a more artisanal, craft-like science. STMs that were digitally controlled and had computer outputs produced large amounts of data quickly; whereas the Zurich machines produced data monstrously slowly, each image a hand-crafted jewel of a result. Binnig and Rohrer departed very far from the initial idea of building an industrial surface characterization tool. Instead, they were doing self-consciously exploratory research, wandering around picking off the low-hanging fruit.

Surface Science and the 7x7

The problem was, no one was interested in that fruit. Binnig and Gerber had acquired a feel for the STM's habits, and cultivated an ability to read its almost indecipherable real-time images, but they had not seen anything in those images that

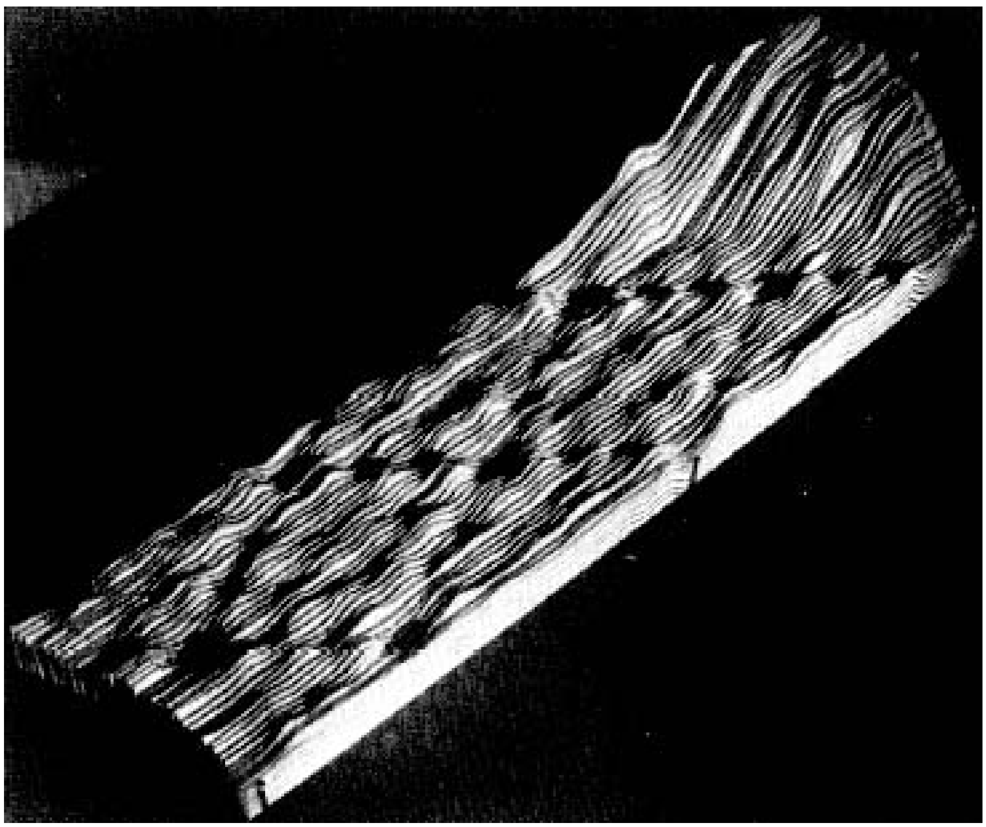

Figure 3-2: Binnig and Rohrer's 7x7. The famous cardboard model of the 7x7, built by Christoph Gerber by taking a series of line traces from a chart recorder, cutting them out, gluing them to cardboard, then gluing the cardboard together. Two whole unit cells are visible as the two diamond-shaped structures pointing toward the upper right. Individual adatoms are visible within the unit cells as large raised bumps. The corner holes of the unit cells are visible as prominent depressions at the vertices. From Binnig, et al. (1983b).

sparked anyone else's imagination. After the first stationary tunneling results in March 1981, though, the group began talking to surface scientists at Zurich. The STMers wanted samples that were easy to prepare, would stay clean, had properties that would be readily visible in an STM, and about which surface scientists already knew a great deal but wanted to know more. One material the surface scientists recommended was gold, because it is well-understood, chemically inert (i.e. easy to keep clean and "well-defined"), and readily available. STM trials with gold revealed a landscape of "rolling hills" typical of low-resolution STM images. After a while, though, Binnig and Gerber were able to find spots where they could see the corrugation (the wave-like rows of atoms at the surface – see Figure 3-3). From this they produced a rather wrought extrapolation from the images to a model of the surface reconstruction of gold (Binnig, et al. 1983a).

It is difficult to overstate how little notice this article attracted. It was published in *Surface Science*, a highly regarded journal; and, as later events indicated, it crossed the desks of surface scientists around the world [DH1, 2/28/01]. Yet no one seemed interested. Gold was already a well-understood material, but not one in which surface scientists took any special interest at the time. One feature of the study of surfaces was the tremendous amount of work generated by certain favorite, yet recalcitrant, reconstructions; and gold was not part of this canon. Surface scientists could bring their battery of experimental and theoretical machinery (their "alphabet soup" of different spectrometers, microscopes, and diffractometers) to bear on favorite reconstructions for years, providing material for generations of dissertations, without ever settling on a model. Some models were excluded in the process, but as theorists sharpened their skills they simply added more variations, all fitting the data.

Because of their bond structure, surface reconstructions are more common and dramatic in semiconductors than metals; most metals have a configuration at the

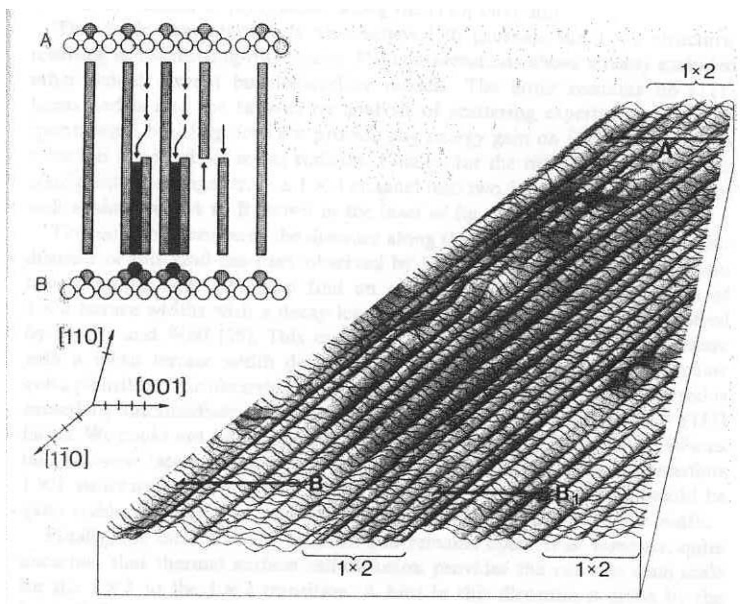

Figure 3-3: Gold (110). The first STM image purporting to show atomic resolution, along with a proposed model for the gold (110) 2x1 reconstruction. The image was made by off-setting line traces on a storage oscilloscope, then outputting to a chart recorder. Note the extra margin placed at the bottom to make it look as though the image shows the surface at a slight angle to the viewer (my thanks to Jochen Hennig for pointing this out). This image is remarkable for being unremarkable; though surface scientists knew of it, few were interested and none were roused by it to replicate the STM. From Binnig, et al. (1983a).

surface that is more or less a cut off version of the bulk structure. Thus, surface reconstruction work tended to concentrate in the big corporate research labs – Bell Labs, IBM, Xerox – where semiconductors were of more interest, and where the computing power to solve them was easily available. For researchers interested in metals, reconstructions were a low priority, although the few reconstructions that had been "solved" by the early '80s were mostly metals, since the reconstructions were simpler. Nevertheless, a few metals remained, including the gold (110) 1x2. Thus, Binnig and Rohrer's surface science colleagues initially recommended they look at this reconstruction. Yet because the gold 1x2 was not the center of any ongoing debate, their results attracted virtually no attention.

Much more dramatic than gold, though, was a reconstruction known as the silicon (111) 7x7. This was one of the first reconstructions to have become familiar to surface science in 1950s, when the field was first starting to coalesce and when the phenomenon of reconstruction first attracted attention. It's difficult to pinpoint exactly why the 7x7 became so central to researchers in this area. True, silicon is a technologically important material. Yet the (111) is not the cut of silicon used in making integrated circuits (ICs are made with (100), usually in the 2x1 reconstruction). Rather, the 7x7 became the "fruit fly of surface science" [RT1, 2/23/01] because it allowed for a *variety* of *ongoing* work and debate in spectroscopy, diffractometry, and theory – there were many stakeholders in the 7x7, and their stake continued over time.

Instrumentation and specimen preparation were key to the 7x7's status as an epistemic thing. Most reconstructions have small unit cells – 2x1, √3x√3, 2√3x√3, perhaps as large as 2x8 – i.e., two or three times the size of the bulk unit cell. The 7x7 was 49 times the size of the bulk unit cell, and dwarfed almost all other reconstructions. Moreover, it was clear that however the 7x7's atoms were positioned in the unit cell, their organization was exceedingly complex. This complexity yielded a low energy electron diffraction pattern of tremendous delicacy, even beauty [JG2, 2/20/01], compared to LEED patterns of other reconstructions. Since LEED was the key instrument in solving reconstructions, and since it was the primary instrument in determining what reconstructions are present on a crystalline surface, the importance of the 7x7's LEED pattern should not be underestimated. Not only did it present the most intriguing problem known to LEED practitioners, but it was also one of the most familiar and easily recognizable – the complexity of the 7x7 meant that its LEED pattern could be identified almost instantly.

This, in turn, meant that methods and recipes for preparing samples of the 7x7 were much more finely developed than for other reconstructions. There were, by 1980, many different recipes for preparing the 7x7, and learning a few of them was one of the first steps in training students in specimen preparation methods. The recognizability of the 7x7 also offered a way to train up instrumentation – LEED manufacturers, for instance, usually included a 7x7 sample with their devices so that customers could quickly check that they had purchased a working system [CD1, 10/30/03; FH1, 5/9/01]. Similar checks of 7x7 samples could be used over time to make sure LEED instrumentation was being maintained properly. This made the 7x7 attractive in developing other kinds of instrumentation. Since LEED could check very quickly that a sample was a good, clean specimen of the 7x7, that sample could then be treated as a "well-defined," known system to characterize via other diffraction or spectroscopic techniques. Thus, through the '70s, LEED and the 7x7 started to be used more in conjunction with many new types of instrumentation.

In turning to the 7x7, Binnig and Rohrer were treading a path that other kinds of surface scientific instrumentation had been down before. As it turned out, the 7x7 was fortuitous in taking advantage of the STM's capabilities; but Binnig and Rohrer were also extremely skillful in making the most of their successes. Getting to that point, though, was difficult. None of the STMers had experience with or knowledge of surface science; they knew nothing about how to prepare the 7x7, what they should be using the STM to look for, or how to make their results credible to the surface scientists. Received truths surface scientists would never have questioned were unknown to the STMers. For instance, where surface scientists insisted on preparing and characterizing a sample in the same vacuum chamber, Binnig's STM design did not accommodate sample prep technology. Instead, he and Gerber prepared samples in one chamber, then took them out of vacuum and walked them over by hand (in air) to the STM. Later, when Binnig and Gerber got images from samples prepared in this way, they took it as a point in favor of their maverick, outsider sensibility – if you could see something on samples that had been exposed to air, then the surface scientists' fetish for cleanliness was worth challenging [GB1, 9/26/00; CG1, 11/12/01]. For surface scientists, though, the long period when Binnig and Gerber could *not* generate images from such samples was an indicator they were doing something wrong.

Similarly, the STMers had trouble learning surface scientists' recipes for sample prep, yet they eventually turned their difficulties into a vindication of the ethic of naïveté. After months of trying to prepare a 7x7 and finding only "rolling hills" and "gunk" indicative of low-resolution STM images, they began to lose faith. But a final consultation with a surface scientist, Franz Himpsel, who was visiting Zurich from Yorktown, put them onto the right track toward producing more amenable samples.

> I was at IBM at Yorktown Heights when at IBM Zurich the STM was invented, so I ... was involved a little bit in helping them. Just before they got ... the 7x7 silicon surface data, I was at Zurich ... for a talk.... They were looking for some good science demo that they could use the STM for and they had tried already that silicon surface because that's a real classic. The fact that it has a very large unit cell and a complicated structure was known for 25 years or so at the time but nobody knew the structure, and people have been making

> models for years and years. So they thought that would be a good test case to show how powerful their new method was. But they were still having problems cleaning their samples, this business of getting a real clean, well-ordered surface. I'd been working on that problem separately for my photoemission so I gave them a few hints and they managed to then get some of their data. [FH1, 5/9/01]

With Himpsel's help in sample preparation, and the tacit knowledge Binnig and Gerber accumulated in STM operation, circumstances become favorable for the team to generate ultrahigh resolution images of the 7x7. They were aided in this by the 7x7's very large unit cell large, which ensured that any feature within the cell appeared regularly in an STM image at a relatively large repeat distance. Binnig was still the only person who believed the STM could achieve *atomic* resolution, but back of the envelope calculations of its resolution based on the radius of curvature of the tip showed it should be able to see features at scales close to the repeat distance of the 7x7 unit cell. As it turned out, the 7x7 unit cell contained one very obvious feature per unit cell that could be seen easily even without atomic resolution – a large hole in the middle of a ring of adatoms (see Figure 3-4). Thus, with practice, Binnig could coax the STM to where he could see areas with a regularly repeating depression at a scale close to that calculated for the 7x7 unit cell.

Along with their ambiguous resolution of the gold 1x2, "seeing" this "corner hole" on the silicon 7x7 – however indistinctly – was a turning point. Binnig and Gerber now had a feature they could use to fine-tune the STM down to even higher resolution. After this, Binnig became engrossed by the instrument, spending late nights and developing an extreme proficiency at subtly controlling its operation. Interpretation of images also required subtle skills. Since the STM was not generating *images* as such, Binnig and Gerber had to use every cue they could to understand its output [GB1, 9/26/00; CG1, 11/12/01]. By watching the oscilloscope electronically trace out the tunnel current, and also *listening* to the chart recorder that generated hard copies of the line traces, they became proficient at recognizing patterns that recurred

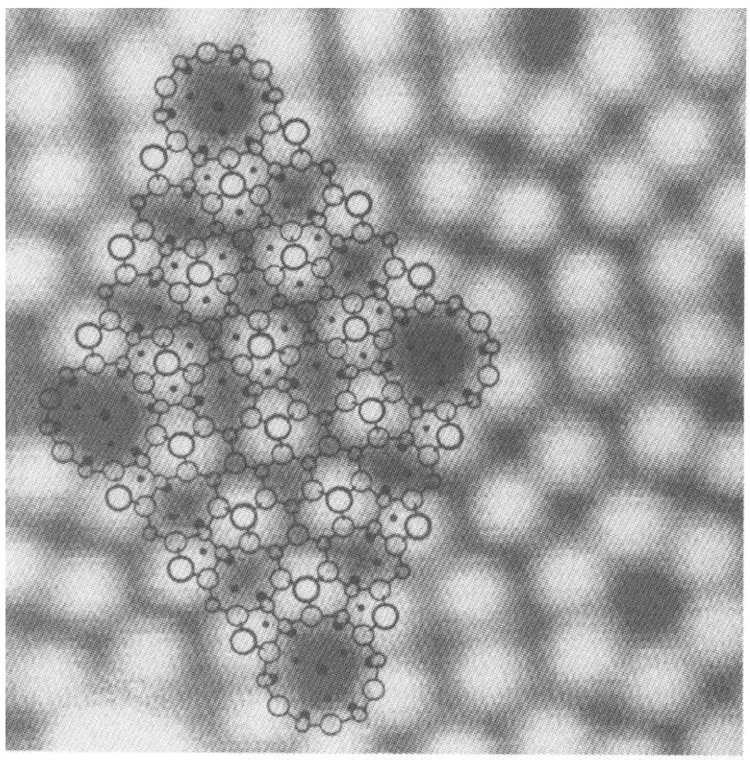

Figure 3-4: 7x7 corner hole. The silicon (111) 7x7 unit cell is extraordinarily large and has a very complex structure. One feature of that structure that aided early STMers is a distinctive "corner hole." This figure shows a model of the 7x7 unit cell (the so-called dimer-adatom-stacking fault model) overlaid on an STM image. The bright white spots are adatoms sitting on top of the surface; the large black spots surrounded by a circle of six adatoms is the corner hole – a deep depression at the vertices of the unit cell. Early STMers could see this depression even when they could not resolve the individual atoms of the 7x7. From Demuth (1988).

in a series of line traces (showing that the traces were registering some repeating "signal" rather than just random "noise"); and by becoming familiar with the sights and sounds of such traces over time, they could tell that the STM was registering very similar patterns on the 7x7 from day to day. As they describe it now, they probably "saw" the atoms of the 7x7 many times this way, but it took them a long time to develop the familiarity and confidence to find a good set of line traces, turn them into a publishable image, and present it to their surface science colleagues.

So, one day when they heard the chuk-chuk-chuk of the chart recorder scribing out regular, ordered line traces, they saved, collated, and handed them to Gerber to turn into an image. Gerber pasted each chart recorder strip to a piece of cardboard, cut out the trace, and glued all the pieces of cardboard together in order to form a *three*-dimensional, solid representation of what the STM had "seen." Hand-made models of this sort have a long history in science, of course, but are rare in microscopy. Surface scientists were used to ball-and-stick molecular models, and most still have such kits in their offices, often arranged to represent popular surfaces like the 7x7. The hand-crafted sculpture of the 7x7, though, was outside their ken.[14]

Surface scientists, despite the name, are interested not just in the top layer of atoms at a surface, but in all the layers that make up the transition between interface and bulk; this can extend 5, 6, even 10 or 20 layers deep. Moreover, when surface scientists talk about the *structure* of reconstructions, they mean the *electronic* and bond structure of the reconstruction as well as the placement of atoms within the unit cell. The STM "image" of the 7x7, on the other hand, only showed the very topmost layer of atoms, and told nothing about bonds or electronic structure. For some surface scientists, therefore, the Zurich team's style of presentation engendered confusion and even hostile skepticism. Audience members at their presentations would get up and

[14] For an excellent article on models and representational conventions, see Francoeur (1997).

leave or ask pointed questions about the credibility of their images [GB1, 9/26/00; HR1, 11/13/01; CG1, 11/12/01; JG3, 2/28/01]. A few times, well-known researchers visiting visiting Zurich came into Binnig and Rohrer's lab and attacked the 7x7 "image" as fraudulent. Some surface scientists claimed, for instance, that Binnig and Rohrer had simply labeled the axes of their images so they would appear to have atomic resolution, rather than ascertaining that features within the images actually corresponded to atoms; others charged that the Zurich team was trying to pass off computer *simulations* of the 7x7 as images obtained from a microscope.

In fact, such outright skepticism did not last long, and no surface scientist developed a critique of the STM or Binnig and Rohrer's 7x7 results that was technical enough to make it into print. When questioned directly about their 7x7 image, Binnig and Rohrer based their reply on aesthetic considerations. On the one hand, they reasoned that the complexity of the pattern in their image could not be artifactual; only the unit cell of the 7x7 could have such an intricate arrangement of its constituents, and therefore it was reasonable to conclude that the STM was achieving atomic resolution. On the other hand, they constructed their three-dimensional cardboard representation in such a way as to defuse the notion that it was a simulation – the image is so obviously homemade and messy that it bears little resemblance to something cooked up inside a computer. Controversy about the 7x7 faded quickly, particularly as Binnig and Rohrer and a variety of surface scientists sought ways to integrate the STM into the discipline. Yet a certain incommensurability of style between the Zurich team and some in the wider surface science community lingered on, much of it sparked by Binnig's willful naïveté about disciplinary conventions. The 7x7 image, for instance, simply did not accord well with how surface scientists thought visually about atomic structure. They were used to thinking in terms of three-dimensional ball-and-stick models or viewed-from-above maps (usually generated by theorists). Binnig and Rohrer's cardboard representation was more of a landscape or even a diorama than a map, and surface scientists found it difficult to integrate it with their theoretical understandings. It's notable that Binnig and Rohrer never again resorted to transforming line traces into sculpture in this way, nor did their replicators. Most later STMers, particularly in the corporate labs, quickly moved to computerized imaging which accorded much better with surface scientific modes of representation. Even for STMers who stuck with analog imaging, the standard way of publishing an STM image for the next decade was to take a Polaroid of the line traces on a storage oscilloscope.

So the Zurich team was clearly aiming for something novel and creative with their first public rendering of the 7x7. While it unnerved some of their audience, it did capture the imagination of many researchers – many more than had ever paid attention to the STM or the Topografiner before. There is something about the sculpted 7x7 that proclaims the new-found tangibility of atoms, and almost all early STMers speak breathlessly of the excitement of "seeing atoms" for the first time [RF1, 5/2/01; DE1, 10/11/01]. As they put it,

> [We went away and] wrote the paper on the 7x7. We returned convinced that this would attract the attention of our colleagues, even of those not involved with surface science. We helped by presenting both an unprocessed relief model assembled from the original recorder traces with scissors, Plexiglass and nails, and a processed top view; the former for credibility, the latter for analysis and discussion. It certainly did help, with the result that we practically stopped doing research for a while. We were inundated with requests for talks (Binnig and Rohrer 1987).

The STM, though, was not the first instrument with atomic resolution – the field-ion microscope had been generating images of atoms since the '50s, and souped-up transmission electron microscopes could do so in the '70s. The sculpted 7x7 demonstrated the novelty of the STM and the skill of its makers, but this hardly accounts for the tremendous significance of Binnig and Rohrer's first 7x7 article.

What does account for this significance, I think, is the way the Zurich team skillfully insinuated their image of the 7x7 into the context of an ongoing debate about that reconstruction within surface science – a debate that, fortuitously enough, was beginning to reach a crisis. Binnig and Rohrer headlined their first 7x7 article with the hand-crafted sculpture (the emblem of their maverick artisanry and ethic of naïveté), but they also coordinated that image with inscriptions (Latour 1988a) that credentialed their work as making a significant, disciplined contribution to surface science. By enrolling their local surface science network into the project, they learned what features of the 7x7 question were under discussion in the surface science community, and what questions their image could help answer. Thus, in their first 7x7 article, they carefully interpreted their image for a surface science audience, transforming it into the abstracted, bird's-eye representations (see Figure 3-5) favored by surface scientists, and then pointing to salient features that could contribute to the 7x7 debate.

In doing so, Binnig and Rohrer elicited a curious but highly effective audience response. Demonstrating the relevance of their image to open questions about the 7x7 allowed surface scientists to take the Zurich team seriously. On the one hand, surface scientists now looked at the STM as an instrument that could say something to surface science and should be replicated. On the other hand, the Binnig and Rohrer article still presented significant gaps in interpretation and by no means resolved the 7x7's issues [CD1, 10/30/03; FH1, 5/9/01]. Yet, because the STMers could be taken seriously, surface scientists welcomed the gaps as an opportunity for them to appropriate and remold the STM. Even if the STM was only an adjunct in solving the 7x7, many surface scientists took it to be revolutionary because it provided atomic-scale, real-space images of materials central to the practice of the field. As noted, both the FIM and the TEM could give atomic resolution under special circumstances, but the materials they could image were seen as too marginal to surface science to be really

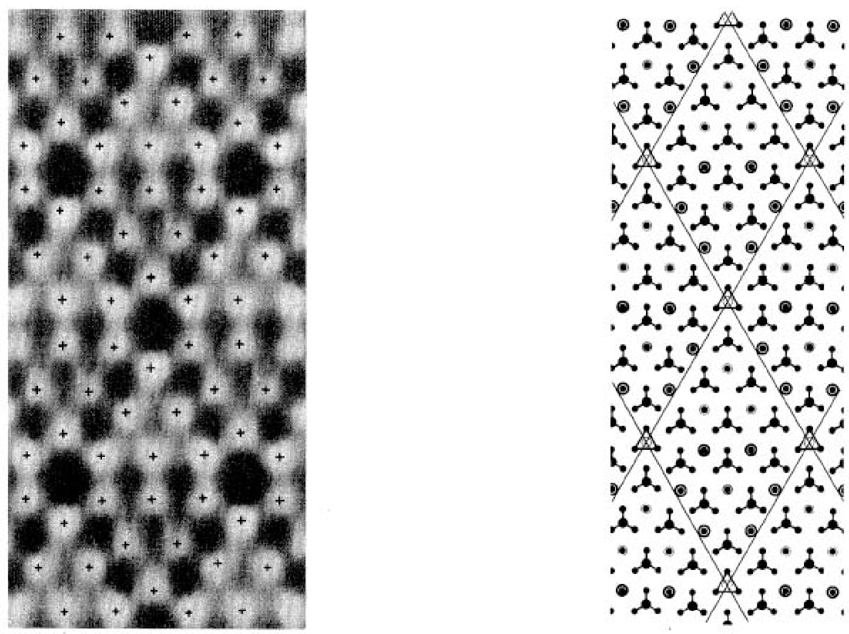

Figure 3-5: Bird's-eye view of the 7x7. In their first article on the 7x7, Binnig and Rohrer tried to show surface scientists that they could make STM data about surface reconstructions speak to the discipline's conventional ways of thinking about structure models. They did this partly by pairing a bird's-eye version (left) of the same data presented earlier as a three-dimensional sculpture (see Figure 3-2) with a proposed model for the structure of the 7x7 (right). From Binnig, et al. (1983b).

interesting; whereas the STM had demonstrated its applicability on one of the jewels in the surface scientific crown. Surface scientists began to see it as a radical shortcut through the laborious, inconclusive morass of LEED patterns and structure calculations. Indeed, in the long run the rise of STM coincided with a loss of prestige both for LEED and for the whole game of solving reconstructions (Lagally 2003).

Encouraging Replication, Averting Resistance

Binnig and Rohrer foresaw some consequences of the success of STM, and, though they had little love for FIM or LEED, they wanted to avert the view of STM as an unwelcome substitute for more traditional techniques. Thus, as Arne Hessenbruch points out, Binnig and Rohrer worked to show that, in application and resolution, STM complemented and only marginally overlapped, rather than subsumed and replaced, SEM, TEM, FIM, and light microscopy (Binnig and Rohrer 1982). To the extent this strategy was meant to forestall *resistance*, it seems to have largely succeeded. Though early on some FIMmers and LEED people opposed the STM publicly and harshly, after the publication of the 7x7 this hostility waned. If, on the other hand, Binnig and Rohrer were trying to make the point that practitioners of other techniques should join the STM bandwagon, they largely failed. FIMmers never cottoned to STM, and well-known LEED people such as Franz Himpsel or Max Lagally did not take it up for almost a decade; early on, surface scientists who were already established in some other technique allowed a younger generation of postdocs and newcomers to replicate and adapt the STM for use in their discipline. Electron microscopists, too, largely ignored STM, even though Binnig and Rohrer clearly expected them to be the first wave of researchers to attempt replication.

The group of replicators that did coalesce around Binnig and Rohrer was a more eclectic lot who often had experience building other kinds of instruments but felt ready to switch subfields. Most had some local link to the Zurich lab, often European academics with long-standing ties to IBM whose students would get jobs at Zurich, or who spent sabbaticals there [TB1, 11/19/01; DK1, 11/15/01; HG2, 11/8/01]. Rohrer was the most successful recruiter of early European replicators. As a senior manager at the lab he could shape research within the institution and attract other IBM researchers at Zurich to the instrument; thus, people like Dieter Pohl, Urs Dürig, Jim Gimzewski, Giorgio Travaglini, S.F. Alavarado, Bruno Michel, and others started to form a substantial STM community within IBM Zurich. By all accounts, Rohrer was the most expansive and personable personality in the original three-man group, and his openness and friendliness both attracted newcomers and set the tone for how the nascent community of STMers would be organized. As one very early academic STMer (whom we will meet again in Chapter Five) puts it:

> We all know of Gerd Binnig's genius for building instruments, though I think Heini Rohrer's profound contributions to the field have been underestimated. From the beginning he was the most open, sharing guy.... He always wanted to help others, accepting their ideas while encouraging them to explore new areas and to try different things. He would have been the natural person to set himself up as the leader of microscopy, who defined what were good projects and what were bad projects and who was doing good work and who was doing questionable work, but he was never like that. [PH1, 3/19/01]

Interestingly, Binnig and Rohrer looked to the history and social organization of other microscopy fields as a way to frame their own conception of the STM field – by, for example, taking electron microscopy's move into biology as a model, and treating the field-ion community as a straw man for how *not* to organize.

> We started to think a little broader. I started for instance operating the STM on DNA,... and we cooperated with people from other universities and completely different fields and we talked to chemists and we talked to biologists.... So we started very early to talk to very different disciplines.... All kinds of microscopes can be used in all kinds of fields, like optical or electron microscopes. Electron microscopy had its first big success in biology. So then it's obvious that you can use STM for other fields besides surface science. And we talked very early to these people, what would be interesting.... So we created a little bit of a culture [of openness] in this community already right from the beginning. Which was more open than, for instance, the community of field-emission or field-ionization people who tried to be closed. We tried to be open and bring things out instead of protecting. [GB1, 9/26/00]

Of course, Binnig and Rohrer's attempt to keep their community "open" and to minimize clashes with other instrumental communities accorded well with the ethic of naïveté. That is, they encouraged the growth of a community where *everything* would be treated as novel and exciting and a possible avenue for further discovery. Discouraging a culture of criticism and boundary maintenance – hallmarks, as they saw it, of the field emission and surface science communities – also averted potential priority disputes and questions about the STM's unrecognized antecedents.

> In the first years I don't know anybody who was not very open and didn't publish openly. Of course once an area develops then the competition aspect comes in. For a while it's the achievement aspect which is much much more important than the competition aspect. You want to achieve something, but that has nothing to do with the competition aspect, that's the scientific spirit of achievement. It is achievement for achievement's sake which advances science, not competition. Unfortunately, later you always have this competition coming in when you fight for recognition. But all these things were not I would say in the heads of all these guys at the beginning.... The best counterexample, that's the field ion microscopy and field emission. That stayed a closed community around Mueller.... That's why it never really spread. But the STM I think that was one of our merits, we kept it open. [HR1, 11/13/01]

Binnig and Rohrer worked hard in the early years to lower barriers to entry for newcomers – though, in the climate of corporate research, there were some newcomers (especially at Bell Labs) with whom they had to be circumspect. Building an experimental culture in which newcomers would neither be turned away nor criticized harshly, however, was not enough to attract replicators. To do this, Binnig, Rohrer, and Gerber developed a three-pronged strategy. First, they continued developing the technology, making it more flexible, easier to use, cheaper, etc. Second, they began demonstrating the STM's relevance across a variety of applications, training it on specimens of interest to a diverse cross-section of disciplines. Finally, they started to investigate and explain more carefully how the instrument worked and what its results meant. Once again, they relied on their group's division of labor, skill, knowledge, and style. For instance, routine instrument development (engineering work that Binnig did *not* find novel and exciting) often fell to Gerber and other auxiliaries. It's easy to see why Binnig shied away – polishing down the STM's rough edges generally did not, *per se*, generate new discoveries or clarify the instrument's attributes, but it did make it less of a virtuosic challenge to operate the microscope. Thus, this kind of STM innovation played against both the ethic of naïveté and Binnig's construction of the STM as a site for maverick artisanry.

Interestingly, instrument development became a way to induct younger members into the STM team. A Swiss undergraduate named Othmar Marti, for instance, worked summers at IBM Zurich, sharing a lab with Binnig and building circuits for the STMers [OM1, 11/16/01]. Eventually Rohrer arranged for Marti to seek a graduate degree under Rohrer's old thesis adviser; for his dissertation Marti built one of the first low-temperature STMs, doing most of his experimental work at IBM. Marti's significance, though, extended beyond his low-temperature design; his dissertation (Marti 1986) circulated widely and became a kind of handbook for many early STMers. In their articles, Binnig and Rohrer thinly referenced literature that could be seen as preceding and relevant to the STM; moreover, their descriptions of how the instrument actually worked lacked much detail that might have been useful to a replicator. Marti's dissertation, on the other hand, included a long discussion of the history of tunneling, a thorough examination of STM theory, and explanation of practical details such as feedback circuitry, vibration isolation, and tip preparation.

Marti also had expertise in electronics and software, an area the STMers struggled with. In building his low-temperature instrument, he often put together complicated, problem-specific circuits and electronics modules that he shared with Binnig and Gerber. Eventually, he was asked to develop a more or less standardized package of modules to circulate among the STM groups at Zurich. This package (the "Blue Box") was the first attempt to automate the tip-sample approach and make life easier for the operator. Moreover, the Blue Box could interface the STM to a computer for the first time. Until the Blue Box, "data analysis" could really only be done by Binnig and Gerber, since they were the only people with enough experience to develop a sense for what squiggles on the oscilloscope meant. Now, data could be stored in a computer and made portable; "seeing" an STM image could become a routine, non-artisanal skill.

Digital storage encouraged the Zurich team to develop digital methods for presenting and analyzing images. Again, this task fell not to the computer-averse Binnig, but to a junior researcher named Erich Stoll. Stoll worked with both theorists and experimentalists at Zurich, developing simulations based on the theorists' models and processing algorithms to enhance the experimentalists' images (Stoll and Marti 1987; Stoll and Baratoff 1988). Interestingly, this could be seen as pushing Binnig and Rohrer closer to the corporate center – after all, computing was what IBM did best, and groups at IBM Yorktown were working on similar ways of controlling the STM and processing its images. Instead, the reception of Stoll's work indicated some unease about the Zurich style among American IBMers. Some at Yorktown believed Stoll was over-processing images, filtering to produce spurious high resolution in a way that could, they worried, damage the credibility of the whole STM community.

> [At the first big STM conference in 1985] the Zurich group ... had this one guy [Stoll] and he started doing some pretty heavy image processing, taking data that was really borderline and massaging it to get something out of it.... That was not very well received. Obviously that had not been done in the early Zurich data, and everybody knew that the data was just fine. Nevertheless, there were a few outsiders, totally unfamiliar with the field, that had expressed skepticism about the whole thing, said it was all made up.... I mean, [Stoll's] results were probably fine, but that's what happens – they had nice data on all sorts of things but at some point of course you're so busy getting Nobel Prizes you stop taking data and then somebody ends up analyzing the stuff that normally you would have set aside and re-acquired with greater quality. [RF1, 5/2/01]

Interestingly, most of the post-7x7 Zurich innovations that were taken up by Yorktowners were developed by Gerber. In particular, Gerber's answer to the bedeviling issue of vibration isolation became a widespread modification. Gerber was entrusted with modifying portions of the Zurich STM from generation to generation, adapting it for specific projects. Often this meant modifying the microscope to make it more relevant to some technical subculture – surface scientists, electrochemists, tunneling experts, etc. One modification centered on getting electron microscopists to take notice. Gerber made an STM small enough to fit *inside* the chamber of an electron microscope. This would allow the same sample to be imaged almost simultaneously by the two instruments, over a much wider range of resolutions than either could provide on its own (Gerber, et al. 1986). This would appeal both to metrologists and to people interested in looking at a sample at low resolution and then swooping down on one or two spots (a typical practice in biological microscopy).

In making the instrument small, though, Gerber stumbled on something of much wider significance. Remember that the first few generations of STMs had large, bulky, complicated, esoteric vibration isolation systems. Suddenly, Gerber found all these were more or less unnecessary – simply by making the microscope small, one could cut out a very large range of vibrations. The mechanics of this are fairly simple – think of how a one-story house behaves in an earthquake or a high wind relative to a twelve-story construction. The taller building must be built much more massively than the smaller one in order not to topple; if they were built the same way, the taller building would sway more widely and unpredictably than the smaller building. The "mechanical path" from the ground to the top of the edifice is much longer for the tall building than the small house. In an STM, the mechanical path is the distance from the sample through the sample holder, the microscope base, the scanner, and the tip. Gerber shrank this path by an order of magnitude, giving the tip less play with respect to the sample. This obviated the high frequency vibrations impinging on the STM. To deal with the low frequency vibrations, meanwhile, Gerber found a UHV-compatible rubber called Viton that could be set in layers underneath the scanner.

This "pocket STM" (see Figure 3-6) with its compact base and scanner, an inch or two on each side, and three or four layers of Viton and metal sandwiched underneath, caught on like wildfire. People who were already in the field quickly understood the value of a small mechanical path, and raced to build smaller microscopes [GB1, 9/26/00; CG1, 11/12/01]. One European academic, Karl Besocke, for instance, developed the "matchbox STM" and would go around conferences showing his tunneling microscope inside a matchbox (Besocke 1987). Others, such as Cal Quate, looked forward to etching STMs directly into silicon wafers – the "STM on a chip" detailed in an NSF grant entitled "Microfabrication of the Scanning Tunneling Microscope." Within IBM, small microscopes with Viton spacers became the semi-official look of Big Blue's STMs – with both Zurich and Yorktown producing batches of such microscopes for internal use, the design spread quickly. Moreover, as new people came to STM they were introduced to the field through designs for the pocket STM – thus, the look of Gerber's instrument began to crop up in very distant places.

After the 7x7

I want to close by looking at where each member of the Zurich team went and what they did after the 7x7 – both because their contributions in the wake of the 7x7 were significant, but also because this phase of their work tells us a great deal about their interpretation of the STM and their construction of scientific personae in an expanding technical community. Discovering the 7x7 is remembered as a momentous event personally for Binnig, Rohrer, and Gerber. Its reception radically changed what it meant to be an STMer. In a relatively short span, their fringe, hidden side bet

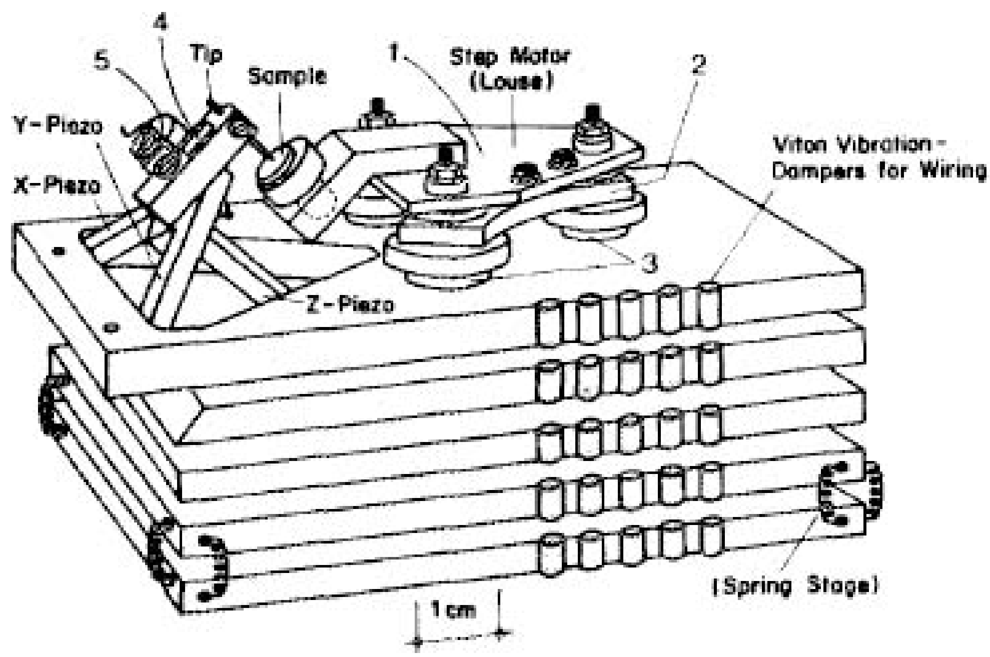

Figure 3-6: The pocket STM. This very compact version of the STM, with its distinctive layering of metal plates and Viton spacers was developed by Christoph Gerber and spread rapidly through the STM community because of its improved vibration isolation capability. From Gerber, et al. (1986).

became the center of attention and praise. Being the inventors of the STM, far from a somewhat irrational career choice, was now the grounds for fame and glory.

In the immediate post-7x7 period, Rohrer became an ambassador, smoothing the reception of the microscope into new communities, offering advice to STMers who sought it, contacting people whom he thought should take up the instrument, and managing some of the STM groups at Zurich. “Elder statesman of STM” was an easy role for Rohrer to fill as a senior researcher and manager, and his avuncular personality set the tone of the community. Others looked to him to fill this role as well – Cal Quate, for instance, asked him to represent IBM Zurich at the first informal STM workshop in 1984, in which a small group of replicators sought his guidance; and IBM Europe asked him to organize the first big STM conference in 1985. When prominent STMers today discuss the modern version of the STM conference, they speak of Rohrer’s original vision of the conferences with the respect and admiration due a founder of the field [PH1, 3/19/01].

Gerber, meanwhile, continued work on projects related to microscope design. In the ’80s, Gerber’s work tended to be tied to Binnig’s; when Binnig’s wanderlust took him on sabbaticals, Gerber was usually posted to the same place. Later, when Binnig drifted away from probe microscopy, Gerber took on a more leading role, eventually splitting his time between Zurich and H.-J. Güntherodt’s group at Basel, while managing his own effort in AFM-based chemical sensing. Of the three original STMers, Gerber seems to have been the most steadfastly committed to probe microscopy, and the most willing to stake his career and his technical identity (Haring 2002) on it. Indeed, in conversation Gerber evinces much more fascination with, and willingness to pitch, the technology than either Binnig or Rohrer [CG1, 11/12/01].

Where Gerber has attached himself to probe microscopy, Binnig, until the late ’90s, gradually drifted away from the field he started. Binnig’s career is characterized by a series of transitions into new fields, and he has oriented himself more to starting new things than to steadily plying a routinized technique. This exemplifies the scientific persona he has cultivated. Boredom and impatience with familiar tasks were central components of the ethic of naïveté; by definition, a technique that had become routine was one about which too much was *known*, and hence no longer a site for untutored creativity. Thus, after the 7x7, Binnig only briefly continued looking at samples of interest to surface scientists, and the little surface science work he did was framed more as a suggestion for what other researchers could do than as a rigorous investigation of a particular material. For instance, experience with the vagaries of the STM taught him that different tunneling voltages yielded very different images; with a few simple experiments he demonstrated that this fact could be used to turn the STM into a powerful spectroscopy tool to investigate how tunneling over a range of voltages could illuminate the bond structure of the surface. As we will see in Chapter Four, though, he left the work of rigorously transforming the STM to others.

> [Yorktowners and Almadeners] came here and visited us to learn about this technique. We were the first who started with this STS [spectroscopy] technique but actually I have to admit ... the Yorktowners and people from Bell Labs did it much nicer.... I never published our spectroscopy results very carefully – just in a conference proceedings.... It was a more or less preliminary result and ... they both knew about these results because they were present at the conference – they probably were stimulated a little by this, but then they did an excellent job in solving real problems. We just did something to see whether it works. [GB1, 9/26/00]

Once STM broke into surface science, therefore, Binnig let more disciplined practitioners adapt, transform, and promote it. He began instead to look for new collaborators and networks through which STM could propagate.

The first of these was in electrochemistry [DK1, 11/15/01]. Like surface scientists, electrochemists are interested in how physical forces and chemical reactions change the structure and electronic characteristics of metal surfaces; in fact, some of the surfaces the Zurich team had already looked at – especially gold – were canonical electrochemical materials. Also, like surface scientists, electrochemists were used to approaching surfaces through indirect means (by using current-voltage curves to describe how changes in the voltage of an electrochemical cell affected the microstructure of the surface). Thus, when Binnig found electrochemists to talk to he analogized their field as "underwater surface science" and made the first steps toward incorporating the STM into their practice (though, again, the real work of making the STM an electrochemical tool would be left to others) (Gomez, et al. 1986).

Next Binnig became interested in biology, and an extensive bio-STM effort began at Zurich, first in collaboration with a visiting Spanish physicist, Niko Garcia, later in Giorgio Travaglini's group under Rohrer's supervision, and finally several years later in Binnig's own group at Munich. Clearly, bio-STM offered wide scope for the ethic of naïveté – no one in the original Zurich team knew anything about preparing biological samples or interpreting STM images of those samples. Moreover, the turn to biology was inspired by two historical analogies. On the one hand, the twentieth century saw any number of gifted, maverick physicists take to biology – Schrödinger, Feynman, Gamow, etc. On the other hand, taking a new microscope and adapting it to look at biological samples was a well-trodden path both for optical and electron microscopy, and the biology community made those instruments much more successful than physicists could have on their own.

Still, as novel as bio-STM was, it did not keep Binnig's attention forever. As we will see in Chapter Six, in 1990-2, bio-STM entered a phase of intense controversy that ended in the almost complete disavowal of STM as a biological tool. Binnig played an important part in this debate, but his taste for dispute, and for STM, diminished significantly. Even before then, in the period after the 7x7, his duties as inventor of a newsworthy instrument were beginning to pall on him; he traveled extensively through 1983-4, spreading news about the technique at conferences and visiting replicators to help them build their machines [CG1, 11/12/01; JM1, 3/15/01]. He also wrote widely about the instrument in a general tone as part of IBM's effort to help secure the Nobel Prize for him and Rohrer (an effort that succeeded in 1986). The almost inevitable result, given Binnig's self-presentation, was that he became even more disaffected with the technique and with the routine in Zurich.

When Cal Quate, one of the first North American replicators, invited Binnig to come to Stanford for 1985, therefore, the offer was hard to resist. Binnig agreed to take a sabbatical in Quate's lab while spending part of his time at IBM Almaden (outside San Jose), while Big Blue reassigned Gerber to Almaden for the year with the understanding that he would occasionally assist Binnig and Quate in Palo Alto. Gerber was assigned to bring the Almaden STMers up to speed, travel to academic labs to help other replicators, and tinker with the design to make the STM more reliable. Binnig's goals for the year were less clear-cut. Having become "tired" of the STM, he wanted to find some area where he could bring his experimental prowess (and the ethic of naïveté) to bear on an outstanding physical problem.

Initially, he lit on gravity wave physics as an area where conventional experimental solutions had proven uninformative and an untutored, maverick attempt might succeed. By the mid-'80s, gravity wave physicists were leaving behind the era of small, home-made detectors and focusing on large (kilometer-long) detectors built and managed by huge teams of researchers and engineers. Binnig saw an opening to use STM technology to counter this trend and turn gravity wave physics back into a "small science."[15] There were features of Quate's group that led Binnig to this topic – he envisioned using a low-temperature vibration sensor, similar to a low-temperature

[15] For gravity waves' vexed journey from small science to big science, see Collins (1975; 1992; 1999; 2003). Perhaps most interesting here is Collins (1998), with its discussion of "open" and "closed" communities. We can also see Binnig here as trying to short-circuit some of the imperatives of "big science" with a small-scale gravity wave detector; see also Galison (1985).

acoustic microscope Quate built in the ’70s – but it seems equally likely that, like vacuum tunneling or the 7x7, gravity waves were simply an appealing unsolved mystery. Interestingly, the foray into gravity waves also confirmed some of the problems with the ethic of naïveté:

> [With vibration isolation] we were very naïve. We first thought if you levitate something on top of a superconductor that’s the best thing.... But this was stupid.... We also had this eddy current damping then in the beginning. This worked very nicely. Then we had multistages.... Actually people from gravity waves, they knew things like multistages very well. We should have just studied their literature a little bit more carefully we would have learned a lot. But you see I’m not a guy who likes to read literature, so I didn’t do much in this respect. But I learned that later then when I studied the literature about, because I wanted to use the AFM for gravity wave detection.... I realized hmmm, these people, they already knew many of our vibration isolation tricks long before we developed them. So, we just could have taken those ideas from these people. [GB1, 9/26/00]

As it turned out, though, he spent relatively little time on the gravity wave detector, although he continued to play around with it until the early ’90s [GB1, 9/26/00; MK1, 10/12/01; FG1, 11/16/01].

Instead, he used his year in California to create the atomic force microscope (AFM), which, today, dominates the probe microscopy field. From the standpoint of the ethic of naïveté, one of the great failings of the STM was that it could only image conducting samples. For surface scientists, this was not much of a concern, since their materials of choice were metals and semiconductors; but for Binnig, whose experimental style privileged moving from one class of sample to another, this limitation was onerous. Thus, when he arrived in California, he intended to leave STM behind and move into gravity wave physics. The Quate group, though, had a long history of doing non-destructive microscopy (i.e. techniques where, unlike in electron microscopy, the sample is not destroyed in the process of looking at it) on all kinds of samples, and by 1985 were working hard to expand the capabilities of STM; in this atmosphere Binnig became more reluctant to leave microscopy behind.

Instead, he thought about ways to let STM image insulators and thus expand its range of imageable samples.

One solution was to coat the sample with metal and do STM on the metallic replica, a technique tried at Zurich and elsewhere, particularly for biological samples. Binnig saw, though, that a more non-destructive approach might be to scrape a small, flexible strip of metal over a surface and feed an STM back on the strip, rather than the surface itself (see Figure 1-3). If the spring constant of the strip were smaller than the force holding together the atoms of the sample, then little or no damage would be done to the sample during imaging. Where the STM's mode of feedback had been the exchange of tunneling electrons, the new microscope would depend on the strength of the atomic forces between tip and sample – hence, the "atomic force" microscope (Binnig, et al. 1986a). The invention of the AFM shared many characteristics with the STM. For instance, once again the ethic of naïveté clouded Binnig's awareness of a variety of relevant predecessors. Binnig happily cites one source within the STM community that pointed him toward the AFM – work by John Pethica at Cambridge using STM to study adhesion and interatomic forces – but other forebears (such as Urs Dürig's work on adhesive forces using STM at Zurich, or Clayton Teague's discovery of the role of tip-tip force interactions during his vacuum tunneling research) are less visible in his descriptions of the initial AFM work [GB1, 926/00; UD1, 11/12/01; CT1, 6/28/02]. Yet, clearly, given Quate's involvement in the non-destructive testing community and ties to semiconductor manufacturing, he must have been aware of, for example, surface profiling techniques that strongly resembled the AFM and which had been in existence since the '50s.

As with the STM, though, the ethic of naïveté allowed Binnig to find new perspectives on old problems. For instance, he identified more imaging modes, and ways to image less destructively with higher resolution, than could have been imagined by the surface profiler community – by thinking in terms of atomic forces as a feedback mechanism, Binnig showed that the AFM could operate both with the probe scraping the surface ("contact mode") and with it wiggling above the surface ("non-contact mode"). As before, though, the ethic of naïveté created problems in patenting Binnig's work. IBM had arranged that products of Binnig's sabbatical year at Stanford would be patented under Big Blue's aegis, so the papers for the AFM patent list Binnig, Gerber, and IBM as the patentees, even though the original paper announcing the AFM was authored by Binnig, Quate, and Gerber. Moreover, the patent neglects mention of technologies audiences might deem relevant such as profilers [MK1, 10/12/01; SM1, 3/13/01]. IBM has spent considerable money and energy trying to collect on the AFM patent, especially once the instruments began to be commercially produced. Other AFM patents have generated millions of dollars for holders such as the University of California system; yet the Binnig patents yielded very little. As with STM, AFM benefited Big Blue by generating prestige but not direct revenue; again, it proved difficult to reconcile Binnig's maverick role with the exigencies of corporate profit.

A similar problem dogged the other major innovation to come out of Binnig and Gerber's year in California. Recall that earlier designs had used three perpendicularly stacked piezo crystals to control the fine movement of the probe – one pointing in (and controlling) the x-direction, one for the y-direction, and one for the z-direction (height). Binnig and Gerber, in the course of helping both the Stanford and Almaden teams devise more rugged and reliable STMs and AFMs, saw that *one* stack of piezos could control fine motion in all three directions. The piezoelectric crystal, after all, could change shape in any direction so long as it experienced a voltage drop that pointed in the right way. By gluing electrodes pointing in all three directions, you could make a single crystal change shape any way you wanted. The old piezo stack design had bedeviled STMers because the piezos were thin and brittle (and hence easily broke off), and because the process of gluing them together (and keeping them glued) was laborious and unreliable. The new piezos (called a "tube scanner" because the piezo piece was usually a hollow cylinder) were thicker and hence less brittle, and it proved easier to glue electrodes onto one piezo than to glue three perpendicular piezos together. Again, as with the pocket STM, this innovation spread rapidly through the STM community, and the ease with which a tube scanner could be constructed induced many people to begin building STMs. Yet, as with the AFM, the naïveté that guided the invention of the tube scanner also made it impossible to patent – Binnig and Gerber's application was rejected on the grounds that it replicated a design from the '50s for a phonograph needle assembly.

These patent difficulties were symptomatic of a slippage between Binnig and IBM that widened in the years after he returned from California. Having received the Nobel Prize in 1986, he was a valuable commodity for a research organization that prided itself on its international reputation and clutch of prize-winning scientists. Yet Binnig's ennui with probe microscopy, and his self-consciously marginal style of science, made his superiors uneasy and left him dissatisfied with Zurich. Thus, in the late '80s IBM allowed him to set up "IBM Munich" – an outpost consisting of Binnig, Gerber (part-time), and a student and/or postdoc. With resources to do what he wanted, and no management to constrain him, this could well have been the high point of Binnig's career. Yet the teamwork that made him so successful in Zurich and Palo Alto was missing in Munich; and, though he proved adept at picking and training postdocs, his own vision lost focus. Thus, the IBM Munich crew first worked on the gravity wave detector, then bio-STM, and finally non-contact UHV AFM [WH1, 11/14/01; FG1, 11/16/01]. Binnig himself attended to these projects, but his own interests slid further away from probe microscopy until, in the early '90s, he turned to chaos theory and fractal mathematics. Binnig the experimental genius now became Binnig the programmer and, interestingly, Binnig the writer. Indeed, it is in Binnig's popular – yet strangely opaque – writings on chaos theory and creativity that we find some of his clearest expressions of the ethic of naïveté. In fact, he opens with a statement that sums up this attitude, and which his closest colleagues and collaborators often cite as exemplary of his experimental style:

> I admit, it is really presumptuous of me to write about the theme of creativity: I have until now never engaged scientifically with this theme, nor have I studied the literature on it. But possibly this is just the right method to go at a relatively new theme (Binnig 1989a).

One other piece of Binnig's analysis of creativity nicely rounds out our story. For Binnig, creativity is a fractal phenomenon. That is, each new element is built from smaller ones, and each new piece of creativity is synthesized from smaller bits of creativity in particular domains. Moreover, creativity is an evolutionary process – complexity increases as growth unfolds over time in a random (yet constrained) process. Thus, in Binnig view, *replication* is fundamental to creativity. It is only in reproducing an idea or an element that new bits of complexity creep in.

In the next two chapters, we will see how creativity and replication came about for the STM and the AFM. Interestingly, these two chapters reproduce the tensions surrounding the ethic of naïveté, but on a much larger scale. At the corporate labs of Chapter Four, there was little place for Binnig's path of naïveté and creative failure. Instead, the values of experiment were centered more on personal and instrumental *success*, and on the positive accumulation of new knowledge and techniques within a disciplined framework. Creativity was still present, but in a more competitive, more scrutinized form. In Chapter Five, we again see something like the ethic of naïveté, for Binnig's way of building microscopes accorded well with the experimental culture of academic STMers in California. Binnig himself spent time with people like Cal

Quate, and reproduced in California the innovative drive that fueled the invention of the STM in Zurich. Over the long run, though, Binnig's style of work proved unsustainable, both for him and his followers. The STM came into being in a remarkable shower of naïveté and optimism; but it survived and prospered by opportunistically tacking back and forth between naïveté and more disciplined skepticism.

Chapter Four

American Corporate Labs and Replication of the STM

It seems likely that the STM could have been invented in many places. It was, in some sense, already invented at the National Bureau of Standards before its appearance in Zurich. Binnig, Rohrer, and IBM, however, spent much time and energy ensuring that it was seen as a product of Big Blue research. This meant that when it eventually crossed the Atlantic it became enmeshed in the world of the American corporate research laboratories. Indeed, integration of the STM into North American corporate research was a natural result of IBM's desire to have the new instrument recognized as a newsworthy scientific achievement. This chapter examines the introduction of STM into the big American research laboratories.[1] Two of them dominated the STM community for much of the 1980s – IBM Yorktown Heights in the Hudson Valley region, and Bell Labs in rural/suburban Murray Hill, New Jersey – but other corporate and national labs also made significant contributions: IBM Almaden, in the hills outside San Jose; Philips Research, on the bluffs of the Hudson near Sleepy Hollow; Ford Research, outside Detroit; Lawrence Berkeley National Labs, in the Berkeley hills; and the National Institute for Standards and Technology (formerly the NBS) on a woody site in suburban Maryland.

These picturesque environs might not seem germane to the story of the STM, but, as Ross Bassett and Bill Leslie have both shown, the big postwar corporate (and national) research labs were purposefully placed in semi-remote locales as a way to set

[1] The early years of the big corporate labs (particularly GE, Westinghouse, and Bell) have been well-documented. See Wise (1985); Reich (1985); Kline (1992); Reich (1983); Hoddeson (1981); and Wise (1996). The postwar labs have been written about less. Interestingly, the now somewhat dated movement in S&TS to examine postwar "big science" largely by-passed the big corporate labs. See the contents of Galison and Hevly (1992). There are, I think, important parallels between "big" postwar corporate research and "big" postwar government-sponsored programs.

off and highlight the importance of the work done there, while insulating researchers from the distractions of the urban and commercial world (Bassett 2002; Knowles and Leslie 2001; Leslie 2001).[2] This had the perhaps unintended consequence of shielding scientists from their own companies, leading to problematic disjunctures between research, development, and manufacturing. IBM and AT&T were so large, and had such powerful grips on their markets, that they tended toward involuted, idiosyncratic solutions to sociotechnical problems, rather than solutions that might be useful outside the local setting. We will see here how the institutional and locational seclusion of surface science at Bell and IBM encouraged a particular style of STM, and trace the development of that style up to the recession of the early 1990s, when the corporate research world turned upside down.

The focus of this chapter will be the people in these labs who first replicated the Zurich work, then dramatically expanded the capabilities of the STM and integrated tunneling microscopy into surface science. Crucially, most of these people were in the early phases of their careers – many were newly-minted Ph.D.s taking up their first positions as postdocs or staff scientists. Molding the STM to the discipline of surface science, while becoming disciplined corporate researchers themselves, was their way of navigating an often harsh institutional environment. Three characteristics of the early corporate STMers' positions within these labs most influenced their experience, and the design and use of their microscopes: time, competition, and community. Time, as Sharon Traweek has pointed out (Traweek 1988), is not on the side of postdocs and early-career researchers – they must accomplish a great deal in

[2] Another take on postwar "suburbanization" of science is Kaiser (forthcoming-c). Seclusion is a major theme in several historical studies of the location of science. See Hannaway (1986); Browne (1998); Shapin (1988). One of the most relevant here is Simon Schaffer's (1998) analysis of science in Victorian country homes. It would quite fair to characterize IBM's research facilities as Thomas Watson, Jr.'s "country homes" for science; indeed, one lab (in Vermont) was chosen for its proximity to his ski chalet.

the 12 or 24 months before their positions are reviewed. Success building an STM and publishing several articles in reputable journals within that time allowed postdocs to continue on as full-fledged scientists. Often, in the semi-closed economy of surface science, postdocs at IBM stayed on at Big Blue or migrated to Bell Labs; an equal number flowed the other way, from Murray Hill to Yorktown or Almaden. A few went to the national labs, especially the National Institute of Standards and Technology (where the surface science legacy of the Bureau of Standards was still strong) or academic posts (an option exercised more frequently as corporate research shrank in the '90s.

Thus, corporate STMers milked every last bit of time for valuable results, as evidenced in this former Bell and IBM postdoc's reminiscences:

> I went and worked for Don [Eigler at IBM Almaden] in the spring of 1988 .... and spent exactly a year to the second. Half the time that I was there we spent waiting for this one translator to be fixed; we sent it out four times for repair. Then, when things were working we had this rule – never leave a working microscope. So we would just crank in the lab, it was really fun.... My last four days were spent imaging and manipulating xenon atoms; we got two or three papers out of those few days.... I was late to my going-away party because I'd gotten these clusters of xenon down.... I remember laying the images out on Don's car and saying "look at this! I can move Xe around with the tip. Get this party over so we can go back to the lab!..." Roslyn [Don's wife] threw a birthday party for me ... and my wife and kids and in-laws were already there and I thought I got vibrational spectra 20 minutes before the party.... I called up Don, I go "I think I got it here", he goes "what are you going to do?" "I'm not leaving, are you?" So we spent all night and we blew off the party. It was really bad; we spent all night taking data and it turned out to be nothing. We both went home to changed locks on the doors and oh it was so bad <laughs>.... [When I was at] Bell Labs, the kind of surfaces, pressures I worked with, you could take data for the morning, and then the crystal was too dirty to be useful. You had to shut down, go to lunch, come back, prepare the crystal again, take data for the afternoon, and again the crystal would be too dirty. You would go home, have dinner with the family, if you're gung ho you would come and go through two more cycles at night-time.... That all went away at low temperature [at IBM] because ... in a month you wouldn't see any time-dependent contamination. You could just keep taking data, which we pretty much did whenever anything worked.... We'd do stupid things while we did that, we got this 5 pound bag of Jelly Bellies and the two of us sat there all night and we polished them off, so in the morning even when we it was time to go, nothing was working anymore, we couldn't

> even move – and then, you'd think we learned our lesson, but a couple weeks later we did the same thing with 5 pounds of gummy bears. [PW1, 5/3/01]

As we will see, one acknowledged solution to such time pressures was to cleave closely to the body of surface scientific knowledge – to draw as much as possible on the surface science framework to choose materials, questions, and interpretive frameworks in order to move quickly from experiment to experiment.

The tradition of fierce competition within the surface science community at these labs both intensified and complicated these time pressures. Competition raged at all scales: among individuals, among research groups, and among corporations. Bell and IBM competed for glory, for Nobel Prizes, and for the right to pick up the most promising graduates from the best academic surface science programs. Indeed, the rivalry between these surface science superpowers fueled IBM's cultivation of an STM community, and drove Bell to overcome a perceived STM gap. As this Yorktowner describes it:

> I was on staff at that point at Research. You have this hierarchical structure and the director of Research has a staff that sort of advises him, or maybe it's sort of training people. People typically after your fourth or fifth year here go and report directly to the director of Research and advise. That's actually when I heard about [the STM]. This was an accomplishment, something they were bragging about.... One of my most terrible faux pas was, I had to have an opinion on this accomplishment and I said something facetious like "this is one of the few times that the Bell Labs guys are copying us instead of the other way round". And Ralph Gomory was not amused <laughs>. In fact he wanted me to go off and talk to people and ask if that was really the way we felt, that we were sort of second, following Bell Labs all the time. [JK3, 2/22/01]

Within these institutions, various research groups competed for personnel, resources, space, prestige, and remuneration. Indeed, at IBM Yorktown such competition officially structured pay scales – different research groups were set to work on similar projects, and their members were reviewed and paid based on their success relative to their competitors [DB1, 2/26/01; JF2, 3/14/01]. Similar, though less formal, systems prevailed at Murray Hill. Newcomers to these labs had to navigate the politics of this competition; as we will see, borrowing too much of knowledge or STM design from one group could risk drawing the suspicion of that group's rivals. These newcomers also experienced competition at the individual level – in order to survive the review process they had to stand out from their peers. This generated a creative double bind – newcomers were expected (and found it useful) to mold their experiments to the established framework of surface science; yet they were also expected to develop individually distinctive solutions to technical problems, and to create a certain nonconformist experimental persona for themselves.

Thus, the surface science community at Bell and IBM strongly shaped early STM design and use. The labs prided themselves on being wealthy enough to concentrate a large and talented proportion of the surface science discipline in one place. The local surface science communities at these places exemplified the ethic of stiff, internal criticism that we glimpsed in Chapter Two – after all, this allowed the local community to vet theory and experiments intramurally, so that only the best results saw the light of day. At the same time, these communities provided new STMers with a tremendous resource – as we will see, it was only with the help of their local surface science traditions that corporate STMers could replicate the Zurich work. In the sense used by Michel Foucault, new STMers were disciplined by these communities. Local surface science traditions were a locus of corporate oversight that molded STMers' practices, reasoning, and identity – even their resistance to this pressure became part of their inclusion in the surface science discipline (Foucault 1977a; 1994b).

The First Replications

When IBM first considered bringing the STM to North America, Yorktown Heights management seems to have believed – as did Binnig and Rohrer – that electron microscopists were the logical first replicators of the instrument [RF1, 5/2/01; JD2, 2/22/01]. STM was simply another high-resolution microscope for looking at conducting samples that might complement an orthodox electron microscope. Even before publication of the 7x7, IBM sent an experienced Yorktown electron microscopist, Oliver Wells, to Zurich for six months to learn the basics of STM. When Wells returned he began construction of an STM using design elements from traditional electron microscope engineering. Wells' effort, however, wallowed and collapsed, though not before he was able to enroll a British postdoc, Mark Welland, who later jumped to a more successful STM program at Yorktown before returning to England and founding his own STM group at Cambridge.

Yet few electron microscopists entered the field and those who did often left soon after. A few electron microscopists made the transition to probe microscopy at the *end* of the '80s, usually with the aid of more experienced STM or AFM groups. Almost all of those who did were interested in biophysical applications and only understood probe microscopy as its biological relevance became apparent [JZ1, 3/20/01; JH1, 6/10/02]. Before that, the touted benefits of STM were less than obvious to most electron microscopists – they already had a stable set of specimen preparation techniques and commercially available instruments and did not want to deal with an unproven technology like STM. Through the '60s and '70s, electron microscopy had established itself as a mature field with core problematics and practices, and the artifacts to which the instrument was prone were so well understood as to seem irrelevant and invisible. Unlike surface scientists, by the early 1980s electron microscopists no longer maintained a strong tradition of instrument building, concentrating instead on tweaking specimen preparation and commercial instruments for use in particular applications. Thus, the electron microscopy form of life proved unreceptive to STM, where instrument-building was required, and where artifacts were still painfully unresolved. Even later, when bio-STM and bio-AFM came on the scene, a great deal of translation work was needed to convince EMers to take up the new technology.

STM and Newcomers to Surface Science

Let us contrast the barriers to the entry of STM into the EM community with its more rapid, but not unproblematic, uptake in surface science. Initially, Binnig and Rohrer faced lack of interest and even opposition from many surface scientists, not least from the juggernaut of the field, Bell Labs. Murray Hill was one of the places where surface science was invented and where its fiercest proponents worked. Intense criticism and skepticism were characteristic of Bell, and at first the STM did not pass muster [HR1, 11/13/01]. It was the 7x7 that captured the imagination of traditional surface scientists at Bell and made it possible for the replication effort to begin. Notably, it was a young(ish) staff scientist, Jene Golovchenko, who was just beginning to transition into surface science, who began experimental STM work at Murray Hill, despite the concerns of his managers:

> [STM] had all of the romance of being a challenging instrument to make work.... As someone who didn't have a very big investment in surface physics myself, I hadn't been brainwashed.... I don't believe anybody else in the whole surface physics community at Bell Labs made the effort to do this kind of thing.... I think they had investments in [other] methods and of course it's younger people [who start something new].... There was probably some suspicion [from Bell Labs surface scientists] because I wasn't really a surface guy in the first place and I wanted to do some crazy new thing.... I remember proposing to do this and estimating what I thought it would cost. I didn't have a very clear idea, I had never seen an instrument or anything, I had only just seen some papers that had come through. I seem to remember being put on hold for a while. Then as part of my strategy to make things happen I invited Rohrer to come and give a talk.... That was the most crowded auditorium. I was just so thrilled because when I started I didn't really detect much support or anything for it. Now people were sitting on the floors in the aisles in this auditorium, it was the most crowded that I had ever seen.... right after that I got ... 100K or something like that to build a vacuum system and that kind of stuff. [JG2, 2/20/01]

Interestingly, like Binnig and Rohrer, Golovchenko had come to surface science through instrumentation – he had developed an ultrasensitive device for detecting and analyzing x-ray standing waves that he had then brought to bear on problems in surface science. The x-ray apparatus used piezoelectric crystals for very fine angular control of the detector – which, in turn, led Golovchenko to the STM, with its piezoelectric positioning system, as an instrument he could easily build.

Similarly, at Yorktown it was another new staff scientist looking for a project, Randy Feenstra, who had experience building instruments and a desire to learn surface science, who first successfully brought STM to IBM in North America:

> I graduated from Caltech in 1982 and got hired at IBM Yorktown Heights.... I could do whatever I wanted, but I had to pick something good.... I had been doing work in semiconductor materials.... At that time it was a bit of a mature field, and so I thought to myself I might like to move into surface science, which back in 1982 was a well-known field but was certainly still on the upswing, it was by no means a mature field. I mean it was a very active field but most of the problems were unsolved at that time.... I ended up at the IBM lab in Zurich ... and so I saw the scanning tunneling microscope, that was the first time I had heard of it. This was the summer of '82, right so, it was not well-known at that time at all. They had published their early work just on tunneling characteristics, which had gone relatively unnoticed.... Seymour Keller who hired me at IBM, he was a manager .... said "well I think somebody from Yorktown should come here and look at the STM and go back to Yorktown and build one." And I said "I'll do it!..." Because it was surface science, it was something new, it looked like a good project, and it looked better than the other things I had considered. [RF1, 5/2/01]

Thus, for a young staff scientist with a desire to *learn* surface science, the STM looked like a doable (Fujimura 1987) and exciting project even before news of the 7x7 crossed the Atlantic. For *established* group leaders and experienced surface scientists, though, it took the 7x7 to inspire dedicated STM activity. This was true for both theorists and experimentalists. As Don Hamann, one of the most renowned surface science theorists at Bell, explains,

> My involvement in this started off back in either late '82 or early '83.... I got Binnig and Rohrer's paper on the structure of the silicon 7x7 surface to referee. I looked at that and I said "hot damn, this is so exciting...." I got very excited by that paper. They had published previously some results on the gold (110) surface that was kind of ho-hum because it wasn't much of a picture, it was like just a simple sinusoidal oscillation that they could see in one direction and this was a surface where structure was quite well established already anyway by diffraction techniques. While I had been aware of that paper, I didn't

> exactly get excited by it the way I did by the silicon. And since I got an early look at the silicon paper as a referee, and I was a theorist and they were experimentalists, I figured there was nothing unethical about my charging full speed ahead into that problem. Jerry Tersoff had just recently come from Berkeley to work with me as a postdoc, and this seemed like a really good project for him to be involved with. [DH1, 2/28/01]

With the appearance of the 7x7, managers and group leaders at many corporate and national labs began looking for postdocs and new staff scientists for whom the STM would be a "really good project" to work on. At Yorktown, Joe Demuth, a surface science group manager, put together a team to build an STM, hiring Bob Hamers and Ruud Tromp and picking off Mark Welland from Wells' group. At IBM Almaden, management hired two Berkeley-trained surface scientists, Shirley Chiang and Bob Wilson (then a postdoc at Bell Labs), to build their own STM. At Murray Hill, Young Kuk and Joe Griffith each started work on instruments, while at Ford Bob Jaklevic, one of the pioneers of sandwich tunnel junctions, put a postdoc, Bill Kaiser, to work on a tunneling microscope. STM was growing fast.

The Dilemmas of Replication: Did Zurich Matter?

All that remained was getting these new STMs to work. What, though, did "working" mean? Atomic resolution of the 7x7 was the most obvious achievement of the Zurich group, and, given the interest of American surface scientists in that reconstruction, the 7x7 quickly became the yardstick for new STMs. To speed progress, IBM sent many nascent STMers to Zurich to spend time with Binnig and Rohrer and learn the basics. Researchers undertook the "pilgrimage to Zurich" [JM2, 7/6/00] because they understood what Harry Collins has explicated so well: namely, that sometimes the tacit knowledge needed to replicate a new technique can only be transferred by personal contact with those who have already gotten the technique to work (Collins 1974).

As Collins observes, though, personal contact does not guarantee replication. It became clear that going to Zurich helped a little in starting a new STM program, but by no means assured success, and *not going* did not assure failure. Also, whatever was gained by going to Switzerland, it did not seem to matter if you spent six months or six hours. Bob Wilson and Randy Feenstra followed Wells and spent a few weeks, and learned some helpful things about STM design; yet it took them a long time once they returned to resolve the 7x7 [RF1, 5/2/01; BW1, 3/16/01]. After them, IBM sent researchers to Switzerland for less time – just enough to meet the Zurich group, see the instrument, and get design tips. Those who went to Zurich still took a long time to build their STMs and replicate the 7x7, even if they were attempting close copies of the original design. Moreover, the Bell teams and some IBMers did not have much contact with Zurich, yet they kept pace with those who had gone to Switzerland.

Two broad knowledge sets were most relevant to STMers at Bell and IBM. First, they needed generalized knowledge about building experimental apparatus – "fingertip feel" or "laboratory hands" (Galison 1987). Second, you needed knowledge of surface science, particularly its core problems and specimen preparation techniques. As STM became more established, tunneling microscopy at Bell and IBM was entrusted to postdocs who had learned surface science as graduate students. Early on, though, it was people from outside the field who used the formidable resources of the corporate lab environment to quickly come up to speed on the discipline's methods and knowledge in aid of their STM efforts. Interestingly, corporate researchers felt that lack of such immersion slowed non-surface science STMers, especially Calvin Quate's group at Stanford, which started before them yet lagged in the race to atomic resolution [JD2, 2/22/01]. Instrument-building experience and availability of needed resources could, as in Quate's case, take you far; but without familiarity with surface science resolving the 7x7 was murderously difficult.

That is, there are many kinds of tacit skills involved in replicating an instrument, and it is not always transference of the skills peculiar to its original inventors that is most important to replication. In the STM case, immersion in the corporate research lab surface science form of life facilitated replication as much as direct contact or rapport with the Zurich group (at least when resolving the 7x7 was the measure of replication; in the next chapters we will see a later, more widespread standard – atomic resolution of graphite in air). Remember that Binnig and Rohrer themselves had needed to learn the tacit knowledge of corporate lab surface scientists in order to get the 7x7 in the first place. Indeed, new instruments are probably very likely to be invented by mavericks outside or at the fringes of a discipline, yet only find acceptance when they strike a chord with more established members of the discipline; thus, gaining access to disciplined knowledge may be as important an ingredient of replication as access to the inventors (Becker 1982).

Personal contact did seem to matter, though, in the final push to replication. As years went by and none of the North American STMers could get the 7x7, worries began to mount. By late 1984, several groups were building microscopes, but successful replication was elusive [BS1, 1/10/03; JG3, 2/28/01]. For postdocs and young staff scientists enmeshed in internal and external corporate lab competition, with managers conducting reviews and asking embarrassing questions, this was a painful time. In November of 1984, though, Quate brought together several of the North American STM groups with the Zurich people to find some way around the hurdles to replication. Quate had several former students at IBM Almaden and good rapport with Binnig and Rohrer, but was not himself tied into corporate rivalries; and as a well-known academic microscopist he had the gravitas to bring together groups that were competing but who, for almost two years, had not found their way to the starting line of the race [BJ2, 6/27/01; JG2, 2/20/01; BW1, 3/16/01].

What resulted was a small weekend workshop in Cancun organized by Quate and a former student, Alex de Lozanne. One representative attended from each of several groups, usually the more senior group leaders: Bob Jaklevic from Ford; John Clarke a physicist from Berkeley, whose student, John Mamin, was attempting an STM; Bob Wilson from Almaden; Randy Feenstra; Jene Golovchenko; Lynn Swanson, a field emission expert from Oregon; Paul Hansma, a tunneling expert from UC Santa Barbara; and, from Zurich, Heini Rohrer and Niko Garcia, a Spanish physicist who had become intrigued by STM during a sabbatical in Switzerland. The participants met in a hotel suite and gave short presentations about progress on their instruments. For the most part, though, the meeting was a chance to complain about not getting atomic resolution, to pick Rohrer's brain about how the Zurich group had done it, and to trade ideas about how to get around the small family of issues that STMers were beginning to recognize as their primary difficulties: in particular, vibration isolation, thermal drift, sharpening tips, preparing surfaces, and constructing feedback circuitry, and acquiring and preparing piezos.

The Cancun meeting came on the eve of widespread replication of the STM. A few months later, by the March 1985 American Physical Society meeting, Golovchenko had atomic resolution of the 7x7, followed quickly by the other groups at Cancun. Attendees have difficulty, though, saying what, if anything, was accomplished there. Possibly it was simply scheduled when the researchers were at their lowest ebb and so on the verge of making the final breakthrough; or possibly there was some tacit knowledge circulating through the small gathering that gave the final hint. Probably different groups gained different things from the meeting. Either way, the situation improved decisively for some groups only after the face-to-face interaction with Rohrer and with other nascent STMers.

Attendance at Cancun, though, was not decisive, as shown by the interesting path of Joe Demuth's group at Yorktown. Demuth was an old-time surface scientist, with a long career of building various kinds of instruments and applying them to current surface science problems. By the early ’80s he had worked his way up in the management in the Physical Sciences Division at Yorktown, so when he ventured into STM he could throw significant resources at the problem [JD2, 2/22/01; RT1, 2/23/01; BH1, 5/9/01]. This meant he could parcel the problem into different skills and assign postdocs to each package – Bob Hamers, an experienced instrument builder moving into surface science, put together the complicated racks of electronics to control the instrument; Ruud Tromp, a surface scientist who had just completed his dissertation on the 7x7 and other reconstructions, contributed surface preparation and data analysis skills; and Everett van Loenen and Mark Welland did the mechanical design, adapting the blueprints of a microscope built by John Pethica and Mike Pashley at Cambridge, whom Demuth had visited early on (interestingly, when Pashley moved to Philips Research, down the road from Yorktown, he borrowed back the Demuth STM and added his own modifications [MP1, 7/13/01]).[3]

Demuth guided experiments to mesh with IBM’s interests and molded his postdocs (sometimes uncomfortably) into successful corporate surface scientists. Bob Hamers, for instance, remembers how his attempts to bend his research back towards his graduate training in chemistry were frustrated both by Demuth’s advice and by the wishes of the higher Yorktown management.

> What I wanted to do was look for vibrationally inelastic tunneling.... I came in February of ’85 as a postdoc and then the next summer I turned permanent. That was right around the time I had just become permanent and I was basically talked out of it [vibrationally inelastic tunneling] by my manager, Joe Demuth, who thought that that was not a particularly productive way to go.... IBM Yorktown was very much a solid state physics environment, and so people like Paul Ho, for example, who was a manager at that time, were really pushing people to look at the metal-semiconductor interface problem.... Chemistry was not very highly regarded at IBM Yorktown, especially after the whole cold fusion thing emerged a couple years later, and there was not a lot of appreciation of looking at molecular systems.... So from that point on I worked pretty much on these metal semiconductor interfaces [BH1, 5/9/01]

[3] Interestingly, the Pashley/Pethica team were working on adhesion, friction, and interatomic forces, not atomic resolution, so they could not have passed on the tacit knowledge of how to do so to Demuth.

Neither Demuth nor his associates had much contact with Zurich, and their relationship with the other STM groups at Yorktown was competitive and strained; nor did they go to Cancun. Yet they went from no program at all to atomic resolution in a much shorter time than the groups that did go to Cancun. When the value of STM was still uncertain, experienced surface scientists like Demuth (who had little to prove to the corporate research world) let newcomers like Golovchenko and Feenstra risk everything; but with the 7x7, someone like Demuth, who knew the ins and outs of corporate surface science, could quickly gather together the resources and tacit skills needed to build an STM.

The STM Family

By 1984, IBM began to push for the creation of an STM community, and encouraged Rohrer to organize a conference to bring together all the STMers in the world. Initially, Rohrer resisted, since no one (even Zurich) had a reliable machine, and only Zurich had atomic resolution [HR1, 11/13/01]. Big Blue pressed ahead, though, and in July 1985, Rohrer organized a conference at the IBM Europe facility at Oberlech in the Alps. Attendees included: European IBMers and academics; the American corporate lab groups; and academic researchers from California (Quate, Clarke, Hansma, and John Baldeschwieler from Caltech). This was a tremendously successful meeting – small, forthright, and held amidst burgeoning enthusiasm about the possibilities of the technique.[4] Most groups had not yet tested those possibilities – the proceedings show many, many diagrams of instruments-in-progress and images of low resolution "rolling hills" – but the gathering sowed the seeds for friendships and collaborations.

[4] See the articles in *IBM Journal of Research and Development*, v. 30, issues 4 and 5 for the proceedings of this conference.

Back at Yorktown, IBM management further stimulated the growth of an STM community. First, they mounted a vigorous campaign to secure the Nobel Prize for Binnig and Rohrer. The 1983 PRL on the 7x7 had aroused attention amongst surface scientists, but not general awareness of the instrument. At the 1985 March APS meeting, Golovchenko displayed his atomic resolution images of the 7x7 so clear that he called them "pornographic"; the standing room only crowd cheered him on and dubbed the STM a phenomenon [JG1, 10/22/01]. IBM, ever jealous of Bell's crop of Nobel laureates, put together a Nobel package for Binnig and Rohrer and pushed them into the limelight with a Scientific American cover story (Binnig and Rohrer 1985). At the same time, Binnig and Gerber were on sabbatical in California. Their presence seems to have facilitated some transfer of tacit knowledge, and both the Almaden and Stanford groups quickly joined the ranks of atomic resolution [CG1, 11/12/01; SC1, 3/8/01]. Binnig and Gerber also spent part of their year traveling to other STM groups on the West Coast to lend assistance. John Mamin at Berkeley, for instance, remembers their skill at turning a "working" STM into an "atomic resolution" STM.

> Binnig and Gerber traveled around a lot, they were very nice and open, very generous with their time, they were visiting some of the other labs, so they came to our lab and they would also tell us "oh yeah, this lab has got, this lab is now working." [They taught us] some of the little tricks of the trade, putting voltage pulses on the tip to try to clean it.... We were just getting our microscope working but hadn't really gotten any halfway decent images and Gerd came in and it was all sort of running but not running very well, and he flipped some switches and played with it, and it's like, wow magically this beautiful image just popped out. [JM1, 3/15/01]

At Yorktown, management saw they had several groups that would work with STM if they did not have to build one themselves. So Demuth's group, along with several technicians, designed a hardened, simplified version of their STM that could be "mass"-produced by the Yorktown Central Scientific Services shop. Between 10 and 20 of these were built and circulated around Yorktown (and the nearby Hawthorn and Eastview facilities) and later, to a limited extent, to outside academic groups [JV1, 6/28/00; DB1, 2/26/01]. IBM offered the instruments "free" to their research groups (i.e., the group would not need a line item in their budget to purchase the instrument), and eventually a few groups succeeded in using the instruments to produce experimental results.

With the CSS microscopes, STMs could, in theory, more easily be entrusted to postdocs and used to produce quick, novel data. The batch microscopes, though, usually needed extensive modification, or even rebuilding, for complex sociotechnical reasons. Postdocs still needed to show that they had experimental "hands" and could create independent niches for themselves within the agonistic field of corporate surface science. Thus, the CSS instruments, built as simplified versions of the Demuth group's design (and hence geared to, and associated with, Demuth's specific style of surface science) could be seen as suspect tools by other groups. *Rebuilding* the microscope was a way to display the skills needed for successful corporate research and form a distinct scientific identity. As Bob Wolkow, who worked at both Yorktown and Murray Hill, describes it, the politics of competition at the individual, group, and institutional levels all shaped the design and use of the batch-produced CSS instrument, as well as other custom-built corporate STMs:

> Demuth was an interesting character. He generously made that machine available to a lot of people to copy. Members of his group were distrustful of one another. They bickered over credit for who did what when – all too common a problem actually. One of the key scientists had a great falling out with Joe Demuth. It became a nasty business. Yorktown was at once wonderous and unfriendly. The setting, the collection of sharp minds, the resources were superb. But the internal rivalries made it quite unpleasant at times. It seemed like for every sexy project going on, I don't know if it was by plan or by accident, there was a competitive group in the same building. So the Feenstra group distrusted the Demuth and Hamers team, and when I came on the scene I was able to get one of these kind of cloned plans of the so-called Demuth machine and, well, it didn't work, I had to make it work and I was just the right fiddly guy.... There was a staff scientist in Phaedon Avouris' group who was charged with getting an STM.... He was kind of collecting pieces and squirreling them away, literally in a closet in his office, and I was a postdoc who was to assist him but no one ever told me who was my supervisor and what I was responsible for and after ... two or three months had gone by and I

> had done virtually nothing and I was thinking "what the hell." I talked to one friendly staff scientist there, and he said "well, if I was you I would just take charge of the situation and don't worry about who you piss off, you're going to die if you go on this way." So I just literally stole all the equipment from this guy's office, moved it down to a lab that had been allotted to me, and I just started building.... I had no support, so this guy was really pissed off with me for doing this, and my boss, Phaedon Avouris, he didn't take sides, he didn't favor me or the other guy. When I got it working, I had proved myself, then he fully supported me, he saw I had the hands to make it work. I added a bunch of things to that machine right away. Well, the funny thing is that, so I told you there were these camps, well Feenstra assumed that I was part of the Demuth camp, I guess because I was physically next to them in the building, and so for a good year or so ... he was very cool toward me, and only when he realized that I wasn't particularly close to those guys either he warmed up to me. The rivalry between those IBM teams and Bell Labs was even more severe, it was like some kind of football rivalry or something. [BW2, 5/22/01]

Making STM into a Surface Science Tool

So how were STMs at Bell and IBM geared to the corporate lab environment? As outlined in Chapter Three, Binnig and Rohrer had not been particularly mindful of surface science in building their instruments, nor had the instruments they built been particularly reliable. When the STM reached North America, many researchers started building close copies of the Zurich design, but found them unsatisfactory; at NIST, for example, they built an exact copy of the Zurich "birdcage" instrument, but found it so unreliable that they eventually had to wait for the arrival of a Yorktown veteran, Joe Stroscio, to build a new one more appropriate for surface science experiments [BC1, 6/11/02].

Unreliability had not been a major drawback for Binnig and Rohrer (indeed, in some ways it was a strategic asset). They were building instruments as an existence proof. Their style was to demonstrate one or two applications and move on. Moreover, in the early days, the STM had been a side bet, something to work on after hours or around other projects. If the microscope was off-line, that merely meant they could work on other things. Unreliability had been part of the exploratory and craft nature of their project, in keeping with Binnig's cultivation of an erratic experimental persona. The postdocs and young staff scientists trying to replicate the STM, though, needed to implement the microscope as a robust, routine (if novel) tool to generate publications. This entailed changes in design, starting with a better fit between the STM and UHV. Binnig and Rohrer had not been wedded to UHV, and when air STM came along they moved in that direction; but for surface scientists UHV was indispensable. UHV was an important nexus of incommensurability between surface science STM and other brands of probe microscopy. Air versus UHV provided a critical axis on which to define one's identity as a corporate surface scientist, and thus it generated critical, often harsh, boundary-drawing with respect to air STMers [BH1, 5/9/01; DE1, 10/11/01].

UHV can be a very time-consuming environment, even for experienced surface scientists. Taking an instrument out of the chamber, tinkering with it, putting it back in and bringing it back down to full vacuum can eat up a week or more of research time. At Zurich this had been routine, since the group was continually making modifications to the design. At Yorktown and Bell, though, researchers needed to put a large quantity of samples through the microscope without making major changes to the instrument itself. So they built in airlocks and sample exchange systems, to allow samples to be moved in and out quickly; they added the traditional surface science complement of specimen preparation technologies, so that samples could be quickly cleaned in-chamber and modified before characterization; and eventually, as they came to believe that tip shape was vital to the quality of the images they produced, they added tip exchange systems, so that a bad tip could quickly be pulled out and a new one put in without breaking vacuum.

Also, they modified the STM to insinuate it into both surface science and the corporate and visual culture of the big research labs.[5] For instance, Binnig and Rohrer

[5] The literature on visual culture and science is now overwhelming. A few key works are: Cambrosio, et al. (1993); Goodwin (1997); Rudwick (1976); Lynch and Woolgar (1988); Jones and Galison (1998).

– despite working for IBM – had flatly rejected using computers to control their microscopes or to display images. Again, this helped them cultivate craft status for their instrument and the images it produced and virtuoso status for themselves. At Yorktown, Murray Hill, and Almaden, though, computers were an essential part of corporate culture and, concomitantly, of the institutional way of doing research. Incorporating computer control was credited with making it easier to operate the instruments and to interpret images, but making them part of STM practice was not always easy, particularly when research needed to be balanced with corporate citizenship – for instance, researchers at IBM and Bell felt pressured to use the latest PCs or operating systems developed within their own institutions, rather than the equipment best-suited to their STMs [RT1, 2/23/01; JG3, 2/28/01].

Perhaps the corporate labs' most major addition to the STM came in the area of spectroscopy. After low energy electron diffraction, various kinds of spectroscopy made up the bulk of the surface scientist's toolkit. "Spectroscopy" is a rather elastic term that describes methods for inputting a range (or spectrum) of values into a system and recording the spectrum of values that come back out. In most surface spectroscopies, the inputs and outputs come either as electromagnetic waves with a spectrum of frequencies (ultraviolet, infrared, x-ray, light) or any of a family of particles (electrons, ions, neutrons, alpha particles, etc.) with a range of momenta. The alphabet soup of surface spectroscopies is a result of the mixing and matching of inputs and outputs. Experimenters can put in any of several kinds of radiation and get radiation back out; they can put radiation in and knock several kinds of particles back out; or they can put particles in and get either radiation or more particles back out. Each kind of spectroscopy corresponds to a different pair of inputs and outputs, and each tells something slightly different about the energy bands in which electrons are bound to the surface, or the strengths of the chemical bonds holding surface atoms together, or the springiness of the internal bonds of molecules adsorbed to the surface.

Much of the work of the discipline went into creating and taming these techniques so their output could be interpreted as complementary. Getting spectroscopic information out of an STM was an advantageous experimental route for nascent surface scientists at IBM, Bell, Ford, and elsewhere. Binnig and Rohrer's limited efforts in this direction had met with little success. The postdocs and junior researchers at North American corporate labs, though, quickly constructed the electronics, software, and data analysis tools to turn their newly working microscopes into spectroscopic instruments. Doing scanning tunneling spectroscopy involves changing the bias voltage between the tip and the sample (the input) and monitoring how this affects the tunnel current (the output). Systematically varying the bias voltage while holding the tip at a constant lateral position yields a current-voltage (or I-V) curve. Changing the bias voltage while scanning over the sample shows you where on the surface various peaks in the I-V curve are located, peaks that correspond to places where surface bonds allow electrons to preferentially tunnel in or out. Thus, scanning tunneling spectroscopy (STS) can tell something about the two-dimensional position of these bonds – microscopy with a "chemical signature." There are a number of ways to do STS, but all of them involve breaking the usual STM feedback loop at some point in the scan; where, and how often, to break the loop was the major question in turning STM into a spectroscopic tool.

It is instructive to contrast the Feenstra group's solution to this problem to the Demuth group's. Recall that Feenstra was new to the corporate research world, and for most of the '80s he usually only had one postdoc working for him. Using their limited resources, his group devised two ways of doing tunneling spectroscopy [RF1, 5/2/01; JS1, 6/28/00]. The first allowed the tip to be positioned over a single spot and kept stationary with extraordinary precision while the bias voltage was ramped up and down, yielding a continuous I-V curve comparable to curves seen with other spectroscopic instruments. The second method allowed the bias voltage to be alternated between two values while the tip was scanning, yielding two electronic pictures of the same area. The images might look completely different if, for instance, one bias voltage picked out electrons tunneling *out* of filled states and the other picked out electrons tunneling *into* empty states; this could be represented graphically by combining the images with different color schemes allotted to each bias voltage. A celebrated early example was a Feenstra and Stroscio image of gallium arsenide showing the gallium atoms in red and the arsenic atoms in green (Feenstra, et al. 1987).

The Demuth group, meanwhile, had more resources and people at its disposal and its spectroscopy design built on these advantages. Their solution was to break the feedback at *every* pixel in a scan and take 24 I-V readings for each point. This would yield *24* different pictures of the same area for each scan, or several thousand individual I-V curves. This entailed an enormous commitment of resources in computer time, in data analysis, and in assembling software and electronics – Hamers, for example, had to string together rack after rack of boxcar integrators, so that the Demuth STM soon filled up much of its lab space [RT1, 2/23/01; BH1, 5/9/01; DB1, 2/26/01]. At the time, there was little to choose between the Feenstra and Demuth techniques. Feenstra's is often called more "elegant" and Demuth's more "brute force," but both had areas in which they seemed most appropriate; where Feenstra made a splash with gallium arsenide, Hamers, Tromp, and Demuth gained acclaim with articles on the 7x7 (Hamers, et al. 1986). The 7x7 had, of course, already been studied by every spectroscopy imaginable. By using their method, the Demuth team could image the whole 7x7 unit cell and display separate I-V curves for each adatom in the cell (see Figure 4-1). This would provide atom-resolved information about electronic states, so that every peak and inflection seen in various bulk spectroscopies could be traced back to a particular atom in the cell. Thus, in one fell swoop, the STM could be stitched into agreement with the whole complement of surface scientific knowledge, instruments, and practices. For other surfaces, though, Demuth's method was bureaucratically and technically burdensome, without producing any more striking results than other STM spectroscopies at Bell and IBM.

This cadre of STMers further synthesized tunneling microscopy with surface science by putting an STM in the same vacuum chamber with other traditional surface science instrumentation. The first to go in was the mainstay of the discipline, low-energy electron diffraction,. Binnig and Rohrer had prepared specimens in one chamber, then transported them by hand in air to the STM; indeed, they counted it as a "discovery" that they saw anything at all after such a procedure. For surface scientists, though, this was bad practice, and they assumed this was why it took the Zurich team so long to tame the 7x7. Instead, they devised ways to bring a specimen into UHV, prepare it with in-chamber techniques, and use LEED to see if the surface was exhibiting the desired reconstruction. Later, other instruments were combined with STM – Auger electron spectroscopy, electron microscopy, x-ray photoemission spectroscopy, etc. Those who moved in this line of experiment built larger and larger vacuum chambers to house all the different tools. At Almaden, for instance, the Wilson/Chiang team took advantage of the construction of a new research building to specify a laboratory that could house an extraordinarily large chamber with a full complement of sample preparation and characterization techniques, through which samples could be moved from one tool to another [BW1, 3/16/01; SC1, 3/8/01].

The coordination of STM with other surface science techniques took place not just in the vacuum chamber, but in all areas of experiment. When Golovchenko had

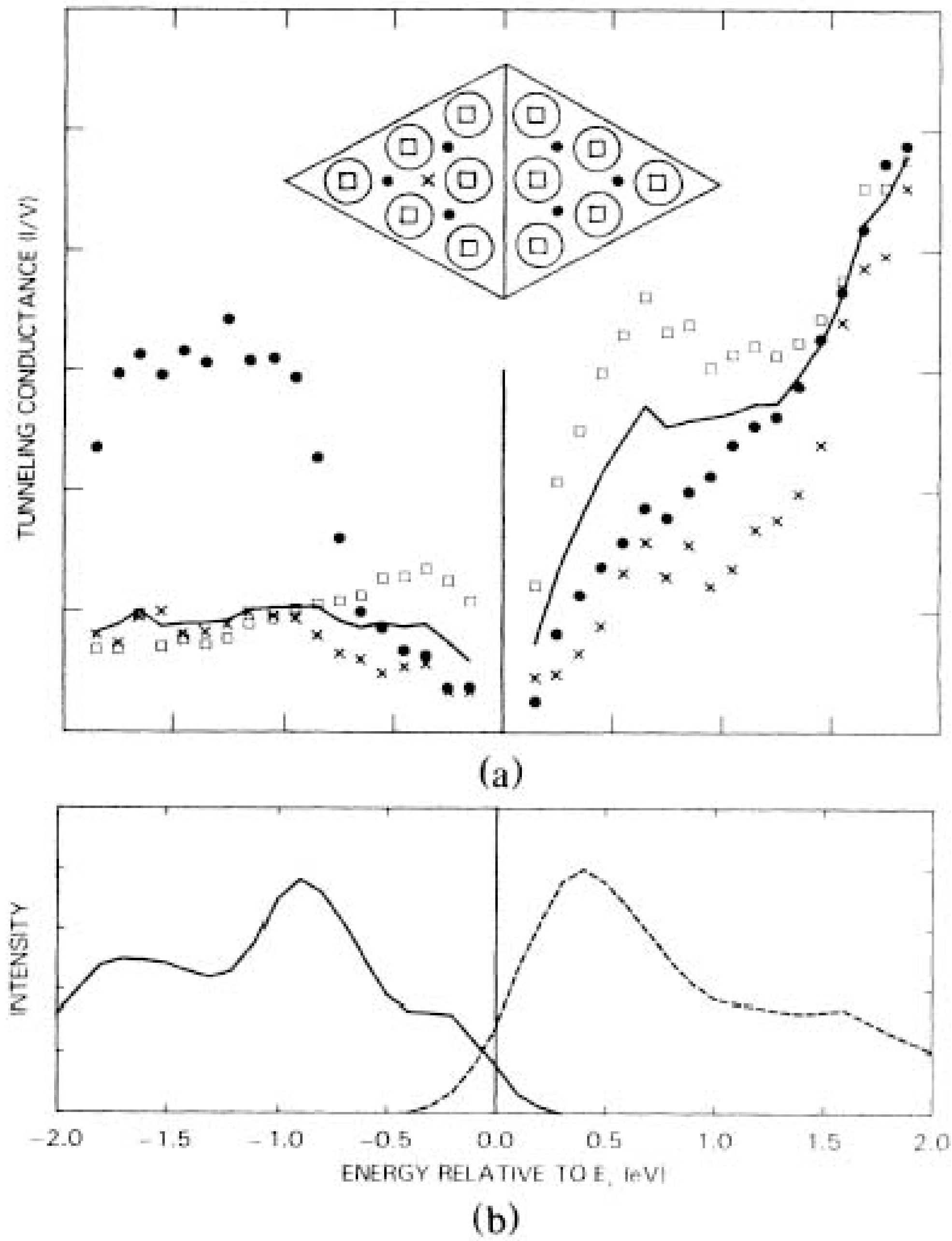

Figure 4-1: Scanning tunneling spectroscopy. One tactic for integrating the STM into surface science was to make it complementary to existing surface spectroscopy tools. This image combines scanning tunneling spectroscopy of the silicon (111) 7x7 – the various atoms in the unit cell of which are pictured at the top – with ultraviolet photoemission spectroscopy (the dashed line in the bottom box) and inverse photoemission spectroscopy (the solid line in the bottom box). The different curves in the upper box represent I-V spectra measured over different atoms in the unit cell. From Hamers, et al. (1986).

talked about atomically-resolved images of the 7x7 as "pornography", for instance, he was referring to the indirectness of results provided by LEED and other techniques.[6] STMers had to work to make STM seem to gave a "direct" image of the same surface entities. Some went as far as to "reclothe" STM images by Fourier-transforming them "back" into something very similar to a LEED image (see Figure 4-2).

> It was never such that you should imagine Fourier transforming a tunneling microscope picture and it would agree with a low energy electron diffraction picture.... But nevertheless it makes people comfortable, you still will see a 7x7 pattern if you Fourier transform the tunneling microscope pictures. It won't have the intensities that LEED guys have or anything like that, but looking at this thing and knowing that it's 7x7, that it's not totally wrong.... I never thought it was worthwhile looking at a surface that I didn't know what the LEED pattern was beforehand, because I wouldn't even know if it was something. You didn't even always know what the calibration [of the piezo scanners] was.... You often saw pictures that looked distorted and not square because people were shifting them around, so that it would conform with fundamental things that you knew about the symmetry of the surface [from LEED]. So it was an interesting thing to see the interaction of these two surface physics ways of doing things, one a new baby and another that had gone on for a very long time. [JG2, 2/20/01]

LEED, though, gives data about atom positions based on an average over a fairly large area (tens of microns by tens of microns), and so making STM images comparable to LEED data encouraged STMers to expand the lateral range of their instruments. From the beginning, one objection put forward by skeptical surface scientists was that STM was so local that one could never be sure its results were not a fluke. So long scan ranges became a priority, particularly as the CSS STM made tunneling microscopy accessible not just for surface physicists but for surface chemists as well [PA1, 2/22/01].

Other aspects of the visual culture of STM in this period contributed to its integration into surface science. The use of computers for image processing and display, for instance, was not merely to tie the microscopes to IBM or Bell's

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[6] The early modern metaphor of science as a disrobing of Nature has been well-documented in Schiebinger (1993), among other works,.

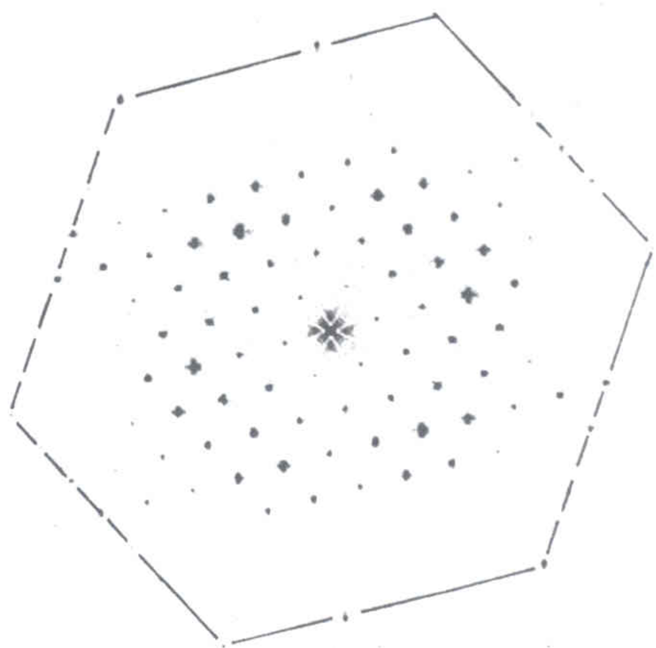

Figure 4-2: Fourier-transformed STM image. One way for STMers to insinuate the instrument into surface science was to take STM images and Fourier-transform them. These "reciprocal space" STM images were then similar, though not equivalent, to LEED images – since LEED is a reciprocal space technique, but one that averages over a much larger area, and to a somewhat lower depth, than STM. Crucially, surface scientists looking at these Fourier-transformed STM images could see that the STM was directly seeing the same unit cell symmetries that could only be indirectly glimpsed through LEED. From Demuth, et al. (1988).

computing infrastructure. It was also to produce images that could speak to traditional ways of seeing in surface science, particularly those ways of seeing that enabled dialogue between theorists and experimentalists.[7] Corporate surface science matured with LEED and the creation of better theoretical and computing machinery to process LEED images; experimentalists created LEED data, while theorists imagined structures for reconstructions and drew maps (with a viewed-from-above perspective) of them, projected the LEED pattern for those structures, and compared them to experimental results (see Figures 4-3 through 4-6). The fertile nature of this work came from the flexibility of the fit between simulated and experimental LEED data, meaning dozens of possible reconstructions could generate similar LEED images.

Binnig and Rohrer's pictures had not been crafted solely with surface science in mind. Their first cardboard "image" of the 7x7 was striking and artistic, but did not resemble the bird's-eye maps surface scientists were used to. Surface science *theorists* wanted something like a topographic map, minus streaky scan artifacts. For instance, the utility of Tersoff-Hamann theory (the most widely referenced STM theory of the time – see Figure 4-7) was that it showed that, often, the STM tip simply measures average charge density; STMers could draw lines of constant charge density and easily simulate STM images for a reconstruction that fit approximations made by the theory (Tersoff and Hamann 1983). Thus, the corporate STMers quickly write line-filling software that would remove scan artifacts and produce an overhead, grayscale view of a surface that could easily be compared to a theoretical simulation in the same way earlier surface scientists compared experimental and simulated LEED results.

Finally, a last resort for harmonizing STM with other surface science instruments was a managerial one – different groups, specializing in different

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[7] The materialization of theory I describe here resonates with Eric Francoeur's analysis of chemistry models. See Francoeur (1997).

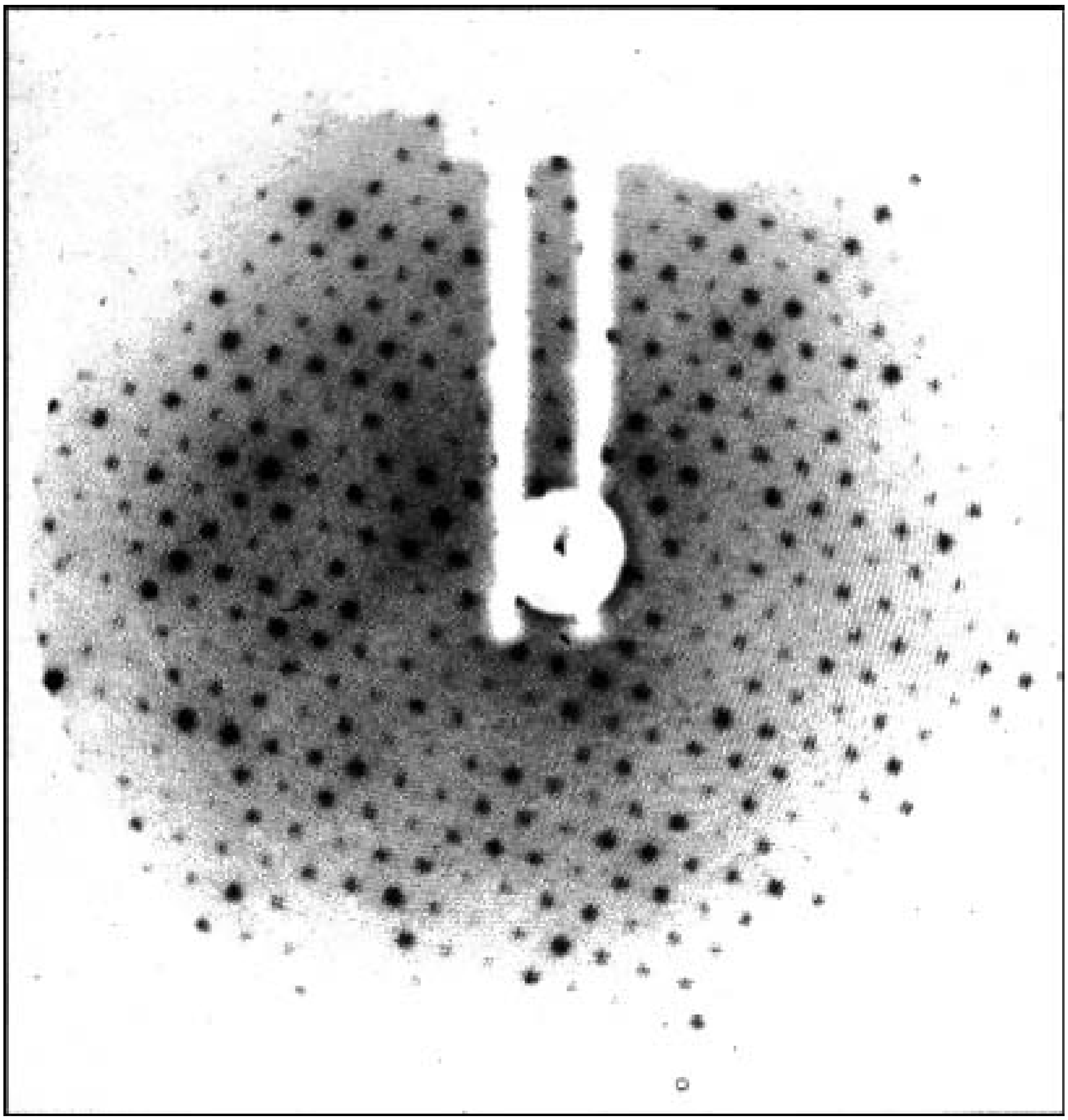

Figure 4-3: LEED of the 7x7. This shows the LEED diffraction spots for a sample of the silicon (111) 7x7. From Miller and Haneman (1979).

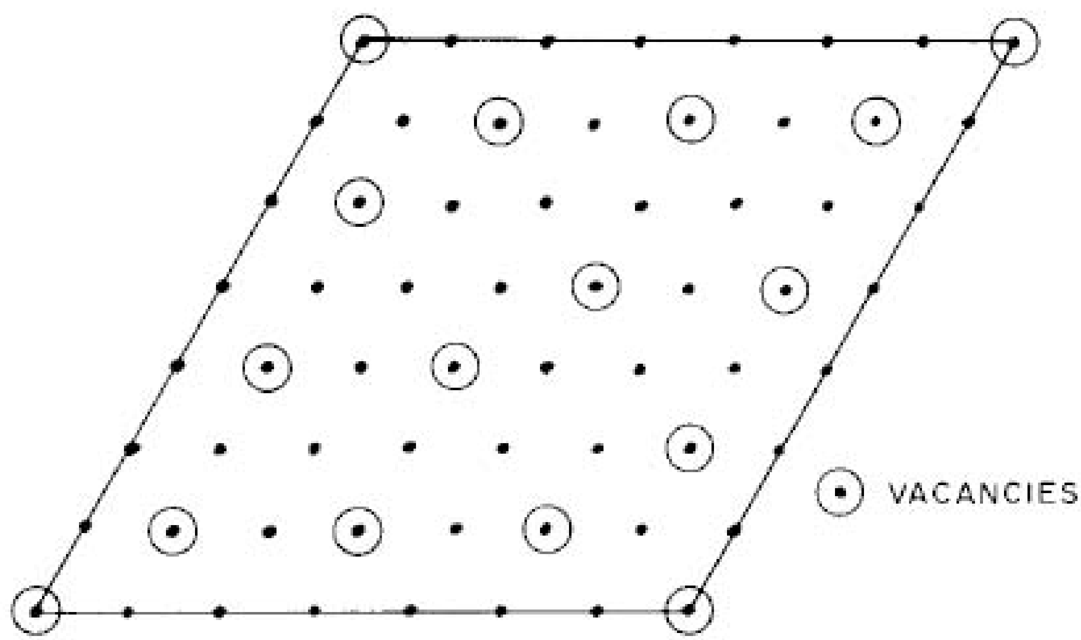

Figure 4-4: Model of the 7x7. From the same article as the experimental LEED pattern in Figure 4-2, this figure shows a proposed model for the structure of the 7x7 (not the one that was eventually accepted). From Miller and Haneman (1979).

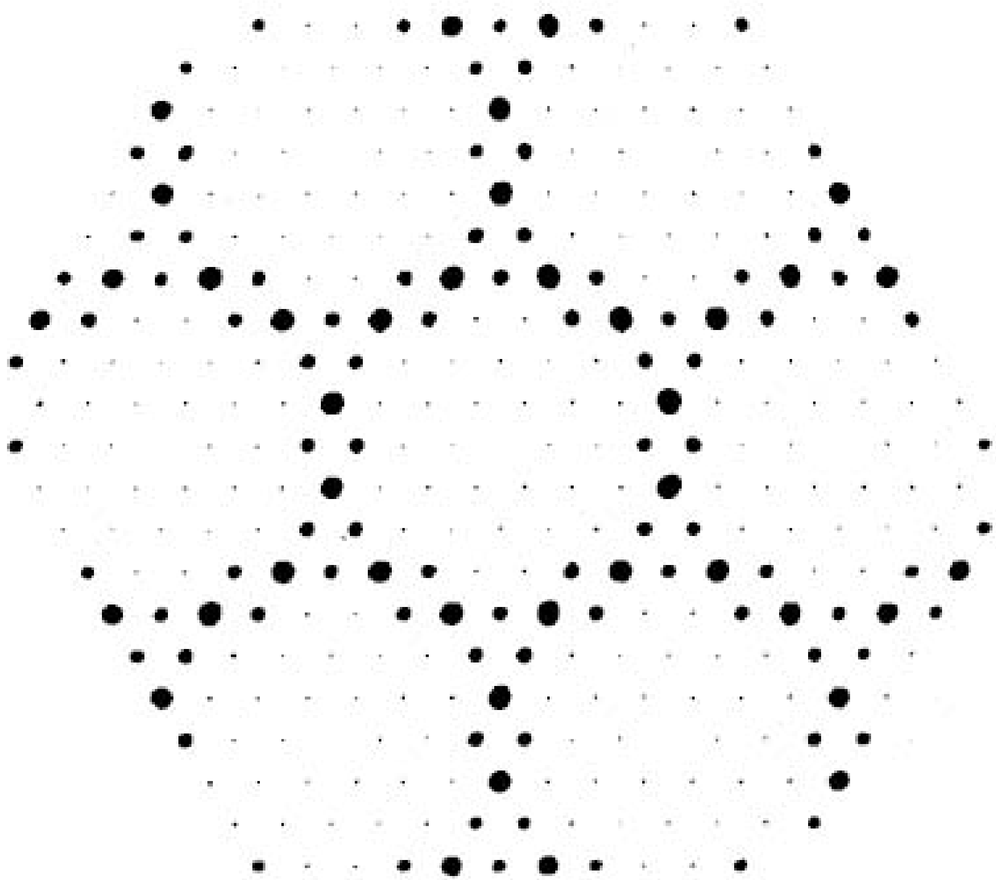

Figure 4-5: Simulated LEED pattern. Again, from the same article as Figure 4-2, this image shows a simulated LEED pattern that corresponds to the structure model proposed in Figure 4-3. This way of matching experimental LEED data, proposed structure models, and simulated LEED patterns was common in surface science, and was a practice STMers had to contend with. From Miller and Haneman (1979).

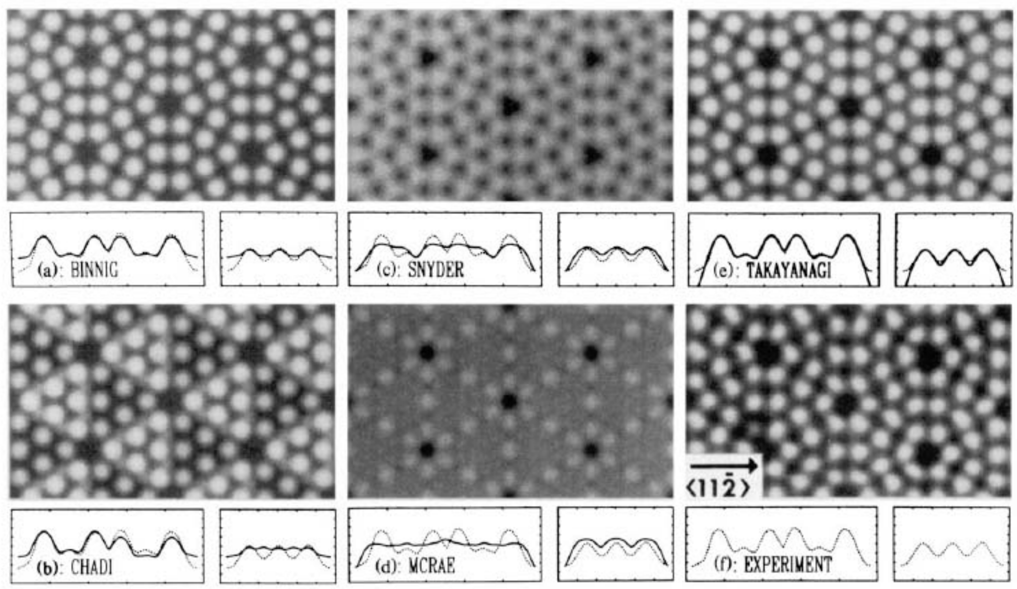

Figure 4-6: Matching STM images to theory. The other way corporate STMers made tunneling microscopy a surface science tool was to develop simulated STM images corresponding to theoretical proposals for various reconstruction models and comparing those simulations to experimental STM images. The Takayanagi proposal (top right) is the D-A-S model that has been most widely accepted. From Tromp, et al. (1986).

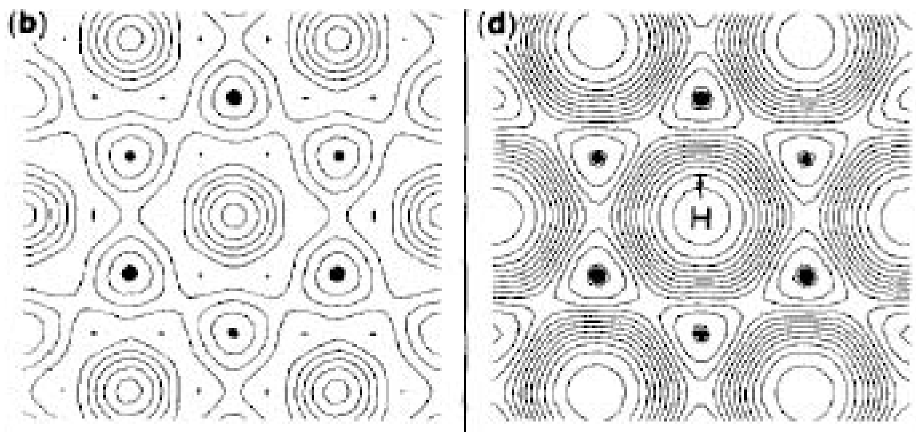

Figure 4-7: Tersoff-Hamann theory. Don Hamann, a surface science theorist at Bell Labs, and Jerry Tersoff, his postdoc (later a staff scientist at IBM Yorktown), formulated the most widely used theory of STM operation. The Tersoff-Hamann theory states that, for some important surfaces, the STM image is a measure of the density of states. Using the theory, STMers could compute the density of states for a given surface, then compare it to an actual STM image. Below are simulations based on Tersoff-Hamann computed for graphite at two different tunneling voltages. From Batra and Ciraci (1988).

techniques, might report to the same manager, compelling eventual agreement about what their data meant:

> JK: There was a group doing infrared [spectroscopy of superconductors], and the infrared gives sort of comparable information, and the size of the gaps that I was seeing [with STM] were much smaller than the size of the gaps in the infrared. It turns out that there's a good reason for that.... The tunneling, it was so confusing, you can argue either way. A lot of these techniques were so confusing. People still argue about whether you can actually see a gap in infrared.... It really took a long time to just figure things out.
> CM: So how did it work talking with these infrared groups?
> JK: Well, I mean they were reporting to me too <laughs>. So they had to be polite. But I certainly respected them. And it was clear that there was some difference and it's probably real. It just took a long time to sort of work things out. [JK3, 2/22/01]

Surface Science and the Course of Experimentation

Once the first STMs were built and running at IBM and Bell, the tunneling microscopy community at these labs expanded quickly. At the same time, as some of the postdocs and staff scientists who worked on the first STMs moved out of the corporate labs and into academia or national laboratories, STM went with them. This was the start of what David Kaiser has called a "postdoc cascade" – the rapid growth of a field from the diffusion of trained people rather than primarily through the diffusion of ideas (Kaiser forthcoming-b). From Bell, for instance, Golovchenko moved to Harvard; his Murray Hill technician, Brian Swartzentruber, moved to the University of Wisconsin to get his Ph.D. and helped turn Max Lagally's distinguished LEED group into an equally distinguished STM group; and Young Kuk moved back to Seoul to found a highly successful probe microscopy group in Korea.

Similarly, from Yorktown Bob Wolkow moved to Bell and then the National Research Council of Canada; Joe Stroscio and John Villarrubia moved to NIST; Bob Hamers eventually went to Wisconsin; Randy Feenstra to Carnegie-Mellon; Mark Welland to Oxford; and Everett van Loenen to Philips. IBM Almaden eventually lost Shirley Chiang to UC Davis, but picked up Don Eigler from Bell, and Eigler in turn trained a series of postdocs in low temperature STM who went to Berkeley, Stanford, Penn State, and the University of Illinois in the mid- to late-'90s. Some of the movement out of these labs was organic; indeed, postdoctoral positions were designed to quickly train researchers in a particular area before packing them off to some other institution. In the early 1990s, however, a mass exodus of early STMers, particularly to universities, began as recession gripped IBM and, to a lesser extent, Bell, effectively stifling much STM research at the big corporate labs [SC1, 3/8/01; RF1, 5/2/01].

Thus, the community expanded beyond the corporate labs, although it was still largely centered on IBM and Bell. Indeed, the race to STM added fuel to the inward-looking rivalry between the two surface science superpowers of the day:

> I did another review article for *Annual Reviews of Materials Science* in which I highlighted their [Avouris and Wolkow's] work [at Yorktown]. In fact that ended up being kind of an odd little article because all of the examples that we gave in that article were either from this building [Murray Hill] or Yorktown Heights. It was as if the rest of the world didn't exist. I kind of felt bad about that but looking at it, at the time .... I wrote that review article it was primarily people at Yorktown Heights and Bell Labs here who had most of the stuff going on. [JG3, 2/28/01]

As the community grew, it did so along dimensions framed by surface scientific practices and knowledge. As building STMs became more routine, for instance, it was expected that STM builders would gear their instruments to specific surface science questions or specific niches of surface scientific practice. Wilson and Chiang, for instance, built an instrument which could prepare surfaces in special ways and add a variety of overlayers and adsorbates to a silicon or metal surface (Wilson and Chiang 1987b); Bob Wolkow built a low-temperature instrument at Yorktown, then at Murray Hill he built a variable temperature one capable of playing with reaction rates in order to understand surface chemistry (Wolkow 1992); and Don Eigler built a low temperature STM at Bell, then rebuilt it completely at IBM Almaden, in order to chase one of the holy grails of surface science, vibrational spectroscopy of an individual molecule (Eigler 1998).

The choice of surfaces to look at was also crafted to the surface science audience. Indeed, as the technology matured, the most valuable group leaders postdocs could work under were those, such as Avouris or Demuth, who could enumerate the most important questions in the surface science community and point their younger colleagues to them [BH1, 5/9/01; BW2, 5/22/01] – an indicator of how the STM community at the big labs changed in the few years since Feenstra and Golovchenko had an enormous impact on surface science despite their own newness to the field. The parameters that had the most meaning to surface scientists, and thus guided what to put in an STM, were the sample material, crystal index, unit cell, and adsorbate composition. A group could move along any of these variables in ways marked out by years of surface science tradition; new instruments were common in this community, and the ways to exploit them in an orderly fashion most conducive to the time pressures of postdocs and young staff scientists were well known.

So, you could pick a particular material and index and go through all of the associated unit cells, as the Golovchenko group did with silicon (111) 7x7, then 9x9, 5x5, and 11x11 (Becker, et al. 1986). You could take a material like silicon and go through a variety of indices and unit cells, as the Demuth group did with the (111) 7x7, the (111) 2x1, and the (001) 2x1 (Tromp, et al. 1985; Demuth, et al. 1985; Hamers, et al. 1986). You could move through different though related materials, whether semiconductors (as the Feenstra group did with silicon, germanium, gallium arsenide, and eventually gallium nitride) (Feenstra, et al. 1986; Feenstra and Stroscio 1987; Feenstra, et al. 1991; Smith, et al. 1998); or metals (as the Wilson/Chiang group did with gold, platinum, copper, and rhodium) (Hallmark, et al. 1987; Ohtani, et al. 1988; Woll, et al. 1990; Samsavar, et al. 1990; Hallmark, et al. 1991). You could put down a series of adsorbates, as the Wilson/Chiang group did with naphthalene, azulene, and several methylazulenes on platinum (Wilson and Chiang 1989); or, like the Eigler group, you could put down a series of individual molecules on a similarly varying series of substrates (iron on copper, benzene on platinum, oxygen on niobium, xenon on nickel) (Eigler, et al. 1990; Weiss and Eigler 1993). Such a course of experimentation could be done methodically and quickly – skills and knowledge useful for one sample easily transferred to the next. The results of such experiments could be easily published and digested by the surface science community – STM helped fill in known gaps in the discourse of surface science (e.g. the atomic structures of reconstructions) and a postdoc or young staff scientist could quickly move from gap to gap, supplying much-needed information in a readily intelligible manner. Thus were surface scientists (and STMers) made.

Hybrids

IBM Research and Bell Labs were large organizations within very large corporations – prior to its break-up in 1984, AT&T briefly employed more than a million people. So it would be difficult to imagine that these institutions were culturally monolithic. Different disciplines within IBM Research and Bell Labs were at different stages in their development, had different histories and ecologies, and hence constructed different ways of conducting experiments and training new generations. Within the local surface science communities, different groups had to forge different practices because of their varying relationships with the corporate center. As we've seen, for example, the more well-resourced Demuth group approached STM design very differently from the less well-established Feenstra group. The culture of internal and external competition tended to homogenize practices somewhat – researchers ofen converged with their rivals on very simlar questions, even as they were pressured to find distinctive, exemplary answers to those questions. There were a few groups, though – particularly at IBM Almaden – who took STM in a more radical direction, and their story is worth noting since it casts light on a style of probe microscopy that we will examine more in the next chapters.

One of the first attempts to explore an alternative path for probe microscopy in the North American corporate world came from Bell. There, management perceived a need to answer IBM's invention of the STM with a new instrument of its own [DP1, 11/7/01; KW1, 2/23/01; SB1, 3/22/01]. They probably knew that an IBM Zurich group (led by Dieter Pohl and Urs Dürig) was working on a relative to the STM known as the scanning near-field optical microscope (SNOM) (Pohl 1993). Near-field microscopy had a pedigree even more ancient than STM. The theorist E.H. Synge (brother of the Irish playwright John Millington Synge) had speculated on the properties of evanescent light waves propagating through a very small aperture in the 1920s, and Hans Bethe had generated a more rigorously mathematical analysis in the 1930s (Wickramasinghe 1989).

Experimentally, a small aperture had been used for scanning near-field microwave microscopy in the early '70s by the group of Ash and Nichols in England, though few had followed up their demonstration (Ash and Nicholls 1972). When the STM showed that very fine control of solid probes at very small distances from surfaces was possible, near-field optical microscopy again aroused interest (indeed, it is possible to interpret the STM as a near-field *electron* microscope, and the first SNOMs – or NSOMs as they were known in North America – used electron tunneling as the feedback mechanism to keep the aperture close to the surface) [DP1, 11/7/01; UD1, 11/12/01]. Bell, with its long history in optics, saw this as an area of probe microscopy where they could quickly establish a lead, and in 1988 they hired a graduate student from Cornell, Eric Betzig, to bring NSOM to the world of the big labs. Within a few years Betzig had helped put together an NSOM group at Bell, and, with Pohl, awakened a community of near-field microscopists; new ways of making apertures and sudden increases in the resolution of the instrument made it seem like a promising alternative to other optical microscopies, especially in the area of single-molecule spectroscopy [BD1, 1/2/01; SX1, 2/21/01].[8]

At IBM, several groups pushed away from STM, particularly after Binnig, Quate, and Gerber developed the atomic force microscope. Big Blue tried to patent the AFM as an IBM invention, and encouraged research groups to take up the new instrument. A few IBM groups, such as Gary McClelland's at Almaden and Nabil Amer's at Yorktown, made major contributions to the development of AFM as a practical, versatile lab technology [GM1, 3/16/01; SC1, 3/8/01]. In general, though, the evolution of AFM owed more to academic groups (and their commercial spin-offs), especially Cal Quate's and Paul Hansma's, which we will examine in the next chapters. Interestingly, four research groups brought personnel from those academic labs into IBM, with telling results: at Almaden, John Foster (a former Quate student) and Jane Frommer worked on air STM; also at Almaden, John Mamin and Dan Rugar (another former Quate student) developed STM, AFM, and magnetic force microscopy; at Yorktown, Kumar Wickramasinghe (a former Ash student and Quate postdoc) put together an impressive program on a variety of probe microscopies; and also at Yorktown, John Kirtley, a former Hansma student, invented scanning SQUID microscopy.

All of these groups adopted some elements of an experimental style that we will explore in the next chapter. None of them, for example, was tied to UHV (indeed the Foster/Frommer group popularized air STM as much as anyone). Of the four, only

[8] Near-field microscopy works by containing radiation so locally that it does not propagate away, but rather "evanesces" in place. Using a very small aperture, one can actually use these evanescent waves to form an image with much greater resolution than an ordinary optical microscope. The most intuitive analogy is that NSOM is the equivalent of a stethoscope for light (Pohl, et al. 1984).

Kirtley's was housed with surface scientists in the Physical Sciences division of IBM Research. Wickramasinghe worked in Yorktown's Advanced Manufacturing Research Division, with responsibility for developing characterization technologies that would be useful to various IBM manufacturing lines – particularly semiconductor manufacturing [KW1, 2/23/01]. Thus, he concentrated on developing an AFM for on-line wafer inspection, creating the SXM, the first of the giant factory-floor AFMs (Martin and Wickramasinghe 1995). For Foster and Rugar, the environment at Almaden encouraged research and development on data storage (the nearby IBM plant in San Jose manufactures most of the company's memory devices), especially since Almaden traditionally had a more applied orientation than Yorktown (Frommer and Foster 1988; Stern, et al. 1988). Later, Rugar and Mamin started working with force microscopy, developing proofs of concept for data storage with AFM, as well as making advances in magnetic force microscopy (which, today, is a crucial characterization technique for the magnetic data storage industry) (Rugar, et al. 1990).

Foster, meanwhile, acquired a semi-commercial air STM from a Quate student, Douglas Smith, and began toying with its electronics and software to develop lithography schemes. When he began seeing contaminant molecules on the graphite substrates on which he was trying to write bits, he called on Frommer, a chemist, to help him identify the contaminants and understand them in ways that would support the lithography project [JF1, 10/19/01; JF2, 3/14/01]. Seeking aid in this way was natural for Foster, since in his dissertation work on Quate's scanning acoustic microscope he had made similar transdisciplinary connections. Frommer provided the know-how in specimen preparation and image interpretation to allow the team to examine first a series of organic adsorbates (simulacra of contamination molecules) and then a series of organic monolayers, including liquid crystals.

Finally, Kirtley started by building his own STM, but with an eye to samples not traditionally used in surface science [JK3, 2/22/01]. Eventually, he became intrigued by the new high-temperature superconductors pioneered by Mueller and Bednorz at IBM Zurich and began veering away from STM (as did John Clarke at Berkeley, a Cancun attendee and John Mamin's adviser before Mamin went to Almaden) (Kirtley, et al. 1987). Calvin Quate, too, became interested in high T_c materials at this time, though he tried to integrate his studies of them with his STM work. Indeed, it was one of the hallmark of academic STMers like Quate, Hansma, Clarke, and their students that they congregated toward new "hot" materials that were receiving sudden, widespread, interdisciplinary attention, rather than concentrating solely on test objects like the 7x7 that had long histories in their own subdisciplines. In Kirtley's case, this interest in high T_c materials pushed him to extrapolate from his STM experience to build a scanning SQUID microscope – an instrument with a superconducting quantum interference device at the end of a probe that could make spatial maps of very subtle magnetic variations in materials (Kirtley, et al. 1995). In truth, the SSM departed widely from the STM that inspired it – as Kirtley puts it, "the only thing in common with the STM is the word 'scanning.'" Nevertheless, the STM should be seen as an older relative of the SSM; this tendency to use one microscope to inspire the design of highly divergent instruments is a characteristic of the Quate and Hansma groups that we will explore in the next chapter. There were other similarities between the Quate and Hansma groups and those of their former students and postdocs within IBM – especially an orientation to outside communities (magnetics experts, the liquid crystals field, semiconductor metrologists, nanotech boosters) much more than the surface scientists. The lesson of these groups is that you did not have to be a surface scientist to do probe microscopy at IBM; but, early on, if you were not a surface scientist, you probably came to IBM from a scanning microscopy group with close ties to the company (in other words, the Quate group), and once there you were likely to do experiments that combined the Quate style with the commercial (as much as research) interests of IBM.

Constructing Microscopes, Microscopists, and Knowledge

I want finally to summarize some salient characteristics of the early STMers' experience of place. The lab was where these people worked – for long durations and at strange hours with ever-present time constraints – as well as ate, passed the time with their fellow inmates, and occasionally slept. More importantly, it was a place where they were processed and transformed, where the raw material of recent Ph.D.s was turned into mature surface scientists and corporate researchers. This was done through an all-pervasive atmosphere of managerial oversight, competition between and within experimental groups, networks of relationships between theorists and experimentalists, and the constant, educative chatter of an often harshly critical surface science community. These combined to discipline researchers – indeed, to make them competent members of the surface science discipline – in ways that affected their every move: how they chose experiments, how they built instruments, how they moved and spoke and held themselves in the lab, how to perceive and interpret images, how to write articles, how to attend to the work of others, how to cobble together and mobilize resources. Their whole habitus was guided – though by no means determined – by the totalizing nature of the big research labs (Bourdieu 1990).

The complex of place, oversight, discipline, and the molding of knowing subjects should call to mind the work of Michel Foucault. The theme of physical space and built environment highlight the intricate balance between the agency of the STMers and of the discipline/institution; certainly, STMers were constrained and molded in many ways, but at the same time they reworked their built environments and brought significant changes to the communities that were shaping them. For

Foucault (particularly in *Discipline and Punish*), built environment materializes power relationships, in ways that facilitate the creation of particular types of subjects (Foucault 1977a).[9] In the physical movement of people through the architecture of institutions – such as the Panopticon – power is inscribed on human bodies and subjects are disciplined into particular modes of behavior.

In some ways this was how the architecture of the corporate labs worked.[10] Bill Leslie relates how Eero Saarinen designed the Yorktown Heights lab with a number of specific purposes in mind – a sweeping arc of natural granite on the outside, beautifully extolling the glory of IBM; inside, a curving hallway running the length of the building, with offices and labs off the backbone; in the hallway, spaces for people to gather and talk; in the offices and labs, no outside windows, but (at least at the beginning) complete transparency to all of the other rooms on the same row; in the cafeteria, a pad of paper at every table, for researchers to continue working through ideas even while eating; and everywhere, omnipresent, the corporate admonition to "Think!", next to a clock, reminding employees of the proximity of the competition (or of their next review).

At Murray Hill, the complex was designed with laboratories along wings, so that anyone walking from their lab to the cafeteria or auditorium or back passed their supervisor's office. The hallway was a site for regulating even mundane behavior:

> One time I came upon Eric in the hallway, I was walking along this empty hallway and he was walking the other way, it seemed like an ordinary situation, we saw each other and said "hi Eric," "hi Bob". We stood and talked for about 20 minutes. I went to my office, he went to his, and within about 3 minutes my phone rang and it was his manager, in fact his manager's manager, a very high up guy, now a Nobel laureate.... He said "Bob, I understand you talked to Eric." I was flabbergasted and I said "Yeah," he said "you told me you wouldn't talk to Eric without my say-so." I had in fact agreed to that.

[9] An excellent explication of Foucault on built environment is Lynch (1991).
[10] There is now an extensive and interesting literature on architecture and science. See the contents of three edited volumes and one special journal issue: Galison and Thompson (1999); Smith and Agar (1998); James (1989); Gieryn (1999); and Ophir and Shapin (1991).

> This manager/scientist was put in charge of shepherding us, these few guys who were going to try and start a company. And since negotiations were so delicate, it had been agreed that none of us would talk about financial details and fussy details in little groups, we would do it through channels and Horst would always be involved if something substantive was coming up. But I never interpreted that to mean we couldn't talk at all, I interpreted it to mean we couldn't talk about percentages or business or something. Eric caused the whole business to fall apart eventually. He soon after disappeared from the field and from science. His meteoric rise was matched by an equally sudden disappearance. [BW2, 5/22/01]

For STMers, the long walk down those hallways was especially perilous in the years when replication of the Zurich results was elusive.

> Kumar Patel [a manager at Bell Labs] of course was very very eager to see this thing succeed quickly. He had gone out to see Cal Quate's operation and a student there had ... built a tunneling microscope ... in about six months.... Kumar came back and announced, if a Stanford graduate student could build an STM in six months an MTS at Bell Labs should be able to do it in a weekend. So we all said "okay, we'll do that." *Two years* later we got atoms. That was a *long* two years, especially that last six months, I really didn't like walking down the halls, I didn't want to run into my management because the pressures were just huge. But fortunately everybody else was having trouble too. Binnig and Roher were having difficulties getting it to work again. [JG3, 2/28/01]

Once replication had been achieved, Bell and IBM wanted to quickly grow the number of working STMs and generate articles as moves in their constant struggle for prestige. One way to do this was to put competing groups in different parts of the same building; they would be constantly aware of each other, always trying to race to the next well-defined surface science goal. This was especially true for the Feenstra and Demuth teams, and to a lesser extent the Golovchenko and Kuk teams at Bell. Another way to grow STMs, though, was to cram researchers together in close proximity to let tacit knowledge flow freely and allow successful design solutions to dominate quickly. At Murray Hill, for instance, STM camaradie was generated in the close quarters of a converted tool shed. One veteran of the shed remembers how its occupants proximity led to a rapid turnover and implementation of designs:

> When I joined the group I started designing and building the low temperature instrument and Russell Becker and Brian Swartzentruber were working on the room temperature instrument.... Russell went this way, Brian went that way in

> terms of design philosophy and everything else, and it turns out that Brian just happened to hit it right. As soon as he did that Russell dropped what he was doing and started working with the machine that Brian had designed and built.... While I was doing the design of my low temperature microscope and trying to implement what the rest of Golovchenko's group – Jene, Brian, and Russell – were developing, on a day by day basis, experience and knowledge, software, a whole body of ... knowledge, sometimes very hard-won, on how to get things done.... I got to see all that happening around me and that very much helped me refine what I was doing.... For instance Brian wrote the software that ran his tunneling microscope – we took Brian's software and adapted it for my work. They had problems with electronics and I learned from their mistakes as I did my electronics because I could see the mistakes and the problems that they ran into. [DE1, 10/11/01]

As we will see in Chapter Five, this close-quarters method of instrument development was more common in the academic labs. In the corporate labs, postdocs and new staff scientists were supposed to carve out scientific identities, sometimes at the expense of others; claiming and reshaping space were important tactics in that struggle.

The basis for many of the STMers' appropriations of their built environment, and the reason why so many of Bell's tunneling microscopists had been housed in such close proximity, had to do with STM design. Early STMs were easily disrupted by even the slightest environmental noise or vibration. When Golovchenko began designing his first STM, he obtained a vibration meter used by architects and walked all around the Murray Hill building looking for quiet spots to put the instrument. When no laboratories were quiet enough, he tried the auditorium, then the acoustically shielded projection booth at the back of the auditorium. Still unsuccessful, he looked for spot away from the main building and found a shed at the edge of the property.

Even for a powerful institution like Bell Labs, though, built environment is a contentious point of intersection with the wider world; and at Murray Hill, the immediate wider world was rural New Jersey townships jealous of their peace and quiet. Within Bell, claiming space was an intensely political matter.

> Jene temporarily got the thing over in an auditorium across the way, which was about an order of magnitude better than the main building. And then there was a huge battle to get it up the hill in the little tractor shed.... Space is fantastically difficult here. Particularly in that time, the township that this

> building sits in ... the front lawn's in New Providence and the main building is in Berkeley Heights. Berkeley Heights was really loathe to allow any new building on the site because of the infrastructure issues associated with it for the town. Any time this site expanded it meant there was more traffic, more sewage treatment ... so the town was keeping a pretty tight lid on that and it meant that outside space was at a premium back in those days. And the Buildings people were not at all happy about giving up a tractor shed for a lab. Well next door to that tractor shed was in fact a lab, owned by one of the directors in my organization. And Jene went after that lab first and tried to get it. The owner of it, Bob Laudise fought them off ferociously. I wasn't involved in that attempt – they went off and did that without telling me, but Laudise was just absolutely livid because I was associated with them. So, he called me into his office one day and just let me have it.... But finally, Kumar was very powerful in those days. He was an extremely powerful manager here and so Kumar was usually not to be denied, when when he wanted something to happen it happened. So they got the tractor shed set up and we were out there for some years. [JG3, 2/28/01]

Similarly, at Yorktown, vibration isolation was a basis for scrounging space and making the built environment one's own.

> It was just too noisy, you could really only do experiments in the evenings and on the weekends. So we would work for a couple days, trying to get analysis stuff ready and analyze data that we had, and then we'd go into a streak where we just worked nights and get data.... We moved to the ground floor at some point because it was just too noisy up here.... At some point CSS ... got new space, and so they moved out and we squatted there for a while. It was this huge space, this enormous lab, and we just occupied a tiny little corner in there and we did a bunch of good experiments. Then at some point we were kicked out of there. So we found an empty office on Aisle 1. This was all pretty informal. One night, we had spotted this empty office so we took all our stuff, our STM, ... at night we wheeled it to the back lab and we started squatting in that office. The office never got converted back to an office again, it became our lab.... Ground floor was important because you're on bedrock there so it's a lot more stable. [RT1, 2/23/01]

Interestingly, claiming space in this way had a double edge. Transforming offices, auditoria, and sheds into laboratories could help achieve experimental goals and demonstrate independence and initiative; but the STMers had to make sure they were still within the disciplining gaze of the corporate lab management.

> These guys had the craziest setup, they had like a clubhouse or a shack that was physically built away from the main laboratory building out on the edge of the parking lot, and it was fantastic because the noise level in the ground was a thousand times less than in the building, so you had to work that much less hard to keep your machine quiet. But it created a weird little cliquey culture. Everyone at bell labs was already a little weird, but these guys were like hermits in their little shack. [BW2, 5/22/01]

> That ended up being a bit of a problem for Russell because he stayed out there to the very end. It's a bit isolating out there.... To be really successful here you have to be a member of the community. You have to be making daily contact with the people around here and the managers have to be seeing you doing things here and hearing about you from other people here. So if you happen to be even just on the other side of the road and get isolated that's very very dangerous. Once I started doing my metrology work I came back into the main building because the vibration issues were not quite as severe, and the technology was getting better, we were learning how to handle it better.... But Russell stayed out there and I think that was one of the things that also hurt him a lot is that he sort of drifted away from the mainstream of the community here. You really have to stay engaged with this community. [JG3, 2/28/01]

Thus, *displaying* both self and instrument to those in charge was an integral part of being a corporate STMer. As these former corporate scientists, one from IBM Almaden and the other from Standard Oil of Ohio, remember, "showing atoms" to upper-level managers was a quick way to win institutional support for a lab group.

> The guy who ran the division would come by, which is a big deal at IBM because everything at IBM is big and it all seems like such a big deal at the time.... This one fellow came by and I was going to give him a demo. He'd typically see five or six labs and hear a pitch from the vice president.... And I was one of the guys. So I was a little bit cocky because I just had the [STM] turned off in the lab, just sitting there idling, so to speak, and he came in, we walked up, I turned the TV on, I started the scanner in one dimension, started the scanner in the other dimension, thing started to take data, and there the molecules were, just sitting there, just like they were the night before, no difference. It was great. [JF, 10/19/01]

> I thought that [STM] would be useful looking at real catalysts, but that was nonsense, as it turns out. What was actually useful [was] ... I can remember being the show-and-tell exhibit for the analytical lab director, who would bring people into my lab and say "Don, show them some atoms." [DC1, 9/5/01]

Display was built into the essence of the corporate research project; the corporate labs were all about building monuments to the company, whether vast architectural ones like the laboratory itself, or nanoscale ones made possible for the first time by the capabilities of the STM.[11] Especially at Almaden, STMers and AFMers exerted themselves to make corporate billboards out of ever-smaller clumps of atoms. As Don Eigler, famed mover of atoms, remembers it:

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[11] The notion that display is an integral part of science is much discussed in the history of early modern science (Shapin and Schaffer 1985; Schaffer 1995).

> Within days of learning how to move xenon atoms around I was either doing experiments trying to figure out what was the ... physics that allowed me to move them around, and/or I was writing "IBM".... I was extremely beholden to people here at Almaden, to the company, to the environment here for giving me an opportunity to be successful to be a scientist and to be part of this team and [writing "IBM"] was just loyalty.... That was one of my ways to give back to the company what the company had given to me. [DE1, 10/11/01]

In one case, the *failure* to manufacture such a tiny monument to the company caused the disciplinary apparatus of the lab to engage. As Randy Feenstra recalls the episode,

> [Russell Becker] eventually got fired from Bell Labs. He was actually the first guy to move atoms, they made a little pile of germanium atoms.... Then the management at Bell came and asked him, "write out Bell Labs." This was really early on, this was practically 10 years before Eigler started moving atoms. Russell thought about it and said "well, it's going to take me hours to do this, it's never going to be practical, this is a waste of time." He said "no, can't do it." So management went off, and then they came back later and they said, "well, how about just doing it in Morse code." ... And he thought about it and he refused.... Anyways, unfortunately that was Russell, and so after a number of years when tough times came to the industrial research labs and there was a lot of rearrangements, he was really more or less blackballed from getting another position at the lab and eventually had to leave. [RF1, 5/2/01]

We see here an extreme rendition of the Foucauldian capillarity of power (Foucault 1977b, 39), the tendency of power to flow into ever more spaces. Research at the big corporate labs entailed expectations of nascent surface scientists. These expectations were made manifest in built space and physical movement at *every scale* – from the architecture and landscaping of the lab, to the positioning of single atoms.

Yet, though these STMers largely accepted the disciplining process that transformed them into good corporate citizens, they also took every opportunity to play the system. Indeed, in matters such as scrounging lab space and experimental materials, taking advantage of the corporation was an accepted way to display drive and initiative. Interestingly, this kind of resistance had its own capillarity – that is, the search for what Erving Goffman calls "secondary adjustments" (i.e. scrounged compensations for being embedded in the institution's transforming apparatus – see Goffman 1961) took place over every imaginable scale as well. The tractor shed/lab at

Bell, for instance, was, on the one hand, the product of an intense technopolitical struggle and a means for management to accelerate STM-building (and the training of STMers) by placing its best people together in a cramped space; but the shed, as we've seen, was also a clubhouse, a site for camaraderie amongst people who were simultaneously resisting and cooperating with the same disciplining system.

The STM's ability to move atoms, too, served, on the one hand, as a means for Bell and IBM management to turn STMers' and AFMers' work to their employer's advantage; but it also provided an outlet for creativity and a means to relieve boredom. The two sides to this activity should not, of course, be seen as separable. Atom-moving STMers use their microscopes to draw stick figures (which appear on the company website) and perform "magic tricks" and spell out friends' initials (micrographs of which appear on the walls of the friends' offices). The art of positioning atoms into pictures or abstract shapes easily transmutes into the science of atomic corrals; and parlor tricks like using two atoms to "magically" move another one around can easily be seen as a secondary adjustment for research on quantum phenomena.

Conclusion

Surface scientists, STMs, publications, and prestige – these were the products of the corporate labs, at least until the recession of the early '90s. After that, the need for quick commercial gains displaced much of the basic research infrastructure of the labs. Many STMers left, and those who stayed generally moved away from STM, often to electron microscopy or AFM. In the '80s, though, when STM was new and surface science was still an exciting field, the corporate labs had operated as an efficient machine for processing people, turning young researchers into accomplished surface scientists. But we have also seen how these people were able to work the system, to carve out a space for their own identities and projects. Today, the big corporate labs are no longer the center of probe microscopy; indeed, the future of these institutions is uncertain. Bell Labs, in particular, is a hollow shell of its former self, and all of its STMers have left or been let go. IBM, after several lean years in the early '90s, has reorganized and mostly recovered, although it will likely never have the preeminence it once enjoyed. There is still some STM and much AFM at IBM, but Big Blue's researchers are no longer the commanding presence in the probe microscopy community they once were. The technology of probe microscopy, and with it the corporate probe microscopists themselves, have matured, and the excitement and dynamism (and also uncertainty) of the '80s have diminished.

Thus, this is a story of both success and failure. The labs themselves, which once brought glory to their companies and supported whole subdisciplines, have faded. Their system of training, and their strategy for expanding STM and AFM, however, have succeeded dramatically. Many of the young postdocs and junior scientists who first got STM and AFM to work are now respected elders in the SPM community. They enjoy the admiration of their peers (for instance, Avouris, Tromp, Tersoff, Feenstra, Hamers, Swartzentruber, and Kaiser have all received honors from the American Vacuum Society) and fill prominent positions at various universities and corporate and national labs. The technique they pioneered is now central to the practice of numerous disciplines, and their contributions to its development are widely recognized and cited. Thus, no matter how total an institution might be, it still engages with the world. For the labs themselves, this engagement with the world (long buffered by the largesse of their companies) became increasingly problematic and even disastrous. For the graduates of those labs, though, the disciplining process they underwent prepared them well for the wider scientific stage.

Chapter Five

Academic Labs and the Transformation of Probe Microscopy

In the first four chapters of this dissertation, we examined early probe microscopy work in corporate and national laboratories. It was there that tunneling microscopy was invented, and work done in those settings made the STM famous and provided the greatest volume of its early successes. Yet there was some early STM research in academic labs in Germany, Switzerland, California, and a few other places. These academic groups lagged early on, yet ultimately it was through them that probe microscopy was transformed from a home-built, high-end technique particular to one or two disciplines to a widespread, off-the-shelf tool relevant to many communities. Thus, we shift our focus here to academic probe microscopy. In particular, this and the remaining chapters spotlight the early California groups, since it was around them that the community of AFMers and non-surface science STMers cohered, and it was through them that the technique became widely commercialized. In later chapters, we will see what happened to the community they seeded; here, though, I want to look at the very local culture they constructed for building and using instruments, and the complicated relationship between microscope-building and the university setting.

The growth of the modern research university has, of course, radically shaped the practice of science, just as the evolution of the scientific disciplines has profoundly shaped academic life.[1] Scholars in science and technology studies have long tracked the mutual construction of these institutions, and the ways in which their joint emergence has underwritten particular kinds of knowledge.[2] Yet this literature still

[1] For the postwar case, one of the most exhaustive studies (and one of the most influential on my analysis) is Leslie (1993). See also Dennis (forthcoming); Owens (1985); Owens (1990); Kohler (1990); Warwick (2003).
[2] Kohler (1994) is one of the best of these, and certainly one of the most influential on this chapter.

understates how closely pedagogy and training can be tied to the content and the practice of science.[3] On the one hand, studies that examine the academic institutions of science rarely make the strong case that physical laws are partly an upshot of the need to train new scientists to use them; and, on the other hand, studies committed to the social construction of knowledge rarely point out how knowledge constructed in campus laboratories bears the mark of the academic institution's training mission.

Thus, there are areas of science studies where greater attention to the mechanics of university research would significantly deepen our understanding of how scientific knowledge is constructed. Most notably, the sociology of scientific knowledge literature seems to cry out for a more pedagogically-oriented analysis. Most of the major practitioners of SSK, including Harry Collins, Trevor Pinch, Donald Mackenzie, and David Bloor, have written extensively about the social construction of knowledge generated primarily *in* campus labs or *by* academic scientists (MacKenzie 1990; Bloor 1978; Collins 1992; Pinch 1986).

Yet the characters in these studies are usually professors rather than students, and the pedagogical aspects of their work are treated as unrelated to the scientific disputes in which they become embroiled. This is moderately surprising, given the Wittgensteinian roots of SSK; as Ray Monk and others have shown, pedagogy (of many varieties) took up a large and vexed portion of Wittgenstein's life and thought (Monk 1990). The *Philosophical Investigations* are peppered with meditations on teaching and learning, and the relationship between those activities and other Wittgensteinian topics such as thinking, recognizing, understanding, meaning, saying, feeling, and showing (Wittgenstein 1997). This chapter, therefore, explores how training was an integral part of the grammar of the language games surrounding early academic probe microscopy. Even when they were not formally teaching or learning,

[3] A nice exception is Olesko (1991).

the students and professors who built these early STMs and AFMs continually strove to construct a sustainable experimental culture that produced both new microscopes and new microscopists. Notably, the recipes they developed in response to this need eventually amplified the influence of this handful of academic groups and became a central – if ever-evolving – part of the construction of a scanning probe community and the attempted integration of that community into both the established disciplines and new transdisciplinary constellations such as nanotechnology and genomics.

The STM Moves West

Unlike the corporate STMers, early academic probe microscopy groups were much more isolated from each other, had to operate with fewer resources, and had no ready-made disciplinary community (e.g. surface science) to judge their work. These groups were – quite self-consciously – embarking on a new venture, one in which they had to cobble together materials, and where they were largely on their own in determining the course of experimentation. Thus, they cultivated a naïve approach to research that, among other things, facilitated stronger rapport between them and the Zurich group than either they or Zurich had with Yorktown and Murray Hill.

Of the academic groups that first pioneered STM and AFM, the most well-established (and the most closely-connected to Zurich) was Calvin Quate's at Stanford. In many ways, Quate exemplified the postwar Stanford created by Frederick Terman, the Stanford Dean of Engineering from 1946-55, and Provost from 1955-65, who promoted early attempts to commercialize academic research by sponsoring former students' start-up companies such as Varian and Hewlett-Packard (Leslie and Kargon 1996). Quate received his Ph.D. at Stanford in 1950 and took up a professorship in the electrical engineering and applied physics departments there in 1961, working for the next decade on microwave research, at a time when the development and commercialization of microwave technology was central to

Stanford's attempts to project its influence throughout the Bay Area. Eventually, he carved out a niche specialty in the interaction between microwaves and acoustics, and soon became a world leader in that area. In the 1970s, he used his expertise in that field to jump into microscopy, helping to invent the scanning acoustic microscope, an instrument related to medical ultrasound technology that used high-frequency sound waves to magnify small objects and even peer inside optically opaque materials (Quate 1976). The SAM seems to have had a brief flowering among academic researchers, followed by many years of productive use in industrial non-destructive testing. Though never a high-resolution microscope (averaging around 5 microns – i.e., 4 orders of magnitude coarser than STM), it did supply many of the needs of the non-destructive materials testing community (in, for example, the electronics industry); and it also laid the groundwork for Quate's later entry into STM [JD2, 2/22/01; JF1, 10/19/01; DR1, 3/14/01].

Our other focus in this chapter is Paul Hansma's group at the University of California at Santa Barbara. Hansma had been a professor in the physics department at UCSB since 1972, where he set up a program in electron tunneling research. Like Quate, Hansma's previous experimental work – in his case, making sandwich tunnel junctions – profoundly influenced the shape of his early STM-building; but also, like Quate, Hansma saw the STM as a significant break in his research, a break signaled by the generational shift of his graduate students as cohorts trained in tunnel junctions were replaced by those trained in STM [PH1, 3/19/01]. Unlike Quate, Hansma did not embark on STM aided by links to Silicon Valley industrial institutions; the UCSB group was, in the beginning, smaller and more peripheral, and as a result its technical solutions were sometimes more idiosyncratic than the Stanford team's.

One well-circulated story about the origins of Quate's interest in STM is that he read a *Physics Today* article on the STM while flying to London in 1982 to accept the Ranke Prize for Opto-electronics from the Royal Society (Schwarzschild 1982; Quate 1986). While in London, he met with Eric Ash, a well-known acoustic and near-field microwave microscopist to discuss the STM; Ash had a former student at IBM Zurich, through whom Quate arranged to meet Binnig and Rohrer. In 1982, the STM was still quite callow; beyond demonstrating the vacuum tunneling signature, its most successful application to date had been an unspectacular and almost universally ignored imaging of atomic steps on gold. It had certainly not yet shown itself to be of any value to surface scientists or microscopists. Quate, though, was not a surface scientist, and more an inventor/engineer than a dedicated microscopist. Indeed, by 1982 he was beginning to lose interest in the SAM, and took the advent of the STM as the occasion for a major shift in his research program. From then on, no new cohorts of graduate students worked on the SAM.[4] Recall that the resolution of the SAM was quite modest, and that its advantages lay more in its non-destructive character and its ability to peer within materials or to offer contrast even in optically or electronically transparent media such as glass. Thus, for Quate, the then-low resolution of the STM was small concern, whereas he found its ability to offer non-destructive information about the electronic characteristics of materials highly intriguing.

Hansma's introduction to the STM, meanwhile, came through Rohrer, who had done a sabbatical at UCSB a few years earlier and knew about Hansma's work with tunnel junctions [PH1, 3/19/01]. Thus, Rohrer directed Binnig to make a side trip to Santa Barbara from a conference in Los Angeles to discuss the Zurich team's vacuum tunneling experiments with Hansma. Specifically, Binnig and Rohrer had always assumed that it would be possible to do tunneling spectroscopy with the STM, and had suggested as much in their early papers (the technical details of tunneling

[4] Indeed, several of Quate's former students and postdocs who worked on SAM – Dan Rugar, John Foster, Clayton Williams, Kumar Wickramasinghe – followed their mentor into STM and AFM.

spectroscopy are outlined in Chapter Four, since corporate STMers pioneered the technique at the same time as Hansma). They had little desire, though, to turn the STM into a spectroscopic instrument themselves. Binnig and Rohrer only ever dabbled in spectroscopy, and never managed to get publishable results. They did, however, inspire a variety of other groups to make the attempt, including Hansma's. So, where Quate approached STM as a new kind of microscope, Hansma saw it as a variation on the tunnel junction, leading to profoundly different approaches between the two groups early on.

From the beginning, Hansma and Quate were in a league of their own. Around North America, throughout the mid to late '80s, groups in physics departments decided to build STMs. Some, like John Clarke, a physicist at Berkeley, or Emanuel Feuchtwanger at Penn State, made early contributions but quickly petered out; others, like Max Lagally at Wisconsin or Barbara Hope Cooper and Wilson Ho at Cornell, started later but became leading contributors. With their headstart and cooperation with Zurich, though, Hansma and Quate were able to mold a West Coast STM community. This community included notables like Clarke; John Baldeschwieler, a well-known chemist at Caltech whose group, like Quate and Hansma's, became associated with a start-up microscope manufacturer; and Stuart Lindsay, a physicist at Arizona State, who, following a successful collaboration with Hansma, formed his own probe microscopy group and, in turn, started his own instrument-building company. In addition, the West Coast probe microscopy community included researchers at the Universities of Arizona and Oregon, UC Davis, and the Battelle, Sandia, Livermore, and Lawrence Berkeley national labs.

As with the corporate STMers, there was initially a long period (almost three years) when the academic groups had little to show for their efforts. Indeed, Quate faced almost exactly the same replication hurdle as the groups at Yorktown, Almaden, and Murray Hill, since he chose to go down the surface science path in building his first STM. From his interactions with Binnig and Rohrer, Quate knew basically how a Zurich-type STM should work, and entrusted the construction of such an instrument to a graduate student, Sang-Il Park, with the mission of reproducing atomic resolution of the 7x7. Because of his acoustic microscopy work, much of the infrastructure for building this STM already existed in the Quate group – piezoelectric scanners, feedback circuitry (and skill in constructing such circuits), output and imaging equipment, etc. Thus, it was in these areas – particularly image production and rendering – that the Stanford team made its first significant contributions to the practice of STM construction. That is, *building* the first West Coast STM presented few difficulties. Indeed, Park (along with current and former students such as Scot Elrod, Alex de Lozanne, and John Foster) accomplished this task so quickly that he caused some corporate lab managers to reassess their own institution's progress. At the time, though, the test of a *working* STM (i.e. one capable of generating credible new knowledge) was atomic resolution, usually of the 7x7. Here, Quate fared no better than the corporate labs; in fact, Golovchenko beat him to it, and Demuth and Feenstra more or less tied. Both the corporate STMers and members of the Zurich team account for the Quate group's slow pace by pointing to its lack of experience in surface science [JD1, 3/19/01; CG1, 11/12/01]. Turning a microscopy group into a surface science group – building a UHV system, hardening the microscopes to survive the ultrahigh vacuum environment, learning the recipes for preparing ultraclean samples of the 7x7 – proved difficult for Quate. Thus, Park was able, in short order, to get his STM to produce low-resolution images – rendered exquisitely using the group's advanced imaging software – but atomic resolution remained elusive.

More or less the same story held for the efforts at Berkeley and Caltech. Clarke entrusted the building of his group's STM to a graduate student, John Mamin, who was completing a dissertation on superconductivity. Mamin continued on as Clarke's postdoc, making slow progress toward a Zurich-type UHV instrument – slow, again, because learning the intricacies of UHV and sample preparation (without being located in an institution such as a corporate lab where surface science was routine and well-supplied) was difficult. Similarly, at Caltech, one of Baldeschwieler's postdocs, Paul West, heard about the STM at a conference, promoted the idea of shifting the group's research, and began building a UHV instrument [PW2, 3/30/01; JB1, 3/28/01]. Baldeschwieler was one of the early workers in both the nuclear magnetic resonance and ion cyclotron resonance fields, so the task of building new kinds of instruments was familiar; but, yet again, turning an instrument-oriented chemistry group into one capable of building a surface science STM was problematic. West visited Switzerland in 1983 and brought back some of the Zurich team's know-how about piezoceramics, tip preparation, and vibration isolation, and – like Park – rapidly built a "working" UHV STM. Going from a "working" to an "atomic resolution" instrument, though, took more time, energy, and interest than he could invest:

> What I did there [at Caltech] was, built it and demonstrated the scan and did tip approach and you could measure voltage-currents, those sorts of things. Demonstrated the tunneling effect.... Before I left I don't think we did any really high resolution images. Getting high resolution images at that time was a real labor of love. You might spend two or three months screwing around with it before you got a high resolution image. At the time I was a postdoc so I had other – I liked the stuff but I also wanted to get a job. [PW2, 3/30/01]

Hansma, meanwhile, decided to sidestep many of these problems by heading in a completely different direction. Seeing that IBM and Bell had the surface science and UHV applications locked down, he moved to adapt the STM to fit with his tunnel junction work. This meant building "squeezable tunnel junctions," devices where two conducting elements (usually gold wires) could be brought very close together and a voltage applied until electrons began to tunnel across the gap (Moreland, et al. 1983). With their use of crossed conducting strips rather than a two-tip or tip-sample configuration, the squeezable tunnel junctions built by Hansma and a graduate student, John Moreland, *looked* much more like traditional metal-oxide-metal sandwich junctions than did any of the previous vacuum tunneling attempts by Young, Teague, Thompson, or Binnig and Rohrer (see Figure 5-1). By the mid ’80s, though, the tunnel junction business was losing steam, and even the advent of vacuum gaps did little to stimulate it. Initially, one motivation for Hansma to move to the new configuration was the potential for an easier insertion of molecules – especially biomolecules – into the gap. While these squeezable junctions were relatively easy to construct, though, they presented great difficulties of interpretation. Here, Hansma had to choose his audience. If he went to the tunnel junction community, he would need to present some gold standard of calibration, much as Ivar Giaever had done for sandwich tunnel junctions in the ’60s (Giaever 1974). If he moved to the STM community, in which image-making was the primary locus of activity, he would probably need to build a full-blown microscope. In the end, what made the decision easier was the more or less accidental discovery that the squeezable junctions worked just as well in air as they did in a vacuum, a discovery with profound implications for Hansma’s local moral economy (Thompson 1971; Kohler 1994) of training students and building microscopes, and equally important ramifications for the STM community as a whole.

Recall that all the initial Zurich work (including Binnig and Rohrer’s patents on the STM) had been done with the assumption that a high vacuum was necessary for electrons to tunnel. In part this was to appease surface scientists (who needed a high vacuum to keep samples “well-defined”); but cursory calculations also indicated that typical voltages between the tip and sample would cause electric breakdown of the air. Some people did benchtop testing of their STMs in air, of course; but for surface scientists (or anyone following their lead – e.g. Binnig or Quate), getting credible

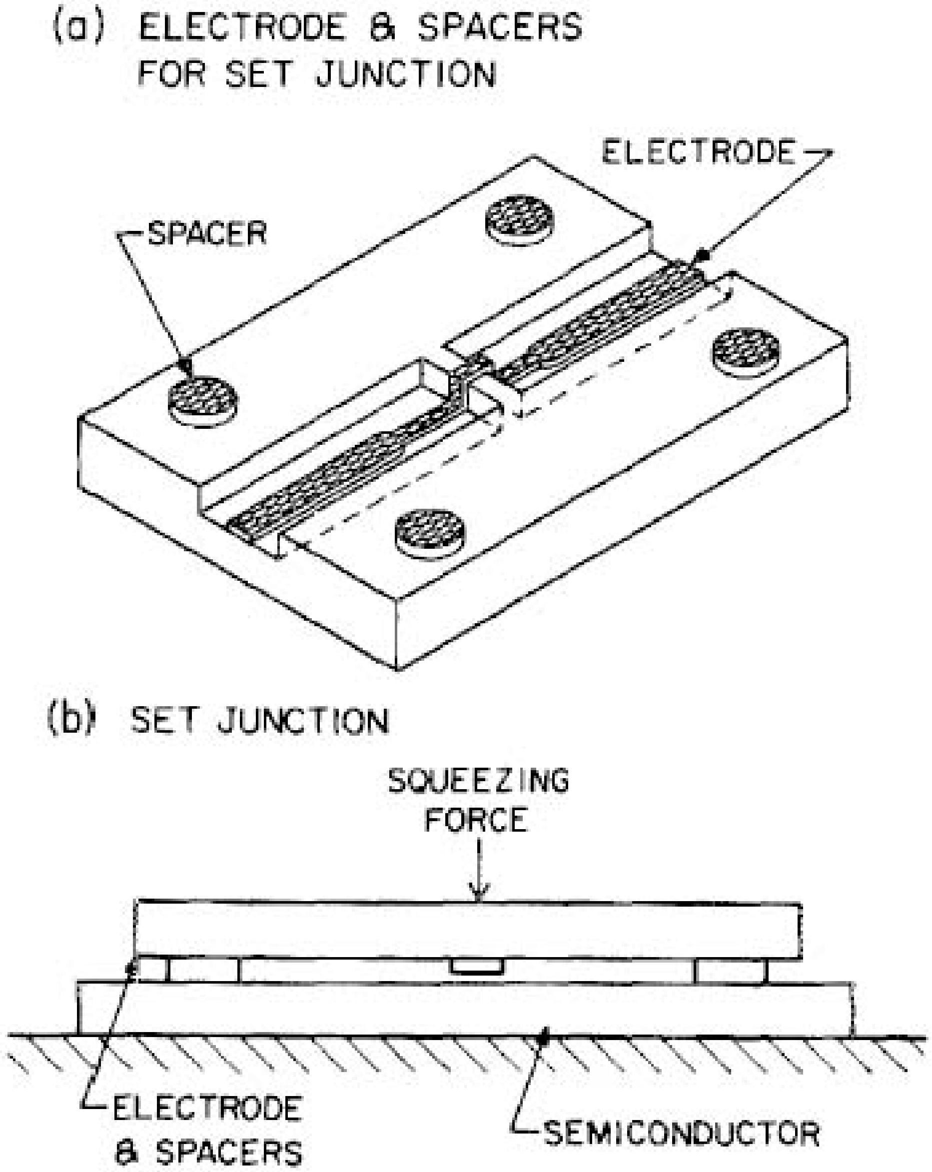

Figure 5-1: Squeezable tunnel junctions. The Hansma group developed this device that could measure vacuum tunneling to a strip electrode, rather than to a tunneling tip. From Sonnenfeld, et al. (1985).

atomic-resolution images in air was unthinkable. Hansma, though, was not trying to image; and what he found in testing his tunnel junctions was that, spectroscopically, air or vacuum seemed to make little difference (Moreland, et al. 1984).

Starting Over 1: Diversification of Approaches

News of Hansma's discovery began to spread just as, in late 1984, the groups that were following the surface scientific path were reaching crisis point. No one in either the corporate or academic labs, could replicate atomic resolution of the 7x7. Even Binnig and Rohrer had not advanced much further in a year and a half. So, as outlined in Chapter Four, Quate assigned a former student from Mexico, Alex de Lozanne (by then a professor at the University of Texas) to organize a small meeting of nascent STMers in Cancun. The Cancun meeting catalyzed the rapid spread of atomic resolution in late 1984 and early 1985, such that there was significant hoopla over the instrument at the 1985 American Physical Society March meeting, and IBM went ahead with its plans for an STM summit meeting in the Austrian Alps at the end of the year. Thus, 1985 is, in many ways, *the* pivotal year for the early development of probe microscopy. Atomic resolution, while still not routine, was at least known to be replicable; air STM was now a possibility; Binnig was in California, developing significant new designs with the Quate group; IBM was mounting an aggressive promotion of the STM, leading to the Nobel Prize for Binnig and Rohrer the next year; both Binnig and Rohrer were beginning to tire of STM in ways that would profoundly shape probe microscopy; and a probe microscopy community was coming into existence, through conferences, collaborations, and the spread of microscope designs.

Even before Binnig's arrival at Palo Alto, air STM had worked its way up from Santa Barbara to Stanford (as well as down to Pasadena [RC1, 6/27/02]). An air instrument offered a great deal to academic groups that saw themselves as oriented more to instrumentation than to surface science (or, in fact, any other subdiscipline) –

indeed, especially at UCSB, that self-perception was co-constructed with the ability to work in air. If air STM could be made routine and credible, it would be much easier to build and tinker with than its UHV cousins. It did not need, for instance, to be built with the special materials and design features needed in UHV; and there would be no cumbersome, expensive, finicky vacuum chamber. Use in air eliminated lengthy pumpdowns and bakeouts needed to evacuate a UHV chamber; thus, unlike surface scientists, air STMers could take apart and modify microscopes in minutes rather than weeks. No vacuum chamber also meant greater bodily access to the microscope. Of particular importance was the ability to eyeball the microscope and the sample and make real-time judgments about (and modifications to) both.

Air STM also meant significant changes in the practices surrounding samples. Samples could now be replaced much more quickly, potentially increasing the pace of experimentation; samples that would not survive the rigors of UHV (including many biophysical samples) could now be examined; and since exposure to air was no longer a problem, sample preparation could now be done away from the microscope, again eliminating much of the cumbersome bulk of a UHV STM. Obviously, samples examined in air would no longer count as “clean” or “well-defined” by the standards of surface science, but the California academic groups quickly used this to their advantage. Air STM opened the doors to a vast array of samples, all prepared less rigorously than silicon or other surface science materials (and, potentially, with less need for skill and expertise in preparation techniques). Thus, air STM offered the grounds for a divorce from surface science, and the reformulation of the tunneling microscope as a general-purpose, easy-to-use tool available to many disciplines and many kinds of samples.

Seeing STM as a general-purpose microscope led Quate and Hansma to reorganize their practice in two significant, enduring, and related ways. First, it meant the lines of development could be moved further away from those narrowly dictated by surface science; thus, an intense diversification of designs, modes, and applications of the instruments ensued. Second, a utility microscope had to be useful *to* someone, so the California groups began searching for users of their instruments. The first of these consequences – diversification – was apparent early in the Hansma group's attempts to operate squeezable tunnel junctions, and then STMs, in environments such as water, oil, air, and other gases and solutions. The move to liquid environments stemmed partly from a desire to carve out a niche distinct from the corporate STMers' dominance of UHV and surface science.

> We realized IBM was doing a wonderful job with Gerd Binnig and Heini Rohrer in high vacuum, and there was no hope of competing with that. They had too much talent and money for us to meaningfully compete. But I thought there would be a niche in making scanning probe microscopes that would work in air and liquid. [PH1, 3/19/01]

It was also, though, an attempt to meet the corporate STMers' rigorous standards of cleanliness, but without the time and expense associated with UHV – surfaces could be kept very clean if they were immersed under solutions with very tightly controlled purities, yet a liquid "cell" containing the sample and probe was much smaller and easier to deal with than a bulky UHV system. This change in design, though, was accomplished with a view to how it might be relevant to new uses and users. That is, because liquid environments are conducive to biological systems, liquid STM was immediately *coopted* to allow the Hansma group to make its work relevant to biologists and biophysicists.

> The first people who conceived these STMs, they were interested in atomic scale physics and so they had the idea that surfaces needed to be atomically clean and these things needed to be in ultrahigh vacuum chambers so that you could keep your surfaces atomically clean. And Paul had two real insights. One is that the biologists need to operate in liquid, in physiological environments, the other insight that he had is there are two ways to keep a surface clean – one is to protect it by enclosing it in a vacuum, ultrahigh vacuum. The other is to protect it by enclosing it in a very clean fluid. [CP1, 3/19/01]

Diversifying the shape and uses of the microscopes only became routine in the Quate lab after Binnig arrived in 1985. The groundwork for this shift was laid partly by the invention of AFM and single-tube scanners. AFM, with its wider array of samples and its more intuitive imaging mechanism, had the potential to be a more interesting instrument to a wider range of disciplines than STM had been thus far; while the single-tube scanner offered quicker, less finicky construction. With AFM and air STM – and with Binnig's presence in the lab – the practices of the Quate group shifted considerably. There was now much more room to choose among different designs, and applications. Here, the pedagogical mission of the laboratory became crucial. Each graduate student carved out particular niches in the technology and use of both STM and AFM, niches that self-consciously built on what had been learned previously about the instruments, but would expand that knowledge base in some distinctive way [MK1, 10/12/01; SG1, 3/27/01; JC2, 3/20/01. Contrast this with the situation of postdocs and junior researchers at the corporate labs. The corporate STMers learned new things about their microscopes, but they were being evaluated on how they used the STM to fill in gaps in the body of surface scientific knowledge. The students in the California academic groups did not see themselves as making such a positive contribution to a formal body of knowledge. Instead, they referenced their projects to their own local group's needs and expertise; and that expertise, rather than accumulating positively, spread outward in a series of shifts – STM in air, in water, in oil, in an electrochemical cell, then AFM and other types of microscopes.

In the flurry of activity between 1983 and 1990, even when these groups might have had reason to reference what they were doing to external needs and to contribute positively to a cumulative body of knowledge, they subordinated those needs to their penchant for a more free-wheeling practice. For instance, in the early days the grant officers at the National Science Foundation and the Office of Naval Research who funded Quate and Hansma gave them some leeway in jumping quickly from project to project in ways that departed somewhat from the original grant proposals. Tunnel junction grants funded STM work, which secured STM grants that funded AFM work. On occasion, students had to scramble to bridge the work they'd been doing to the work they'd been funded to do:

> Dr. Quate is a master of raising money. At one point he got money from DoD to do very small scale data storage, the idea being that a cruise missile would have an ultracompact topographic map of the entire world's stored on it. That's the way he pitched it anyway. It seemed like we worked on everything but that for my first three years. Then he said "oh, we're up for renewal, we have to get some results in," so Tom Albrecht and Morris Dovek and I performed this experiment based on a fluke accident that Doug Smith came up with. Doug had attributed it to something else, thought that he was depositing molecules, but it turned out that the fields were blowing holes in the first atomic layer of the graphite. So Tom and I figured that out and then came up with a way to do a bunch of these, and made a program that let you spell things. In fact, that's sort of how I proposed to my wife, was on a little piece of graphite. So basically, we did that work to satisfy a contract, largely. But then that really set Dr. Quate off thinking, "well, now I can maybe make teeny transistors and, maybe if you have a 10 nanometer line of conductor on an insulator you can figure out what would be the transistor's beta coefficient, what will be its properties and voltage transitions and all that sort of stuff...." We did a lot of quick experiments. But that whole thing, we did it in like three months – made the microscope, made the software, made the electronics, and wrote a paper and Tom and I graduated. Literally, we're writing our theses and it was the last chapter in my thesis because it wasn't going to be there until by the time I got done. [MK1, 10/12/01]

This quote exemplifies three characteristics of the Quate and Hansma labs: their strong orientation to funding (which corporate STMers did not have to think about in the same way); the *ad hoc* way in which they matched their practices to representations of those practices in funding proposals; and their tendency to bounce from topic to topic in search of funding or relevant communities, rather than to positively accumulate knowledge in any one area. As students carried out experiments, they and their group leaders pushed their work toward whatever bits and pieces of "interesting" artifacts or expertise or knowledge could be created most quickly. Group leaders could keep long-term goals (such as those expressed in grant proposals) in mind, and could reference current work to those long-term goals; but the short-term work also generated surprises and data that could hive off. Importantly, as we will see in the next chapter, the array of smaller "results" that was created could be packaged as contributing to "science" *or* "technology" – creating knowledge or advancing the technique – and individual results could sometimes be switched from one pigeonhole to the other. No short-term result carried heavy epistemic weight, but all results were taken as opening possible avenues for quick, generative experimentation.

Quate, who started STM work with more funding contacts than Hansma, had the resources to recruit a large crew of students and set them working on multiple microscopes at once. With many projects ongoing simultaneously in a tight-knit group, people, designs, equipment, and samples could all circulate freely between experiments:

> [Quate] is remarkable in that, unlike most professors, he would bring in students from physical chemistry, from electrical engineering, from the medical group, from applied physics, and physics, and they would work on these projects. He would sort of let us work in twos and threes and the group would get together all the time. Because there was such a diverse group people could say "oh, okay, you're going to do the electronics on this," or "I'm having problems debugging this board, can you help with that?" or "I need to make something for ultrahigh vacuum, get this surface scientist that has tons of experience." There was a ton of different expertise and very open kinds of people. It wasn't like "I've got my project," it was a more team-oriented thing.... Microscopes were made very quickly and debugged very very quickly. [MK1, 10/12/01]

One hallmark of the academic groups was a readiness to use any handy flotsam and jetsam to jerry-rig microscopes together. Found, borrowed, appropriated, and cobbled cultural materiel all made their way into their STMs and AFMs. At Caltech, Baldeschwieler's group obtained atomic resolution using a pencil lead as an STM tip; at UCSB [JB1, 3/28/01], the Hansma group found that surgical tubing offered the best vibration isolation, hand-crushed pawn shop diamonds made excellent cantilever tips, and plucked eyebrow hairs were good brushes for gluing the diamonds to the cantilevers [SG1, 3/27/01]. Group members recall Hansma excitedly coming to work one day describing an ad he had see on television for Gillette Platinum Plus razor blades, and proposing to use the oxide-free blades as tunneling surfaces in his squeezable junctions [BD2, 10/18/01]. At Stanford, this kind of bricolage flowed in part from (or at least accorded well with) Binnig's unique gifts and unusual experimental practice:

> Gerd Binnig is one of the most amazing people in that he comes up with, I don't know, 10 ideas a day. 9 of them suck, but he can cycle through ideas so quickly, and he gets one good a day where the average person might get one per 10 days. He's willing to take a chance, tries crazy ideas, never believes that something is too wacky.... It was about 10 o'clock at night on a Friday night, and Gerd and I were working on this low temperature microscope, and we had an arcing problem or something, the electronics fried, so I said "well, shoot, my fiancée is coming in 10 minutes, maybe we should call it an evening." He goes "no, we have 10 minutes." I swear to god, we got more done in that 10 minutes, it was just unbelievable.... [Another time] literally in less than 24 hours we made this microscope, out of glue. I would go "this glue takes 2 hours to cure." He pulls out a heat gun and says "no it'll take 10 minutes to cure" and sure enough it worked. We just accelerated everything like that, it was really a lot of fun.... The guy can get anything to work, he could take a pile of bananas and band-aids and get it to work. [MK1, 10/12/01]

As this quote emphasizes, experimental work at Stanford, UCSB, and Palo Alto tended to be fast but somewhat chaotic. In the corporate labs, remember, postdocs tried to beat the clock by structuring their work according to the disciplined practices of surface science. In the West Coast academic labs, this disciplinary structure was less apparent. Indeed, participants align the more free-form experimental outlook of the Quate and Hansma groups with their immersion in a wider "California" context. One former member of the Hansma lab remembers the 8 to 5 workday of the UCSB team in the late '80s this way:

> CM: So most of the actual operation of the AFMs was during the day when there would be a group around?
> SG: Right, lunch would hit and boom. This is California, everybody would be out, it would be a good lunch, and in fact I remember coming in and doing stuff at night and it was actually frustrating because I'd start collecting data and I'm thinking "oh maybe this is real" and I'd go "I don't know, can

> somebody else help me?" If someone was there, great, and if someone wasn't then we were stuck. [SG1, 3/27/01]

One of defining characteristics of the Quate and Hansma groups was the substitution of short-term, dynamic frameworks for the more encompassing structures of disciplines like surface science. At Santa Barbara Hansma used rules of thumb to motivate his people and set boundaries for experimental work; for instance, group members recall that Hansma would sometimes reassess a design and declare that the group had until the end of the week to make the design work before moving on to something new. As often as not, this meant that the group's most significant discoveries came on Friday afternoons, as group members worked to squeeze the last results out of an instrument [BD2, 10/18/01]. Hansma also structured lab work with proverbs (Shapin 2001), some of which continue to circulate widely through his network of former students and collaborators; two are particularly worthy of note. One, borrowed from his mentor at UCSB, Herb Broida, said to "do everything as poorly as possible;" i.e., throw experiments together with little polish or gloss, concentrating on quickly getting the basics to work. The second said to "make as many mistakes as you can as quickly as you can;" again, the idea being to head in intuitive directions, test them rapidly, and move on.

Starting Over 2: Outward-looking Focus

This emphasis on intuition, and de-emphasis on disciplinary frames, oriented the West Coast groups (particularly Hansma's) toward activities of self-cultivation. The source of skills was seen less in formal training than in the exploration of new niches of tacit and personal knowledge. Self-cultivating activities, whether inside or outside the laboratory, could contribute to this exploration. At times, this simply manifested itself in the much greater importance of outdoor activity and team sports at Stanford and UCSB than in the corporate labs. Likewise, we can see these cultural values expressed in, for example, group members' activities during the summer:

> Paul [Hansma], he's pretty interesting about this, he takes his summers off, essentially every summer. He really is gone. He'll be incredibly reluctant to come in at all to the university. Not while I was there, but in subsequent years, he would actually have lab meetings occasionally but he'll have them at his house, okay. So he would just stay away for the summer. He would come out to a retreat-like thing on the East Coast, he had some Indian guru type that he was close with somehow. [JH1, 6/10/02

Barney Drake (Hansma's technician), too, often disappeared from Santa Barbara, usually to travel the world or to work as a river guide. Other self-cultivating activities often mentioned by group members include photography, yoga, and meditation. Moreover, we can see ways in which this orientation to hobbies and activities played out in Hansma's method for translating design ideas into working prototypes.[5]

> [Hansma] has a woodshop at home, and he would go home for a few days or over a weekend, and using the tools he had available in his shop, his band saw and his drill press and a few other wood tools, he would carve up a prototype. Basically, it would always be a 2 to 1 size prototype, and he'd use hot melt glue and stuff like that to stick it all together, and then he would bring this in to Barney Drake, the technician, and then Barney would actually reduce that to machine drawings and metal and make up a real prototype.... The fact that he focused on designs that could be built in an afternoon or two in his woodshop with his woodworking skills, it forced an elegant and simple design. What flowed from that is they were elegant and simple to operate, much more ... than the things that had preceded it. [CP1, 3/19/01]

The pedagogical heterogeneity of the UCSB group reinforced their valuing of tacit knowledge. If the ability to acquire *ad hoc* informal skills was valued more than a pedigree of formal, disciplined knowledge, then almost anyone might possess suitable skills (or the ability to develop them). Thus, one could find valuable personnel by looking in unexpected places. Hence, Santa Barbara became a magnet for people with backgrounds that would have seemed highly unusual at other centers of probe microscopy. The Hansma group was not more heterogeneous than other groups by some of the classic sociological metrics (race, gender, ethnicity, etc.). In educational

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[5] For some interesting studies of hobbyists, see van der Grijp (2002); Haring (2002). Both these studies detail the complex circulation of artifacts and expertise in networks that, like the probe microscopy community, include old-timers and newcomers alike and, eventually, manufacturers catering to those participants.

background, and life experience, though, the Hansma group seems qualitatively much more diverse. The range of actors who became enrolled in building SPMs at Santa Barbara included junior high students, river guides, undergraduates, yoga instructors, retirees, and historians [PH1, 3/19/01; HH1, 3/19/01; BD2, 10/18/01; MT1, 2/26/01; OM1, 11/16/01]. This diversity would, I believe, be unthinkable at the corporate labs, or even at Stanford.

Three such people particularly contributed to the group's success. The first was Paul Hansma's wife, Helen.[6] Although she had a Ph.D. in molecular biology, by the time the group started into STM and AFM Helen had been underemployed in scientific work for a number of years – spending her time instead raising the couple's children, and teaching yoga and elementary school science. A persistent theme of the group's research, though, had been the desire for projects that would draw on her expertise enough that she could work at least part-time in the lab. Even from the squeezable tunnel junction days, biomolecules had been part of the group's focus; and as the microscopes were adapted more for biophysics, Helen's contribution became more central. Through the '80s, she re-entered lab work by preparing biological samples, and by teaching some of the rudiments of molecular biology to the group's instrument builders. By the early '90s, she was a full-time professor, with her own projects, her own postdocs, and – importantly – a large crew of undergraduates, all learning how to prepare samples and characterize them with the AFM. Interestingly, *undergraduates* seem to have played a significantly larger role in events at Santa Barbara than anywhere else in the probe microscopy community [HH1, 3/19/01].

The second unorthodox character was Barney Drake, a UCSB alumnus who came to work as Paul Hansma's technician in the tunnel junction days. Drake is

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[6] For some material on scientific spousal collaborations, see Rossiter (1997); Rossiter (1980); Outram (1987); Pycior, et al. (1996).

Hansma's self-described "hands," the person who made the group leader's designs a reality, who coaxed instruments into operation so graduate students could use them, and catalyzed the transformation of these idiosyncratic research microscopes into commercial products. Drake gives a telling anecdote about how he became central to the group's STM work [BD2, 10/18/01]: once, while he and Hansma were on sabbatical at the University of Virginia, working with Robert Coleman, they built a low-temperature air STM. One Friday (again, a pivotal day for the group), Hansma, Drake, and one of Coleman's students were trying to get the STM to work with little success. At that point, very early in Drake's time with Hansma, he had not yet proven himself and did not get a chance to try to make the STM work; but, after 5:00, when Hansma and the students went home, Drake stayed on, thought through the design of the instrument he had helped build, saw a possible problem, and fixed it simply by looking down the long dewar in which the STM was submerged and shifting it so that it would be less subject to incident vibration. Then, in half an hour, Drake got some of the first images ever of charge density waves, and, excitedly, took them to Hansma's house over the weekend. From that point on, as Drake remembers it, Hansma let him run every instrument; indeed, graduate students remember Drake as the one who would come in each day, calibrate the microscopes, and tell them which ones were working and which ones were "fussy." As Drake's anecdote hints, one important characteristic of Hansma group culture was that the labor of putting together a microscope was seen as conferring a more intimate knowledge of how it worked – a kind of tacit knowledge that overcame deficits in formal, disciplined knowledge. Thus, students were encouraged to build and rebuild instruments not only to learn about instrument-building, but to gain a better feel for the images being generated with the microscopes.

The final member of this trio of characters was a retired physicist named Sam Alexander. Late in his career, Alexander had developed a pet alternative to Einsteinian relativity, but had never been able to test his ideas [PH1, 3/19/01]. In retirement, though, he found he had little to occupy his time and asked his former colleague, Hansma, for some lab space and a little equipment so he could carry out his experiments. These consisted of attempts to measure very slight changes in the lengths of metal bars as they were rotated from one orientation to another. The detection scheme he cobbled together from spare lasers and optical tables drew on a concept known as the optical lever.[7] In a laboratory that fed on undisciplined – even fringe – ideas like Alexander's, and depended so much on the quick circulation of skills, it was almost inevitable that his work would be coopted for the group's microscopy research.

The linchpin for this cooptation came from a perceived flaw in then-current AFM designs. The Binnig AFM – with the STM probe mounted on the back as a detector – had spread rapidly to IBM Almaden, Santa Barbara, and a few other places. These first AFMs, though, were notoriously difficult to operate. The user had to manually approach the STM tip down to the AFM cantilever; then, once the STM tip was tunneling, approach the AFM cantilever down to the surface. Then scanning could begin, but it would only last as long as both the STM tip and the AFM probe remained sharp – STM tips being notoriously erratic, especially in air, and AFM probes inevitably degrading over time (especially in the then-prevalent contact mode, where they scrape against the surface). Even if an image were formed this way, the user could never be sure whether they were seeing features the AFM probe measured

[7] An optical lever uses the fact that, when arranged properly, very small changes in the position of a light source can yield very large changes in the point where the light beam strikes a surface. Think about shining a flashlight on a wall – you can turn your wrist only a few inches, yet this causes the point where the beam hits to the wall to shift by several feet.

on the sample surface, or features the STM tip measured on the back of the AFM cantilever's surface. As one former Hansma student remembers it, Hansma saw that STM detection was problematic and worked toward simpler designs, but he also expressed frustration with students' slow progress generating data with these AFMs.

> Paul [Hansma] could never understand why we couldn't get it, I mean we spent all our time doing maintenance. It was like 85% maintenance.... Everything broke. If the electronics didn't break then just putting the cantilever in one of the metal pieces that was holding the cantilever holder would break. Getting one good image was *it*. In fact, Paul used to have a saying, "one paper one machine." Basically, one image, one paper, and then we'd build the next microscope. [SG1, 3/27/01]

Note the proverb of "one image, one paper" – again, this was a maxim rather than a hard and fast rule. It could be brought in to structure work when needed, but the group could also draw on multiple images from multiple microscopes in the publications when that avenue seemed to offer greater opportunities.

Because of the problems with STM detection, Hansma was on the lookout for a detector that would make the microscopes less finicky, and starting to turn his sights toward Alexander's eccentric project in one corner of the lab:

> [Sam Alexander] was working one afternoon a week, for the fun of it, in a corner of our lab, mostly by himself. He went through a series of instruments, each one getting a little better and each one getting him thinking that there might be an effect right at the edge of measurement. Then we built ones that were increasingly better, but the effect would still recede.... It was becoming clear to both of us that this effect didn't exist. Also, we were getting so frustrated with the electron tunneling for detecting the deflection of levers that we decided to combine two projects that weren't working into one that would. We decided to use Sam's optical lever to detect the deflection of our cantilevers. [PH1, 3/19/01]

The combination of the two ailing techniques that Alexander and Hansma and Drake (and graduate students like Scot Gould and Craig Prater and a postdoc, Othmar Marti) worked out was to make the AFM cantilevers reflective (either by coating them or by gluing small mirrors or shards of glass), then shine a laser onto them; the laser would reflect off the cantilever into a photodiode, which would produce a voltage correlating to the deflection of the cantilever (see Figure 5-2). By feeding the voltage into almost exactly the same feedback and output circuitry as an STM, they suddenly had an AFM with significantly fewer moving parts and much less delicate calibration.

The process was not immediate, of course. According to Hansma group mythology, there were seven prototypes between the first optical lever and a routinely working instrument. The most fundamental changes between prototypes involved shrinking the optical lever. The first prototype was mounted on an optical table and the laser spot reflected off the cantilever onto the laboratory wall, but eventually the group got the whole apparatus down to "coffee can size" (in fact, coffee cans were used as vibration shields). The optical lever clearly ranks with the tube scanner as one of the developments that made AFM a technique that the many rather than the few could invest in. Though others – notably Meyer and Amir at IBM – invented similar optical lever schemes almost simultaneously, the innovation quickly became associated with the outward-looking Hansma group rather than with the more involuted corporate AFMers.

Though more plugged into the corporate world of IBM and Silicon Valley, the Quate group resembled Hansma's team in that they worked and played hard, and looked beyond their lab for inspiration and help. For instance, where Hansma found inspiration in woodworking and spiritual retreats, Quate looked to physical activity and the camaraderie of team sports. As a short bio on the IEEE website puts it, "Quate is an enthusiastic outdoorsman. He skis, hikes, jogs, and was an addicted kayak enthusiast until he discovered the sailboard."[8] Clearly, these aspects of the Stanford form of life contributed to Binnig's rapport with Quate and his students:

> Gerd is an incredible "let's just try it" kind of guy. If you actually saw anything that he built, you would have to say that it's a bloody miracle that he

[8] From http://www.ieee.org/organizations/history_center/legacies/quate.html. I base some of my analysis here on Warwick (1998).

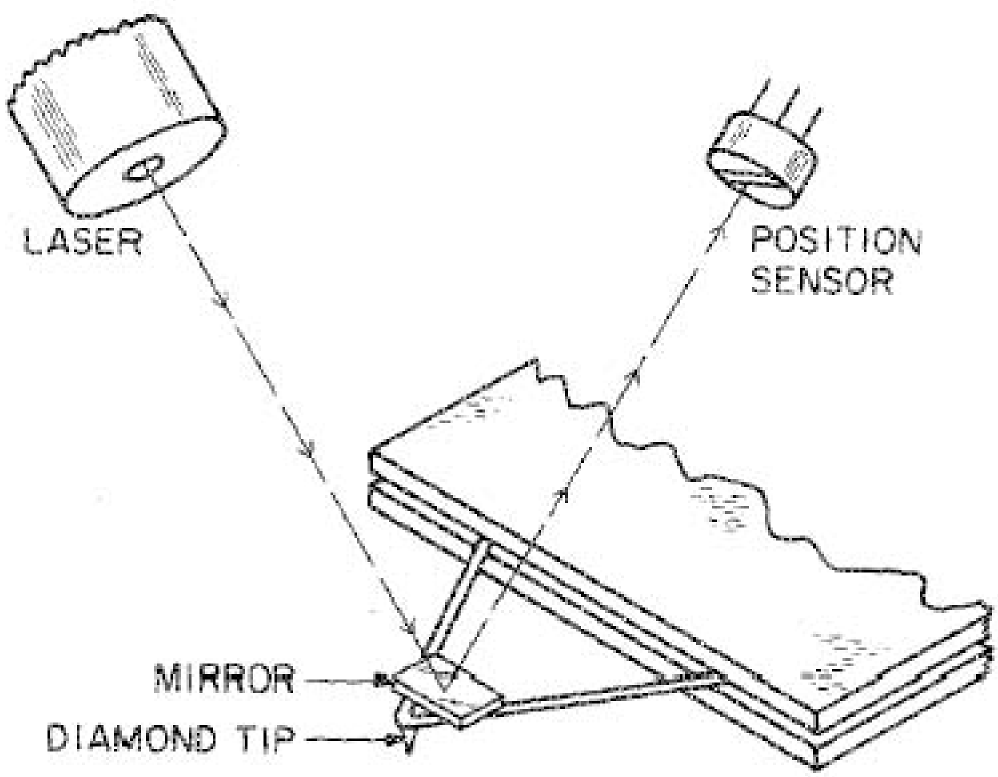

Figure 5-2: Optical lever detection for AFM. Light from a laser bounces off the back of the cantilever and into a photodiode. The output from the photodiode indicates how much the cantilever is flexed. From Alexander, et al. (1989).

> got it to work.... That also added to the fun of being in that group, just seeing [Binnig's] working style and, you know, you have to understand, this is Stanford and, when I joined the group I *had* to improve my volleyball, I *had* to learn how to windsurf, we'd work like crazy and then we'd go off and blow steam. It was a really fun time. [JN1, 6/28/01]

With time, the Quate and Hansma groups started to coordinate both their similarities and differences in a collaborative way. Some Quate students regularly visited Santa Barbara, bringing with them cantilever technology; and some Hansma students made it to Palo Alto, bringing with them optical lever techniques [CP1, 3/19/01; MK1, 10/12/01]. The two lab leaders were in frequent contact, and together they came to a new understanding of probe microscopy. Though IBM and Bell dominated STM, Quate and Hansma quickly became the centers of gravity for the non-surface science STM and AFM community.

Drafting the Disciplines

The same bricolage that informed Quate and Hansma's cobbling together of microscopes applied as well to their search for materials to characterize with the new instruments. They trained their students primarily to build instruments, rather than to use those instruments to learn something new about a particular kind of sample; thus, they often had little idea what samples would be interesting and informative. To some extent, all STMers faced this problem; *no one* fully understood what the capabilities of the microscopes were, so any sample could potentially be the next 7x7. The corporate STMers solved the sample-selection problem by referencing the disciplinary framework of surface science; they narrowed their scope to samples about which they already knew a great deal, and where they could solve particular key questions.

The academic groups, on the other hand, responded with a diffuse, even Feyerabendian, methodology.[9] With instruments constantly being produced at a very

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[9]. That is, the Quate and Hansma group's style of work resonates with Feyerabend's observation that "science is an essentially anarchic enterprise.... Proliferation of theories [or experiments] is beneficial for science, while uniformity impairs its critical power" (1988, 5).

quick pace, students and professors had little time or inclination to learn the difficult sample preparation techniques and formal knowledge associated with a discipline like surface science. Quate tried that path initially, but difficulty learning to prepare the 7x7 led to long delays and microscopes lying fallow for months. Once air operation arrived, both Quate and Hansma often had three, four, five, or more microscopes operating at once, with each instrument being retired after just a few months [MK1, 10/12/01; JN1, 6/28/01; JH1, 6/10/02]; the corporate groups, on the other hand, would have one or at most two microscopes in use, and would use them for years, even decades; even individual samples could last for months or years [DE1, 10/11/01]. With so many surplus microscopes, and so many students trying to figure out what to do with them, the California groups were tempted to characterize anything and everything at hand. "Found" samples became the order of the day:

> So one day I'd come into the lab and there's this bloody instrument cable going into the freezer. "What are you doing?" They'd say, "Oh, we're trying to do AFM of ice." They were trying to AFM of scotch tape. Tunneling in insulating fluids, so we tried to tunnel between Pepsi and Coke.... On the air side it was just crazy, random things that were being tried all the time, and some of them worked and some of them didn't. But, you just dream an experiment and then you'd go do it. [JN1, 6/28/01]

Indeed, the development of AFM (which could image insulating and conducting materials) owed much to the desire to characterize anything and everything. Given that, in 1985-6, STM was becoming routine, while the first AFMs were unutterably difficult, it is a testament to the strength of that desire that these groups stuck to AFM.

Problems with this methodology began to appear early, however. If one characterized samples in the absence of some disciplinary familiarity with their properties, one did not really know what to expect from an image; and if the image disagreed with what one saw in a textbook or calculated on the back of an envelope, then it was difficult to say whether the disagreement was "real" or an artifact of the microscope. This was a problem that confronted Hansma in the first days of air STM;

when he first imaged graphite in air, he found he could obtain atomic resolution, but that the heights of the atoms seemed to be ten times larger than predicted. According to the group's mythology, he held off on publishing anything about graphite while he tried to figure out the discrepancy – only to be scooped by Binnig (Binnig, et al. 1986b), who had found the same thing a little later but, naïvely, had not hesitated to publish even the most counterintuitive results [BD2, 10/18/01]. From that point on, Hansma resolved to follow Binnig by pushing ahead with publications even when all the i's had not been dotted and the t's and had not been crossed.

Thus, Hansma's response to his misstep with graphite was two-pronged: on the one hand, insouciance in the face of possible error; and on the other hand, the mass production of articles to protect against the consequences of that insouciance. From this, first Hansma, and then Quate, developed the outstanding hallmark of the West Coast academic style of probe microscopy: the production of an astonishingly high volume of papers, instruments, applications, and data that were both tremendously valuable to the development of the technique and also of widely uneven quality. As these two former postdocs, one of Quate and one of Hansma, remember it,

> It was all over the map. I've published some things that are wrong, like everyone did. I mean basically we were saying "these are the atoms" and that's a tremendously naïve point of view.... I think there was just an understanding that results in there to a certain extent were not very reproducible. In the early days of course there was enormous pressure to be first to publish something, even if it was wrong. The Quate group, more work was inspired by some of their wrong results. Some of their wrong results were so inspirational that they became very important results anyway even if they were completely wrong.... Such as the inelastic tunneling spectroscopy that was done by Doug Smith on molecules in liquid helium ... that inspired an enormous number of follow-up experiments. Most of what was published in that paper turned out to be wrong.... For me the point of that paper wasn't the details of the spectra that they published and the interpretation, the point was "hey, you could dunk this thing into liquid helium and do neat stuff...." The weakness was that when you actually tried to make it into science, that's where often it went astray. But I don't think that it mattered very much in the end. I mean that inspired so many people to try and do low temperature STM

> experiments of various types that even if they were publishing to say that Doug Smith was wrong it didn't matter. [JN1, 6/28/01][10]
>
> Paul [Hansma] has published a lot of papers that were just terrible.... His favorite papers were the ones where you couldn't quite be sure if you were right or wrong. If you were right it's very important, if you're wrong, pfft, you're wrong, okay.... Paul was just motoring a hundred miles an hour and was leaving a bit of a mess in his wake.... Paul has said that "look, people will forgive you if you're wrong. If you're right occasionally, and you're right on important things, people will forgive you if you're wrong. If you're always wrong, it's not so good, you want to be right, particularly in the beginning, and after that you can be wrong occasionally and it's okay, people just don't hold it against you...." You can actually be wrong but still make an important contribution.... Even this paper on hydrogen bonding, I don't know if it's wrong but it's not clearly demonstrably right. But there were papers like that that Paul published that are incredibly inspirational. [JH1, 6/10/02]

Within the probe microscopy community, this willy-nilly experimental work found differing receptions. Many surface scientists found it unnerving and careless. In particular, the occasional unreproducibility of the builder groups' results seemed to confirm to surface scientists that STM in air was a worse than useless technique.

After 1985, though, dismissiveness from the surface scientists gave Quate and Hansma little pause. The initial phase (when replication was difficult and cooperation between the West Coast academics and the corporate STMers fruitful) was over, and both UCSB and Stanford were drawing adherents. Moreover, through the '80s, almost any STM or AFM image stood a fair chance of reaching publication; images that, today, *everyone* (including their authors) describe as "wrong" or "garbage." Even at the time, some probe microscopists knew some of their "discoveries" would be un-discovered at a later date; it did not matter, so long as they were advancing the community and the technology.

Both Quate and Hansma realized, though, that this could not be sustained in the long run; simply letting graduate students loose on samples was generative and productive, but it led to utterly uninterpretable images. What they sought, then, were people from outside the groups who knew what samples were scientifically

[10] The article referenced is Smith, et al. (1987).

interesting, who possessed the skill to prepare those samples in such a way that they would yield results under the gaze of an STM or AFM, and who were expert enough to interpret the resulting images in a way that would lend the academic groups credibility. Like Hansma's proverbs and rules of thumb, interdisciplinarity offered these groups a means to structure and make sense of the creative, generative chaos of their students' instrument-building practice.

Note how the academic groups differed from the corporate STMers. At IBM and Bell, such a large proportion of the surface science community was in one place that STMers did not need to look outside their institution (or, if they did, they looked only to the competing institution) to have their work judged and validated. Quate and Hansma, and the few other academic probe microscopists, were essentially alone in their endeavor – they were surrounded not by surface scientists, but by biologists and geologists and physiologists and chemists and electrical engineers and so forth. To have their STM and AFM work seem credible or even intelligible to anyone in their own institutions, these groups had to build some kind of interdisciplinary bridge.

This kind of pattern had a fairly long history in the Quate group, going back to the acoustic microscopy days. Thus, when air STM, and then AFM, became available, Quate started sending graduate students out to collaborate with other groups; and he brought in postdocs to help graduate students figure out how to choose and prepare their samples and interpret their data (Kirk, et al. 1988; Richter, et al. 1990; Dovek, et al. 1988). At Santa Barbara, interdisciplinary collaboration was partly associated with the desire to draw on Helen Hansma's training; as Paul Hansma puts it, he "married into biology." The group's first joint project, though, came when Hansma and Drake took a sabbatical at the University of Virginia to work with Bob Coleman, one of Paul's friends from graduate school. Coleman studied charge density waves at the surfaces of layered compounds (compounds which are relatively easy to keep clean in air), so it was a natural project for Hansma and Drake to build a low-temperature air STM and try to integrate it into Coleman's research (Coleman, et al. 1985).

The Virginia interlude showed that Hansma and Drake could design and build an instrument with a specific type of sample and application in mind, and collaborate with a practitioner in the field most associated with that sample in order to generate credible knowledge. This success encouraged Hansma to bring collaborators to Santa Barbara to interact with his graduate students. Fortunately for the UCSB group, in the late '80s, many many people wanted to come to Santa Barbara and strike up such collaborations [PH1, 3/19/01; HH1, 3/19/01; BD2, 10/18/01; JH1, 6/10/02]. By 1986, Binnig, Quate, Hansma, and others like them had shown that the new instruments might be applicable to a wide range of samples and might be compatible with the work of a wide range of disciplines. Moreover, they had shown that they were willing to share instrument designs, helpful hints, and the various tricks of the trade – the small bits of tacit or not-so-tacit knowledge that newcomers needed to get started. As a result, UCSB (as well as Stanford) became the center of a dense network within the probe microscopy community. Almost everyone who was already in the field was in frequent contact with Hansma and sent their preprints there; and almost everyone who wanted to break into the field visited Santa Barbara and/or sent their students and samples there:

> Paul [Hansma] also was a nexus for information in probe microscopy. Everyone who was in the business would send him preprints. And he'd circulate those preprints. So we really saw just a huge amount of information very very quickly, long before it was published and it was just an incredibly rich environment. There were always a lot of visitors and things like that. I don't know how it is now but I've really seen few scientific environments that were quite that rich and rewarding. [JH1, 6/10/02]

With all of these visitors, samples, and preprints flowing in, time became even more of an issue. As usual, there was a Hansma maxim to guide the group's behavior: visitors could come to Santa Barbara for "an afternoon or six months." By the early '90s, the tide of "afternoon" visitors was so great that one graduate student was designated a dedicated lab tour guide. Most of these visitors saw the instruments, talked a little with Hansma, and perhaps brought with them a sample to be quickly characterized; later on, as the instruments were commercialized, many of these people were prospective customers of Digital Instruments (the Santa Barbara-based STM and AFM manufacturer) and the Hansma group acted as a kind of scientifically-accredited sales adjunct for the company.

It was the six-month visitors, though, who really powered the experimental engine of the UCSB team. As in the corporate labs, many of the people who filled this role were postdocs or young professors – people with disciplined skills and knowledge, but looking to use the new and relatively untried microscopes to carve out a productive niche within that area [PH1, 3/19/01]. In some cases, professors at UCSB (such as Joe Zasadzinski or Galen Stucky) might share a student or a project with the Hansma group; in other cases, professors at other universities (such as Carlos Bustamante or Hermann Gaub – both biophysicists) sent their postdocs to Santa Barbara to forge a collaboration and then bring the technique home; in yet other cases (such as Stuart Lindsay, a biophysicist, or Bruce Schardt, an electrochemist) a relatively young professor came to Santa Barbara for a while to learn about the technique then return to their home institution to found a new center of STM or AFM. The mass of the "six month" (often really a year or even two) visitors, though, were self-guided postdocs – people who had heard of probe microscopy, found out who the best groups were, and contacted Hansma to join his lab.

The first in this line was Othmar Marti [OM1, 11/16/01], whom we met in Chapter Two as the builder of the famous "Blue Box" at Zurich. When he came to Santa Barbara in 1987-8, he filled the same niche in skills and experimental persona that Binnig did at Palo Alto (with Barney Drake playing the Gerber to Marti's Binnig).

> [Othmar Marti] is a character, oh he is a character.... He had expertise in everything. He was great as a machinist, they hated him in the machine shop I believe because he would take no precautions, he would just get in there and burn and cut and drill and press and the whole thing, chips were flying everywhere and at least he wore glasses since he probably never wore safety goggles. He'd come back and his hair's coated with the stuff.... Othmar did one of the first machine jobs on the microscope that we buy commercially [from Digital Instruments]. But Othmar was out of control so Barney [Drake] went down and did a good job and the rest is history. [SG1, 3/27/01]

Unlike most of the later postdocs, though, Marti (in his work, knowledge, and approach) overlapped significantly with Drake, Hansma, and the lab's graduate students. He was, in Hansma's parlance, a "builder" [JH1, 6/10/02] When Marti first arrived, the group was still most preoccupied with figuring out how to build working instruments, and he was instrumental in encouraging the switch from STM to AFM, and in making the optical lever detection system for AFM reliable. By the time he left, the focus had shifted to figuring out how to use AFM in new applications, and how to make connections to new disciplines. Thus, he was followed by a steady stream of postdocs whom Hansma called "runners" (i.e., people whose main task was to run instruments on new samples, rather than design and build new microscopes): Jan Hoh, Manfred Radmacher, Hans Butt, Gernot Friedbacher, Peter Dietz, Irene Revenko, Roger Proksch, etc. – mostly drawn from biophysics and molecular biology. Most of these people (especially in the early days) came to Santa Barbara with little or no firsthand experience with AFM (or its cousins). Usually, they had seen a few images, read a couple articles, talked with an adviser – i.e., informal, casual acquaintance with the technique, and vague but high expectations about what it could do. When they got to UCSB, they usually familiarized themselves with the technique by working up to the now standard test of instrument and instrumentalist – atomic resolution of graphite in air. Then, they would start to prepare and characterize their own samples – gap junctions, Langmuir-Blodgett films, amino acid crystals, proteins, DNA, biominerals.

In the process of figuring out how to image the "epistemic things" (Rheinberger 1997) of their own disciplines, these postdocs would consult with other members of the lab. If the postdoc ran into trouble imaging, they could always ask a builder (who possessed the subtle knowledge of the instrument thought to come with building one); at the same time, they would teach the builders some of the rudiments of specimen preparation and biophysics, and give them some idea how to interpret the images of their samples [CP1, 3/19/01; JC2, 3/20/01]. As in Galison's description of "trading zones" (Galison 1997), there would usually only be a local pidgin, rather than a full-blown technical language, common to both postdocs and Hansma's students and technicians; postdocs would bring with them sophisticated knowledge of how to prepare and understand certain samples, while the students and technicians would turn to college biology textbooks to figure out what they were looking at or what they should look at next (since the builders saw these samples largely in terms of how they could enlarge the capabilities of the instrument, rather than as objects of their own expertise). The runners allowed the Hansma group to speak more authoritatively about a wider range of samples, and therefore more credibly to a wider range of audiences. This was helpful in getting articles accepted, in bringing in more visitors, in inducing users to buy instruments based on Hansma's designs, and in getting funding for more work – all the things Bruno Latour and Steve Woolgar describe as part of the "cycle of credibility" (Latour and Woolgar 1986).

> By the end of the summer I had collected a bunch of data in which the adhesion between the tip and the sample could be modulated depending on the solution conditions. I was doing that mostly because I was trying to understand what was going on, but when Paul came back [at the end of the summer] it turned out that he had been getting money from NRL, but that NRL program was getting phased out, so he was actually in a tight financial spot. But he came back and he looked at that data and we talked about using the AFM to do quantitative adhesion, things like that. He actually took that data and went back to the NRL and said "well look, this is what we want to do, we want to use AFM to study adhesion." That was a thing the NRL's incredibly interested in, so he got a big chunk of change actually from the NRL to move

> all that research over to adhesion, so he was very happy with me, he was incredibly happy with me, because I'd inadvertently if you will, but still, I'd collected a bunch of data in my first three months there that got him I think a fair amount of money. So I think about then he said "well, okay, here, now you can stay" <laughs>. "I'll find money, okay." [JH1, 6/10/02]

Several tangibles emerged from the postdocs' long visits. First, a series of journal articles, usually coauthored by a postdoc and a graduate student, each showing how AFM could be applied to different systems and samples (Hoh, et al. 1992; Hansma, et al. 1994; Manne, et al. 1990; Friedbacher, et al. 1991; Dietz, et al. 1992 Hansma, et al. 1996; Proksch, et al. 1995; Hansma, et al. 1993). In writing these articles, graduate students showed they could build a successful instrument *and* work with a user of that instrument closely enough to understand their disciplinary needs and get the instrument to reveal something new about their samples. Postdocs, meanwhile, showed they could attach themselves to a new technique and integrate it with the traditional knowledge and skills of their discipline. Finally, the visitor left Santa Barbara, taking the technique with them and founding, joining, or rejoining new centers of probe microscopy.[11]

Thus, the STM and the AFM were routinized into the life of these academic groups, in ways that were reflected in the design of the instruments. Keep in mind, though, what "routine" meant in these "builder groups" – it meant they could routinely build more microscopes, funnel more graduate students into constructing new variants, attract more postdocs, and bring in funding to build new kinds of STMs and AFMs. Probe microscopy was still not "routine" in the sense that it could be used on a daily basis by a wide variety of researchers. That was a goal of the builders – indeed, it partly motivated the drive to bring in postdocs. Up to about 1989, though, while Binnig, Quate, and Hanmsa *had* managed to help a few other builder groups get

[11] Again, as in Chapter Four, an example of a "postdoc cascade" (Kaiser forthcoming-b).

started, they had not managed to spread the technique amongst people who did not build their instruments.

We will see the successes and failures of exporting probe microscopy into new contexts in Chapters Six through Eight. It was not inevitable that STM and AFM spread this way; indeed, the technique moved slowly out of the corporate labs, and only to similar institutions or to isolated academic groups (who then built their microscopes). The tendency at Stanford and UCSB to circulate the technique among more diverse communities, and to apply it in more diverse ways, was closely tied to the Quate and Hansma groups' pedagogical mission. In the corporate labs, young surface scientists were surrounded by a substantial portion of their discipline, and supported by resource-rich corporations; this combination yielded a more formalized, involuted way of doing STM. In the academic labs, graduate students and postdocs were surrounded by a variety of disciplines, and made do with meager resources. Where the corporate STMers followed trusted paths for establishing credibility in surface science, the academic SPMers had only *ad hoc* recipes to make their microscopes relevant to various communities.

From this comparison, we can pick out sources for the academic SPMers' practices. Beholden to no single discipline, they instead built links with every discipline they encountered; having few resources, they built instruments from cobbled-together materials and then used those instruments to look at "found" objects; not having a recipe for constructing new kinds of instruments, they tried a variety of methods and built a variety of microscopes; seeing the dangers of too much generative chaos, they imposed temporary order through maxims, proverbs, and borrowed disciplinary frameworks; not tied to any particular discipline, they looked inward to cultivate new skills, and they looked outward to find people with unusual insights, materials, and skills; in looking outward, they created an SPM community. All these characteristics were closely bound to pedagogy. Most importantly, the academic groups were surrounded by a multiplicity of disciplines because they were located in modern research universities; and while Quate and Hansma could have isolated themselves from that environment, they saw they could use it to improve the technique and give their students useful experience. Though negotiating meaning with new disciplines was not always easy, it was, in the end, extraordinarily valuable. As we will see, the Quate and Hansma groups laid the foundation for the immense popularity of STM and AFM; and, at the same time, they trained cadres of graduate students and postdocs whose adaptability and resourcefulness have made them leaders in both academic and commercial probe microscopy.

Chapter Six

DNA Debates and the Shift to AFM

By 1990, STM and AFM were becoming routine, reliable techniques; and, though not yet commonplace, they were diffusing rapidly into new communities. This diffusion was a product of three mechanisms: the “postdoc cascade” spreading out from the corporate labs and the Quate and Hansma groups; an influx of new people building microscopes; and the availability of the first commercial STMs and AFMs. Routinization brought not just new participants, but a change in attitude. Probe microscopists talk about the period from 1990 to 1992 as a tipping point, when, for instance, the papers at STM Conferences became overwhelmingly concerned with what authors *saw* with their microscopes, rather than what they had *done* to build or innovate their instruments. In some ways, this influx of newcomers and change in attitude were exactly what early STMers had sought: IBM, for instance, wanted a large STM community that would reflect glory back on Big Blue, while Quate and Hansma sought to enroll new allies in order to generate credibility and hence attract funding, students, interesting samples, and further collaborations.

Expansion came at a cost, though. The probe microscopy community became a more diverse place, where participants had less and less in common. Small frictions that could be overlooked when STMers had been focusing on building reliable instruments became more intractable as researchers started to debate the knowledge created with those instruments. Newcomers’ values sometimes conflicted with those of the old-timers, and old-time STMers’ values began to shift as they saw their community changing. What resulted were a series of disputes and controversies, culminating in an argument in 1990-2 about whether air STM could atomically resolve, and perhaps even sequence, DNA.

Scientific disputes have, of course, been central to the science and technology studies literature. In the sociology of scientific knowledge tradition, especially, controversy has been a powerful tool of social analysis, a mallet for cracking open epistemic bottles and taking out the ships.[1] In other areas of S&TS, controversy is methodologically less crucial, yet still useful in telling engaging stories about how the weave of seemingly universal, timeless knowledge often contains idiosyncratic, non-conformist threads.[2] So far in this dissertation, we've seen only the minor frictions of a nascent community, rather than outright controversy, even though many participants found some of the knowledge created with the new STMs and AFMs to be wrong or inadequate. Controversy about questions of knowledge only broke out when the social order of the small early STM community began to break down. The DNA dispute of 1990-2 is important because it paved the way to a new social order within a larger probe microscopy community. Analytically, this controversy is interesting because some participants used technological measures (what is the best way to design a microscope? which is more useful to the majority of researchers, STM or AFM?) to answer scientific questions (can an STM sequence DNA?), and because the commercialization of the instruments offered a means to reorder the probe microscopy community and quiet the dispute.[3]

Cultures of Controversy in the SPM Community

Before we examine this dispute, we should look at the different kinds of STMers (and AFMers) involved, and how they preferred to handle controversial

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[1] The "ships in a bottle" metaphor is from Collins (1992, 5). See also Pinch (1986) and Bloor (1978). MacKenzie (1990) is an outstanding controversy study that aims SSK in much the same direction as this chapter – i.e., an analysis of a simultaneously scientific and technological controversy.
[2] Latour, for instance, transformed his lab study at the Salk Institute (Latour and Woolgar 1986) into an analysis of controversy in *Science in Action* (Latour 1987). Haraway's *Primate Visions* (1989), too, is by no means SSK, yet several of the chapters clearly focus on SSK-type controversies.
[3] I draw on Lynch and Bogen's (1996) concept of "sleaze" to analyze this kind of exploitation of interpretive flexibility.

science.[4] To see why and how disputes arose in the probe microscopy community, it is necessary to understand that different kinds of STMers belonged to different cultures of controversy, and that different ways of handling and exaggerating or minimizing disagreement profoundly shaped the development of the technique. The corporate STMers, for instance, rarely seemed interested in bringing scientific disputes to a boil. Surface science, particularly at the corporate labs, could be ruthlessly critical, with a stringent policing of sample preparation and other practices. This minimized full-blown public controversies that could reflect badly on the corporation. When discrepancies between different researchers' results did become public, there seems to have been little hurry to resolve them; surface scientists assumed that, with time, "the truth will out" (Gilbert and Mulkay 1984). So long as a researcher's work had been vetted by his or her institution, uncertainty need not spark controversy. For instance, while there were disbelievers in the initial Zurich 7x7 results, their doubts were expressed mostly through backchannels within the walls of the Zurich lab – there is little published material to indicate anyone ever questioned Binnig and Rohrer, and once Binnig and Rohrer had the backing of IBM's surface scientists, naysayers quieted down. Both skeptics and believers took the Zurich work more as a new move in ongoing debates about the 7x7, rather than as an open wound needing closure. This is, perhaps, all the more remarkable given how little Binnig and Rohrer knew of the language and techniques of surface science.

One hallmark of "controversy" in surface science in the '80s was the extraordinary length of time needed to come to closure. Controversy studies in the sociology of scientific knowledge tradition usually depict debate as a no-man's land of ambiguity and uncertainty, with closure swooping in quickly to set everything straight

[4] See Simon (2002) for a recent controversy study that examines how different participants orient to controversy and closure.

again. As far as I can tell, this simply does not apply to surface science; for most problems, surface scientists seem rather to have enjoyed drawn-out, low-level disagreements. Solving reconstructions, for instance, resembles horse-racing more than anything, with various theorists and experimentalists placing bets on different atomic structures and letting time (usually on the scale of years or even decades) yield up a winner. Indeed, one way in which STM significantly disrupted surface science was by helping to bring such horse races to a close much more quickly (leaving surface scientists without a favorite pastime).

Thus, the corporate STMers were usually content to let disputes simmer quietly or disappear altogether. When, for instance, the Chiang/Wilson group at IBM Almaden and the Hamers/Tromp/Demuth team at Yorktown came to contradictory interpretations of images of the same reconstruction (Wilson and Chiang 1987a; van Loenen, et al. 1987), their disagreement is remembered more as grounds for humor and hurt corporate pride than heated dispute [SC1, 3/8/01; RT1, 2/23/01]. Notably, this indulgence in controversy shaped how the corporate STMers approached disagreement and dispute with other corners of the STM community. In particular, though the corporate STMers had doubts about the practices and results of early colleagues at Santa Barbara, Stanford, Berkeley, Penn State, and other universities, they kept their reservations to themselves. Corporate STMers believed that since Binnig, Quate, Hansma, and other early "builders" were not surface scientists, they did not know any better, and that time would eventually set them right.

Like their corporate counterparts, members of the builder groups (Binnig, Quate, Hansma, and their associates and emulators) tried to minimize outright controversy wherever possible. Here, though, they were in a more precarious position. Three aspects of "builder culture" seemed to invite controversy. First, their rapid, high-volume, mass-production of instruments, papers, and samples meant that some materials were characterized quickly and with little sample preparation or disciplined knowledge about how to interpret images. Second, since the materials characterized were relevant to a variety of disciplines, builder groups found themselves generating knowledge for audiences whose language and practices they did not always understand. Finally, the rhetoric of the builder groups emphasized that mistakes were natural – indeed, that they were necessary and important tools of learning.

The leaders of the builder groups recognized some of the possibilities for controversy in this style of work. In pushing the envelope of the instrumentation and its application, they continually risked sparking disputes with practitioners from other disciplines. As we've seen, visitors (mostly postdocs and young professors) to the builder groups lent them the knowledge, the technique, and the credibility to navigate these dangers. Interestingly, the Hansma group enrolled visitors (and thus avoided controversy) more successfully than Quate's team. Quate's students were somewhat less directly involved with their extradisciplinary collaborators, and so sometimes wandered into minefields. For instance, Doug Smith did work on the vibrational spectroscopy of sorbic acid that was clearly an attempt to beat surface scientists to the punch on an important question in their discipline (Smith, et al. 1987). Yet in using an air STM, and in departing from many of the sample preparation methods deemed necessary by surface scientists, he seemed to invite dispute. As it happened, questions about his paper were raised at conferences and in the literature (Hamers 1989; 1996); and it was only in 1998 that Wilson Ho became the first researcher to do scanning tunneling vibrational spectroscopy of a single molecule in a way that was accepted by the surface science community (Stipe, et al. 1998).

In the area of superconductivity, too, Quate's people ran into heavy weather. One student's rather earnest plea in his dissertation nicely illustrates this point, and, indeed, sums up much of the argument of this chapter:

> As the experiments on LaSrCuO were being completed, the world was jumping from the announcement of superconductivity at temperatures above the boiling point of liquid nitrogen. YbaCuO was now the material of choice. So of course there was a rush to put samples of this new high T_c compound under the STM.... By the time Chu had discovered superconductivity at 90K just about every living scientist, it seemed was involved in superconductivity.... Though this was an exciting time, the scientific integrity, or at least thoroughness, was not at its usual high level. I will use my own work as an example of this hysteria. We were anxious to publish this tunneling data, as we knew that several other groups were examining the same materials with STM's [*sic*]. The LaSrCuO experiment was repeated just twice to verify the gap value.... The paper was written in two days and simultaneously we were performing the same experiments on YBaCuO. Again, the results were very hard to reproduce, but once we reproduced the gap that we felt was the best, we started writing. Now there were only two days until the March meeting so it had to be written quickly and, as a result, the interpretation was not entirely accurate. (Kirk 1989, 127-8)

This quote highlights many of the phenomena I want to explore in this chapter: an exciting new field, a rush of newcomers, rallying around a new "material of choice," a palpable sense of excitement, and at the same time frictions and disputes arising from the influx of newcomers.

After these disputes, Quate seems to have tired of controversy and sought out less contentious areas of research. Thus, he began to shift more and more toward technological and industrial applications of STM and AFM – particularly AFM lithography – and also used his group's work with cantilever fabrication to enter the world of micro- and nano-electromechanical systems (MEMS and NEMS) (Emch, et al. 1988; Quate 1992; Park, et al. 1995). Notably, this shift in practice entailed a shift in audience. By producing fewer images for publication in scientific journals, Quate steadily thinned his ties to the scientific communities where he had run into trouble; instead, by working on potentially marketable AFM-based technologies (such as multiprobe data storage systems) he bound his group's fortunes more closely to high-tech corporations such as IBM. At the beginning of the debates outlined in this chapter, the Quate group was still a commanding presence in the scientific STM community; by the end, the Stanford team was contributing relatively little to debates about the proper interpretation of STM and AFM images, and contributing much more to questions about how to turn the AFM into a commercial data storage device.

The Hansma group, meanwhile, remained pivotal to the development of the probe microscopy community, while largely avoiding the disruptions of controversy. Like Quate's team, the Santa Barbara people published results quickly, some of which they and others look back on as "wrong" or "garbage" [JH1, 6/10/02]. Hansma, though, deployed several strategies that minimized the harmful effects of any missteps. For one, he built a more extensive and more heterogeneous network of allies in various scientific communities than Quate did. This network functioned as an effective safety net for Hansma. The biophysics and molecular biology postdocs who began visiting in the late '80s and early '90s showed graduate students how to prepare samples in a credible way and filtered out particularly rash claims from articles. When they left Santa Barbara, they formed their own groups and continued pioneering biophysical applications for AFM; this, in turn, made it harder for life scientists to discount biological applications of probe microscopy on the basis of any single disputed result.

One of Hansma's other controversy-avoidance strategies was to present a moving target. With each graduate student developing some new wrinkle to the technique, and each visitor adding a new application to the group's repertoire, the Santa Barbara team was continually moving from research topic to research topic. By the time any potential critics could respond, the group would already be demonstrating the usefulness of the technique in some new area. More importantly, the group integrated its development of the instrumentation and its development of new applications seamlessly enough that any piece of work could be alternately cast as "scientific research" or "technological innovation." Some results might appear in publications like *Biophysical Journal* (oriented to "runners") and others in the *Review* *of Scientific Instruments* (oriented to "builders"), but the same graduate students were involved in producing both. Results that were criticized could be shifted from one category to the other; in most cases, this meant that dubious "scientific" results were recast as significant demonstrations of "technological" improvements.

Finally, it is clear that Paul Hansma was extraordinarily astute about which research directions to pursue and which results to publish. Time and again, we see that he entered fields just before they became hot, and left them before they began to attract skepticism. In some ways, this was a classic self-fulfilling prophecy – the Santa Barbara team had been at the center of STM and AFM long enough that when they started something, people watched and often emulated them [SL1, 1/6/03; HG1, 11/14/01]. Conversely, when the Hansma group left a field, that topic's immunity to controversy often weakened. This was true both for specimen choices (e.g. graphite) and major design changes (e.g. optical lever AFM). It's important to note, though, that these prophecies were self-fulfilling partly because of the network Hansma had built – by enrolling a large number of collaborators and associates, he ensured an audience for his innovations and experimental choices. Though these associates worked independently, their debt to, and respect for, Hansma made them likely to follow his lead, especially at times when seemingly intractable problems beset the SPM community.

Graphite and Experimental Vertigo

The strongest ties in the Hansma and Quate networks were between the Stanford and UCSB groups, their close collaborators, former students and postdocs (the "six month visitors"), and, increasingly, the start-up SPM manufacturers who employed many of their former personnel. Beginning in the late '80s, though, "afternoon" visitors – as well as those with little or no contact with Santa Barbara or Palo Alto – also contributed to the rapid expansion of STM and, later, AFM. Among academic chemists and physicists, the Quate and Hansma way of doing air STM and AFM became a template for experimental activity. For many academic non-surface scientists, Quate and Hansma had demonstrated that *air* STM was cheap and easy to build (relative to its UHV cousin). An experienced graduate student could build an air STM in less than a day with a couple thousand dollars worth of materials [MK1, 10/12/01]. Even neophytes could construct an instrument from scratch and put it to work in an amount of time approximating the length of a graduate student career or a postdoctoral visit. Also, Quate and Hansma had shown that STM was flexible enough, and had a wide enough array of applications, that research groups, and even individual students, could easily carve out niches – what this researcher calls "low-lying fruit":

> It was pretty clear that one actually didn't know where the best place to use the technology was. That is, you just didn't know what application, where you were going to learn something new and interesting. I mean some people talk about picking the low-lying fruit, it's a little bit like that. There's all these problems out there, but some of them are just going to give more easily to the technology than others. So in the beginning, when I was first here, I spent a lot of time trying a lot of little different things, and that was mostly because I said "look, I'm not problem-driven, what I'm interested in is I think there are a lot of interesting scientific problems that I could work on, I'd like to find something where I can make a contribution...." So I spent a lot of time sort of dabbling in lots of different things. [JH1, 6/10/02]

This notion of low-hanging fruit was central to the Quate and Hansma group's methodology and the way these groups weathered controversy. Although group leaders might have larger goals in mind, at any point experiments jumped toward whatever easily-obtainable, interesting short-term goal was opened up by the previous experiment, rather than building in a positive way toward a single objective.

Two technological innovations – the tube scanner and improved vibration isolation (both explained in Chapter Two) – helped catalyze the proliferation of STM at the end of the '80s. As important as any specific design change, though, was the growth of an STM *community*. As STM conferences became a regular occurrence, and networks for exchanging students, samples, designs, and preprints sprang up, design solutions such as the pocket STM or the tube scanner began spreading more quickly. In the late ’80s, most growth in probe microscopy came from academic labs willing to borrow established designs and quickly get into the business of imaging new samples. Thus, probe microscopy became more standardized and routine; and the community reached a turning point where practitioners published more about the images they were generating than about the new microscopes they were building.

With that turning point, applying the microscopes to new materials drove the rapid expansion of the probe microscopy community as much as new design innovations. Of course, new applications were continually dovetailed with new designs, and with new understandings of what an STM could do and how it worked. In turn, each of these new understandings and new applications opened the door for experimentalists to bring the STM into new communities. The demonstration that STM could be done in air, water, and other fluids, for instance, was perceived as opening the door to electrochemists, biophycists, and others with samples that needed to be kept clean but not exposed to UHV. The rest of this chapter is concerned with one such application/innovation: the use of air (or water) STM to look at biomolecules (particularly DNA) adsorbed onto highly-oriented pyrolitic graphite (HOPG). Graphite and DNA fueled the rapid expansion of STM in the late ’80s; but, by the early ’90s, this complex of practices became the center of a major controversy.

As we’ve seen, air and water STM were central to the Quate and Hansma groups’ strategy of moving probe microscopy away from surface science and into a variety of other disciplines; thus, non-vacuum tunneling microscopes were looked on with eagerness by many academic groups, and with suspicion by the corporate STMers. Likewise, graphite was a problematic but useful material from the beginning. Hansma had imaged it early on, but had seen atomic corrugations hundreds of times larger than made sense – large enough that, even to someone with no specialization in the study of graphite or surfaces, the images seemed absurd and unpublishable.[5] Binnig had imaged it a little later and seen much the same thing; but, insouciant as always, he quickly published these absurd images. We saw in Chapter Three that the large atomic corrugation problem was alleviated by John Pethica's analysis of forces between the tip and sample (which eventually inspired the AFM) (Coombs and Pethica 1986; Pethica 1986; Pethica and Oliver 1987). Even before Pethica gave some rationale for these anomalies, though, newcomers to STM eagerly latched onto graphite as their sample of choice. For Binnig, Hansma, and these newcomers, graphite promised to do for air STM what the 7x7 had done for its UHV cousin. For example, it could play the 7x7's important standardization role. Any group building a new air STM would test their instrument on graphite first – atomic resolution of highly-oriented pyrolitic graphite (HOPG) meant (like atomic resolution of the 7x7) that the microscope had achieved the minimum standard of operation. Even the earliest *AFMs* took graphite as their first samples for this reason.

By the same token, even a few surface science STMers (particularly in California) used graphite to test their instruments in air before putting them into UHV. As this surface scientist at Lawrence Berkeley National Laboratory puts it,

> Graphite I did [image] but more as a test. Not for doing science with it, but for test. As a test material it's a good material. Well at least we thought it was a good material. It has the following advantages: graphite, in air, you can just cleave it, it's made of sheets laid on top of each other, you can remove one and the new surface ... is exposed as a fresh new clean surface ... [and] chemically they are so inert that nothing reacts with graphite. So even though it's in air, the air molecules and junk molecules that float in the air, they may land but they don't stay on the surface.... So as a test material that allows you to do quick tests in air.... Metals ... don't stay clean, so it has to be done in vacuum.... Every time you do that you have to put it in vacuum, pump all the

[5] In a close-packed surface like graphite, the atomic corrugation is the distance between the topmost layer of atoms and the next layer down. Think of a graphite surface as resembling a piece of corrugated tin, with the "corrugation" being the distance between the bumps (top layer of atoms) and troughs (second layer). A good example of atomic corrugation can be seen in Figure 3-3.

> air out, and wait until the vacuum is good which usually takes a couple of days.... So if you could have the microscope operate in air ... it's very convenient. So for that reason, graphite became *immensely* popular. Most people that did STM, the first thing they would try was graphite in air. If they see the graphite they know the instrument is sort of working. Then you can ... do it in vacuum with metals. [MS1, 3/9/01]

So graphite could easily serve as the measure of a working instrument; but it was also used to standardize the skills of new microscopists.

> AG: Whenever anybody's learning a new technique and it doesn't matter what it is, you always have them work on something that's known, say "can you produce what is known here? " Then we can go and move away from that.... I have a new crew that just came in and it's about a month of, you know, we're going to do the standard things here to get up to speed on this....
> CM: Where do the "standard things" come from? Is it a matter of sitting people down with particular specimens?
> AG: Yes, right, exactly. You will run graphite in air because everyone should be able to do that. Then we'll do gold underwater, then we'll put down this metal monolayer business and once you get to that level then you are viewed as being certified. You have to be able to do that. The other thing is that with any technique, the most important thing is to know when the machine is broken. Machines break and that's just the way it is. So we have them do some of these simpler things on a regular basis just to show that the machine is working. For example, graphite in air now is something you should be able to do every day, right – so I say, once a week go make sure you can do it, not because I think you're losing your skills but just to prove that the machine is actually working. [AG1, 6/25/01]

As these quotes show, graphite had a number of advantages that made it such a favorite material. It was cheap and could be easily ordered from laboratory supply catalogs [AG1, 6/25/01]. It was extraordinarily easy to prepare – "cleaning" the sample simply required pulling the top layers off with a piece of scotch tape. Anyone with the skills to build an STM (whatever their disciplinary background) could get their hands on it, put it into their microscope, and stand a good chance of seeing atoms – thus making it attractive to a much wider range of researchers than the finicky 7x7. Indeed, the ease of obtaining atomic resolution was due – at least in part – to the same anomalously large atomic corrugations that had vexed Hansma – a very large corrugation provided better signal-to-noise and hence clearer pictures, even if the corrugation seen in an STM was more a product of the tip pushing on the graphite than of the "real" corrugation of HOPG. That is, the "reality" of the corrugations, and the scientific feasibility of the material, were much less important than the advantage graphite offered in standardizing the instrument and its operator.

Much of graphite's popularity stemmed from its use as a substrate on which to deposit and examine interesting molecules. Electrochemists and surface scientists liked the STM because it provided views of large, clean, flat, crystalline expanses of their favorite materials. As the STM spread to other subdisciplines, though, those fields wanted to use it to look not at surfaces, but at objects deposited on surfaces. Binnig, Quate, Hansma, Baldeschwieler, and their new allies saw the STM as a general-utility instrument (like an optical or electron microscope), capable of looking at isolated entities on a surface and radically magnifying them. In particular, they wanted to look at the "epistemic [and/or technological] things" particular to the subdisciplines of the builder groups' collaborators – things like biomolecules or magnetic bits in a disc drive (Rheinberger 1997). Doing so with an STM or AFM, however, required putting these entities down onto some kind of surface – a substrate. Ideally, this substrate would be inert, clean, easy to prepare, and highly conducive to atomic resolution. Atomic resolution was useful because (A) it gave a quick indicator that the microscope was working properly; and (B) since distances between atoms of a substrate are known, they could be used as a ruler to measure an adsorbed molecule – much as, at lower resolution, electron microscopists use the pitch of the copper grids on which they mount samples to locate and measure features within those samples.

Graphite seemed to exhibit all these properties. Suddenly, it became easy to put all kinds of molecules down onto graphite and image them, and many kinds of experimentalists – physicists, chemists, biophysicists, biologists, etc. – took advantage. The final reason graphite and air STM boomed in the late '80s was that commercial instruments became available in this period. Indeed, as we will see in

Chapter Seven, the first commercial instruments were designed with air STM of molecules on graphite as the major (if not the only) envisioned application. So now, even those without the skill, time, or interest to build their own STM could enter the fray. The result was a gold rush for air STM on graphite. Examining the proceedings of the IBM Oberlech conference in '85, the 1986 STM conference in Santiago de Compostela, and the '87 STM conference at Oxnard, we see a dramatic change in the demographics of STM that accompanied highly-oriented pyrolitic graphite.[6] At Oberlech, speakers were predominantly from IBM, plus a smattering from Bell, Ford, Stanford, UCSB, Caltech, and a few European universities; virtually all of the atomic resolution-capable instruments reported on were being operated in vacuum, though graphite and air were beginning to creep in at the edges. Santiago de Compostela was somewhat more inclusive – many more academic groups, and the first groups from Japan – but, again, graphite and air were still marginal. By Oxnard, almost 15% of the papers had graphite in their *title*, and several more concerned research or instrument design done using graphite. Of eleven sessions at Oxnard, two were on "layered compounds" (i.e. graphite plus some layered superconducting materials) and one was on "biological applications" (usually involving graphite); in addition, talks about graphite surfaced in sessions on "atomic force microscopy," "theory of STM, STS & AFM," and "STM & AFM of liquid/solid interfaces." No wonder, then, that many of the participants, especially those coming from surface science, felt overwhelmed by the tide of graphite. For those who had been doing STM for a few years, such as this IBM Zurich surface physicist, the new substrate and the new microscopists looked suspect:

> Suddenly the world went graphite because everybody could image suddenly graphite but not other things. They could image graphite in air. Paul Hansma

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[6] The proceedings of these conferences are in: *IBM Journal of Research and Development*, 30.4-5; *Surface Science*, 181.1-2; and *Journal of Vacuum Science and Technology A*, 6.2.

> in Santa Barbara imaged graphite with a chicken fat coating and someone imaged graphite with pencil lead. So it really got a bit boring with the graphite. So I moved on to single crystals, surface physics type things.... [At] Oxnard .... it was talk after talk after talk after talk after talk and it was graphite, graphite, graphite, graphite – everybody had a different graphite story and they all invented some new story.... Stuart Lindsay from Arizona, I had my eyes closed ... and Stuart was describing the results, and I was listening about the DNA "and here we can see this part and here we can see this part" and it was so marvelous, I couldn't believe it, I opened my eyes and it [the image] was just awful, it was just unbelievable. [JG1, 10/22/01]

For surface scientists, air STM images, particularly those done on graphite, were a lightning rod – air was so open to contamination and graphite so ill-defined that they had trouble understanding or believing the new STMers' results. Indeed, they had trouble *seeing* (quite literally) what the new STMers saw. Bob Hamers, a former Yorktown postdoc, remembers having trouble interpreting the air work on graphite:

> There was one paper from Ciraci and Batra [two IBM theorists], where they were predicting on graphite that you should have a reversal of contrast.... So, I thought "well, okay, I'm going to see if I can see that." That's the one time I started to work on graphite. At that time Binnig and Rohrer had reported seeing corrugations about 2 angstroms high on graphite. I thought "wow, that should be easy to see." So I tried it in ultrahigh vacuum – couldn't see anything. You could see corrugations maybe a tenth of an angstrom, okay, and I'm thinking "am I doing something wrong here or what?" Actually my manager, Joe Demuth, was getting kind of like "why can't you get this, everybody else is doing these in air and seeing height changes that are 2 angstroms high...." Later people were reporting corrugations of 20 angstroms high and 200 angstroms high between atoms that were only 1.97 angstroms separated, because [in air STM] ... the tip was actually in contact with the surface, and so I kind of got disgusted at that point and figured "you know, this graphite doesn't look like a good place to spend my time." It was kind of misleading.... The first commercial vendors of STMs were doing measurements in air, and they would have this little STM sitting on the table and they would show that you could actually knock on the table and it wouldn't see any vibrations. Okay, but this thing was totally bogus.... That turned a lot of people off to STM at the beginning was the commercial vendors were showing these wonderful images of graphite with atomic periodicity, but not true atomic resolution in air, and making it sound like it was very easy and could be done anywhere, but in fact when you got to any other sample, or anything that you'd really want to study, it was no longer true. [BH1, 5/9/01]

Such qualms about air and graphite were well known, and discussed widely in the literature and at the STM conferences. Corporate surface scientists saw these ambiguities as reasons to disregard any work done in air; for most newcomers, though, these were simply teething problems to be expected from any new technology. For them, the potential benefits of air and graphite were too tempting not to employ them.

The questions about graphite continued to mount, however. As a result, many look back on the late ’80s and early ’90s as a period I have called “experimental vertigo” (Mody forthcoming-a). Air/water STM, with graphite as a substrate, was too exciting and promising to ignore; but the sudden influx of new researchers working with a highly problematic material sparked friction and skepticism.[7] The strains of accommodating large numbers of newcomers into what had been a small community with a relatively common focus were rapidly becoming apparent. Two Zurich STMers – one (Binnig) an old-timer, the other (Bruno Michel) one of the bio-newcomers – remember biology’s problematic integration into the community:

> Heini Rohrer was my boss.... He was a very good physicist, but in biochemistry he said, “do what you want.” He was always very supportive of my work since he wanted his invention to contribute something useful in the biological field. In the initial phase there was a very strong physics group here [at IBM Zurich], and they said “we’re doing the purest and finest research, and there is one guy doing the dirty stuff” <laughs>.... The hardest thing was presenting the results to a wider audience.... We went to a conference.... [My group leader] gave the talk, and that was the first time ever that direct biological imaging by STM was shown, and people laughed at him <laughs>. [They said] “that’s crap.” [BM1, 11/12/01]

> CM: So at the point where there were multiple groups doing STM, did there start to be some standard for judging the competence in using it? I mean, for instance, achieving atomic resolution on different kinds of materials.
> GB: Yes, that was in the beginning easy because we met quite often and we could discuss things and then the field was kind of clean in this respect. So there was not much nonsense published. That happened later, quite a bit. In particular when many people started with biology and many many people – the community grew so fast and then people started measuring with the STM and seeing something and interpreting it immediately in a direction which was completely wrong. They were not very critical and they didn’t think about all the kinds of artifacts you can create. Like this small group in the beginning – we always would point “ah, this might be an artifact, look into that.” By this exchange of information everything was under control, I would say. But later it was a little different. That was a bad phase for STM. STM got for a while a very bad reputation, particularly in biology. [GB1, 9/26/00]

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[7] A classic example of interdisciplinary “bandwagon science” (Bromberg 1982; Fujimura 1988).

The DNA Controversy

Eventually, the doubts about air STM and graphite blossomed into open controversy. The locus of this dispute – and the point on which the fate of air STM hinged – was the ability of the STM to image DNA at very high (possibly atomic) resolution. The rise and fall of air STM resembles many scientific and technological controversies; but it also has some wrinkles that extend our understanding of how disagreements erupt and closure is achieved in scientific communities. This controversy is particularly interesting because, in the end, closure in a technological debate (which is better for most users, STM or AFM?) provided the means for exiting the regress of a scientific debate (can STM image DNA?).

The roots of the DNA controversy lie in the continual quest to find an equivalent of the 7x7 for air/water STM. In *some* ways, graphite fulfilled this role. It was a well-known material and a suitable standard for both instruments and instrumentalists; and in other ways – availability, ease of preparation, suitability as a substrate – it even outperformed the 7x7. Graphite alone, though, lacked the sexiness of the 7x7 that had won the STM many converts, plugged it into ongoing debates, and eventually secured it the Nobel Prize. Importantly, no discipline showed much interest in learning anything new about graphite surfaces, and no field was engaged in any debates about its nature. For surface scientists, it was a more or less understood but disciplinarily marginal material with few technological applications and little new science to offer; for molecular biologists, it was a tool, a ready-to-hand substrate, but not a scientifically interesting object *per se*. STMers had questions about graphite, but they were never able to insert these questions into any other community's discourse; thus, they were left to answer their questions on their own. Moreover, at a visual level, even STMers found their images of graphite to be much more banal than those of silicon. For reasons that would become important later, the STM usually showed monotonous row after row of close-packed carbon atoms with no point defects – an image that offered little visual appeal and almost no new knowledge – though it did make it easier to isolate and observe adsorbed molecules [JM2, 7/6/00].

Thus, STMers were eager to find something to put on graphite that would attract interest and new users. Finding such a material was difficult, though – it had to be more or less conducting, and flat enough to avoid various tip-induced artifacts.[8] One solution was to use the STM to look at *monolayers* of complex molecules – i.e., very thin, flat layers of tessellated molecules. Two kinds of layered arrays of molecules became particularly popular – liquid crystals and Langmuir-Blodgett films (often called self-assembled monolayers) (Foster and Frommer 1988; Nejoh 1990; Lang, et al. 1988; Fuchs 1988; Hansma, et al. 1991). In both cases, there was an active community to whom STMers could communicate their findings. Also, these arrays enjoyed early successes because they "proved" that the STM could, in fact, image molecules. STMers had long seen "dirt" and "gunk" in their images that they assumed were molecules, and some had also offered images of individual molecules that they put down deliberately.[9] Securing the correspondence between those images and the molecules in question was problematic, however; critics could always claim that STMers merely scanned along until they found "gunk" that resembled the molecule they were looking for and assumed (perhaps mistakenly) a correspondence. Ordered, regular arrays of molecules, as found in liquid crystals, seemed to overcome this problem. The regularity and complexity of the micrographs, and the sheer number

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[8] The most notorious of these artifacts is called a "tip-sample convolution." Though not a strict convolution in the mathematical sense, a tip-sample convolution yields an image in which features of the tip and the sample are mixed together ("convolved"). This happens because the tip has a certain width; if it scans across surface feature that is sharper than that width, the tip will slide across the surface feature in such a way that the point closest to the surface is *not* the apex of the tip. Since the STM always produces an image in which it is assumed that tunneling is occurring through the apex, the image will always be somewhat distorted for non-flat surfaces.

[9] Similar to the transformation of photographic "scruff" into an epistemically interesting "pulsar" in Woolgar (1976).

of molecules being imaged, made it more difficult to claim that the images were flukes or wishful thinking.

The real holy grail for air/water STMers, though, was imaging DNA. Even before graphite's advent as an STM substrate, researchers in Zurich and elsewhere were putting DNA down onto silicon, paraffin, and other surfaces (Amrein, et al. 1987; Gross, et al. 1988; Amrein, et al. 1988). With the advent of graphite, however, the combination of nucleic acids (both DNA and RNA, in a variety of guises – single-stranded, double-stranded, four-stranded, single-base sequences, plasmids, etc.) – with the highly-oriented pyrolitic graphite substrate became immensely popular. DNA was of interest to an extraordinarily wide range of researchers – chemists, biophysicists, and molecular (and evolutionary) biologists, even materials scientists, mathematicians, computer scientists, and the new breed of nanotechnologists. The behavior and macrostructure of DNA at the molecular level was long discussed and debated; so if the STM could, indeed, visualize that behavior and structure reliably at atomic resolution, then it might find instant and widespread application.

Two final features of DNA were particularly attractive to instrument-building academic groups like Binnig's, Quate's, Hansma's, John Baldeschwieler's at Caltech, or Stuart Lindsay's at Arizona State. First, just as Binnig and Rohrer had known virtually nothing about the 7x7 when they started imaging it, so DNA seemed (to the builder groups, anyway) ready to yield knowledge even to neophytes:

> I didn't really know much about DNA. But it was a great biomaterial to start with, because in the AFM field it was too early for people with serious DNA research questions to play with this microscope that was still in the prototype stage, so I didn't need to know about the latest DNA research. Oh, it was also wonderful for me that I could have a decade out of the lab and come back in with no particular handicap because nobody knew anything about how to do AFM of biological materials, so there wasn't any body of knowledge. [HH1, 3/19/01]

Thus, builders thought they could concentrate on putting together new instruments and pushing the envelope of the technology, while still producing interesting data. Secondly, DNA – like the 7x7 – seemed to be a highly generative material; that is, there were enough small variations on the DNA molecule, that once you started down that path you could generate a very large number of slightly different yet still interesting experiments. This meant a large number of experiments done quickly, without having to learn a new knowledge base each time – something that fit well with the style of the builder groups.

One possible application of STM of DNA – much maligned today, though widely entertained at the time – was to use a tunneling microscope to *sequence* strands of genetic material. In the late '80s, the Human Genome Project was beginning to gather funds and organize efforts to sequence the human genome. At the time, several techniques were vying to be the project's primary sequencing tool, and there were good reasons to think that STM could compete. After all, STM was an atomic resolution microscope. If it could atomically resolve a strand of DNA, then it might be able to discriminate one base pair from another; if it could do that regularly and reliably, then it promised to sequence the entire genome much more cheaply and quickly than any other technique. Hansma, Baldeschwieler, and Lindsay, in particular, vied for funding from the Human Genome Project and the National Institutes of Health to build microscopes that could sequence DNA [SL1, 1/6/03]. Indeed, Hansma's long line of biophysics postdocs can be seen, in part, as a largely unsuccessful attempt to tap into NIH funding for this and other projects. Baldeschwieler, who had received NIH funding for his nuclear magnetic resonance work in the '70s perhaps knew better how to talk about medical applications, and received an NIH grant titled "Electron tunneling microscopy for biological systems" in 1986.

The first thing builders tried to demonstrate to the molecular biology and biophysics communities in general, and the Human Genome Project and NIH in particular, was that the STM could atomically resolve a strand of DNA. The notion of "atomic resolution," though, was beginning to fray. For one thing, a microscope that could see atoms on flat surfaces of silicon or graphite might see nothing on surfaces with more varied topography. A flat graphite substrate might yield individual atoms, but the single strand of DNA lying on top of it would look blurry and indistinct. Moreover, as both scanning tunneling spectroscopy and STM theory matured, most STMers became less realist about atomic resolution [JN1, 6/28/01]; from one tunneling voltage to another, the positions of atoms could vary quite significantly, a problem that became more pronounced as people started looking at more complex biomolecules.

Finally, the banal, "clean" quality of most images of graphite became a concern. As I've written about elsewhere, defects, impurities, contaminants, and imperfections are an important part of achieving a credible reality effect for microscope images in materials science and surface science (Mody 2001). This is especially true in probe microscopy, where, by the late '80s, participants began to realize that problems in the hardware, software, or electronics can all cause the instrument to produce spurious images that look identical to a defect-free, close-packed crystalline surface. So the total absence of defects in images of graphite began to diminish the credibility of the STM. By 1987, a Quate student, Howard Mizes, had begun to examine the theoretical underpinnings of the tip-sample interaction in air STM of graphite, and showed that "atomic" resolution was something of a misnomer (see Figure 6-1).

> It took several years before people began to realize that the image they were getting off graphite, with apparent atomic resolution, was in most cases flawed and probably reflected a transfer of one flake of graphite to the tip. So what

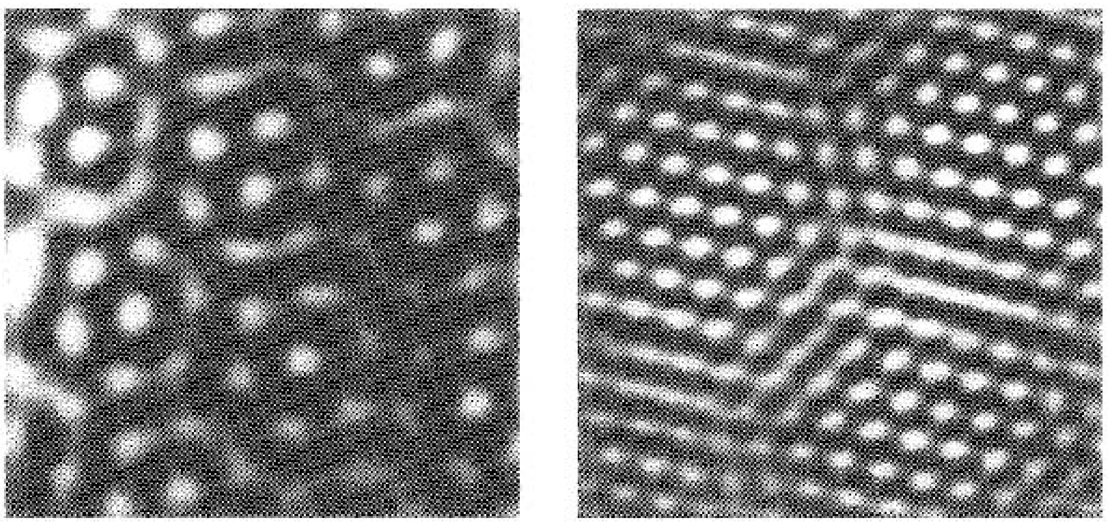

Figure 6-1: Moiré patterns on graphite. A moiré pattern results when two regular patterns (e.g. two grids) overlap, creating a third pattern where the two original patterns cancel or reinforce; imagine looking through two window screens. In STM of graphite, if the tip has accumulated a flake of graphite, then the result is an image that superimposes *two* graphite surfaces (a moiré pattern). Consequently the white spots in the images below are *not* (unlike most STM images) thought to correspond to individual atoms; rather, they represent ensembles averages of several atoms. From Albrecht, et al. (1988).

> you were scanning over the surface was graphite over graphite, and you were looking at Moiré patterns. The reason that people ultimately began to realize that fact is that they looked at the images of the graphite and realized they were never flawed – you always had perfect order. Now, wait a minute – graphite does have flaws in it on occasion. Why don't we see this? Well if you have a Moiré pattern then in fact you have an ensemble average, that's why the images were near perfect. Prior to that realization everybody calibrated his or her ability for atomic imaging with graphite. You joined the world [of STM] - by showing that you could image graphite. Well the truth of it was that, if you couldn't image graphite with atomic resolution, you certainly weren't going to image anything else with atomic resolution, but imaging the graphite was no proof that you had actually imaged individual atoms. [JM2, 7/6/00]

These Moiré patterns (seen with STM *and* AFM, and on other layered materials besides HOPG) did little to tarnish the material's advantages as a standard for instruments and instrumentalists. They did make it problematic, though, to talk about "atomic resolution."[10]

> All surfaces suffer from contamination from the air. For this reason, atomic-scale imaging in air with the AFM has been mostly limited to layered compounds (*e.g.* graphite, boron nitride, mica, *etc.*). All of these have regular hexagonal or square lattices with only a few surface defects. None have a rich structure such as that of the Si[111] 7x7 surface. Lattice images of such simple surfaces do not irrefutably prove that the instrument is achieving atomic resolution.... Strangely, very few variations in the perfect lattice have ever been recorded with the AFM. Could it be possible that the AFM is somehow averaging the lattice structure over a fairly broad area so that point defects do not strongly appear, yet somehow the average lattice structure is being maintained? ... In most of the AFM literature, the term "atomic resolution" means that images are taken which have variations on the length scale of atomic spacings. There is some difficulty, both experimentally and theoretically, in interpreting these variations as true atomic resolution features. (Barrett 1991, 26-7)

Mizes' findings called into question any new, publishable knowledge extracted from such images. It was with this understanding of graphite and atomic resolution in people's minds that the first "atomically resolved" STM images of DNA appeared.

By 1988, these images were coming from a number of places. At IBM Zurich, Giorgio Travaglini, working with Rohrer, modified a standard Zurich STM for larger

[10] As discussed in Chapter Four, there is a vast sociological and historical literature on the visual aspects of science and technology. See Daston and Galison (1992); Lynch (1988); Lynch and Edgerton (1988); Latour (1988a) for a first cut at this topic. The STM case is, perhaps, interesting because STMers were not trying to simplify or abstract images in order to lend them intelligibility; rather, they were hoping to make them more complex and disordered in order to lend them credibility.

scan ranges and lower tunneling currents, until his group was finally able to images helices of recA-DNA complexes [BM1, 11/12/01].[11] At first, they tried a standard trick from electron microscopy, coating the DNA with metal to make it conductive; later, they put uncoated DNA onto paraffin, with similar results. At Arizona State, Stuart Lindsay, a former Hansma collaborator, followed suit (Barris, et al. 1988; Lindsay, et al. 1989). Next came Carlos Bustamante, another biophysicist and Hansma collaborator, at the University of New Mexico (later Oregon and then Berkeley) (Dunlap and Bustamante 1989; Garcia, et al. 1989). Where Lindsay's collaboration with the UCSB group centered more on instrument design, the later Bustamante collaboration focused on sample preparation techniques. One of the problems of using STM or AFM to look at large molecules is that, unless they are anchored properly, the molecules will simply be swept aside by the probe. The Bustamante/Hansma collaboration was among the first to develop a reliable means of anchoring DNA so that it would remain still under the gaze of the microscope [HH1, 3/19/01; CB1, 10/17/01].

Soon, the DNA field became quite crowded – Gil Lee at Purdue, Bruce Warmack and Dave Allison at Oak Ridge National Laboratory, John Baldeschwieler at Caltech, Binnig (now at Munich), Wigbert Siekhaus at Lawrence Livermore, and others all entered the fray, especially as the first commercial instruments became available (Arscott, et al. 1989; Allison, et al. 1990; Youngquist, et al. 1991; Heckl, et al. 1991; Allen, et al. 1991). Almost all of these groups concluded that atomic resolution of DNA was tantalizingly close. One signpost pointing to atomic resolution was the presence of helical strands on graphite that displayed the right pitch (number of turns per unit length) as DNA. The strands themselves were blurry, but they looked

[11] RecA is a protein which can be made to coat a single strand of DNA, forming a recA-DNA complex.

roughly like DNA, and it was hoped that, with modifications and practice, the STM might resolve the atoms making up the helix.

The high point of this line of investigation came in 1990, when Baldeschwieler's group published an atomically resolved image of DNA (see Figure 6-2) on the cover of *Nature* (Driscoll, et al. 1990). Almost immediately, though, questions were raised about this and similar images [WH1, 11/14/01; DB3, 4/3/01]. Notably, many of these questions arose from the interdisciplinarity of the field. Probe microscopists were running into intractable problems grafting STM and AFM onto new epistemic cultures (Knorr-Cetina 1999). In making what seemed like an obvious bridge to electron microscopy, for instance, STMers found themselves under suspicion. Joe Zasadzinski, an electron microscopist at UCSB who worked with Hansma, remembers this time:

> People were just starting to come out with some of the wilder pictures of DNA with STMs and things. Both Paul [Hansma] and I didn't really believe that what they were seeing was what they were claiming to be seeing.... The importance to having a microscopy background is that most microscopists are the last ones to believe any of their pictures. You're sort of raised in that culture. You're supposed to think of all the things that you could've done wrong. See, when it looks like what you expected it to look like, there's such a strong gut level "go out and publish" sort of feeling that you really have to check yourself if most of your work is in visualization.... You could probably get it past the reviewers because, lord knows, a lot of people did <laughs>. That was sort of the era of STM at that time, is people were just looking at everything. It was a new microscope, it was a new way to do things, it promised such high resolution.... As a microscopist I knew some of the drawbacks of drying things out of solution and all of the artifacts you can get in *electron* microscopy. I mean, I wouldn't have prepared the samples for an *electron* microscope typically by a lot of the procedures that [STMers] were using. [JZ1, 3/20/01]

Similarly, STMers found it hard to make inroads into biology. In the days of home-built instruments, and even into the commercial era, few biologists worked with STM/AFM (leading to a lack of biological sophistication in the DNA controversy). One biologist who was able to bring biology into an instrument-building group remembers how rare and difficult that process was at this time:

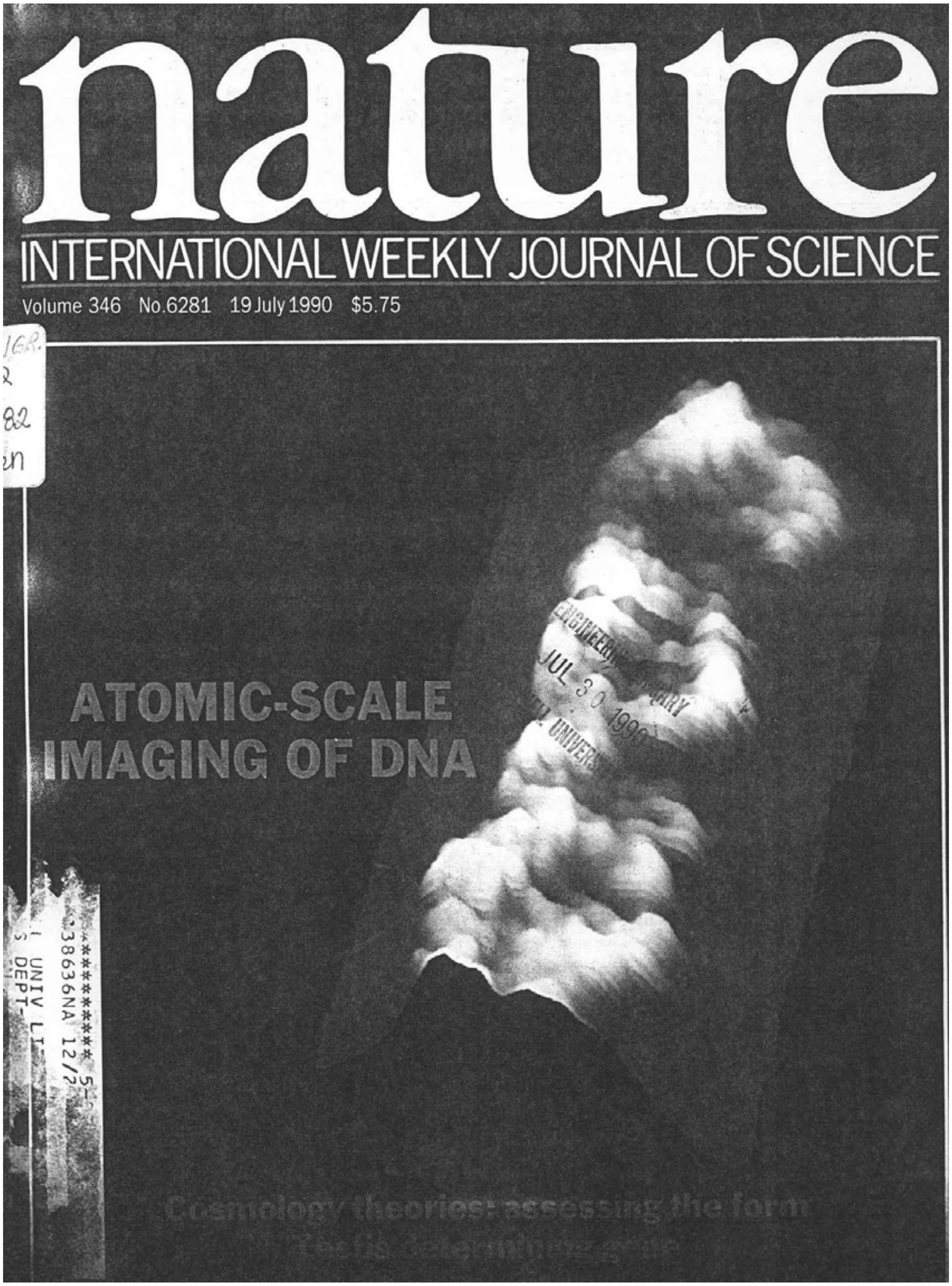

Figure 6-2: Baldeschwieler's cover of Nature. A well-known and controversial image from the Baldeschwieler group purporting to give atomic resolution of a segment of DNA. You can see the turns of the helix and, it is claimed, the base pairs connecting the two strands. From Driscoll, et al. (1990).

> Mainly, everybody in the field was a trained physicist that knew how to do a little biology. That is, they bumped into a biologist in the hall and he gave them a little vial and the biologist said “there’s some DNA in there” <laughs> and the physicist went back to the lab and tried to image it. I mean there were a few biologists in the field but not very many. [MA1, 10/12/01]

By 1990, some of the established STM/AFM builder groups were beginning to realize the dangers of neglecting the epistemic needs of the disciplines for whom they were demonstrating the credibility of the new instruments. It is notable, for instance, that the Hansma group, though heavily invested in DNA, kept its head down during the controversies of 1990-2. It is even more notable that Binnig, once such a maverick, now began to sound a note of caution. Lindsay, too, put the brakes on his enthusiasm and published a skeptical article entitled “Can the scanning tunneling microscope sequence DNA?” (Lindsay and Philipp 1991). For them, and for other skeptics, the infamous Baldeschwieler image became the prime point of contention. The Caltech group’s *Nature* cover had been prestige science, but it was also – almost instantly – controversial science.

Baldeschwieler’s detractors focused primarily on the Caltech group’s apparent lack of a methodology for dealing with contamination and artifacts. Such fuzzy methodology had been standard (indeed, had been tremendously beneficial) early on in STM and AFM; but now, as probe microscopists tried to make the technique credible to disciplines with more method-heavy practices (where the microscope was expected to integrate into an ongoing complex of theories, concepts, materials, recipes, and other instruments), the SPM community’s leaders focused on establishing standards for dealing with contaminants and artifacts. Recall that the first problem with graphite had been anomalously large corrugations due, in part, to a contaminant and water layer at the surface of the highly-oriented pyrolitic graphite, and thus visible only to air STMers and not their corporate, UHV counterparts. A second problem had been the lack of point defects, ascribed to the tendency of the tip to pick up a graphite flake (or other "gunk") and scrape it across the surface, creating a Moiré effect. Finally, in 1990-1, a new problem emerged. STMers began to "see" DNA even where they were not intentionally putting it onto graphite. Perfect helices started to appear without *any* sample preparation whatsoever – i.e., without even a sample to be prepared.[12] The disturbing possibility was that the "DNA" everyone had been so excited about was not genetic material, but some contaminant masquerading as such.

> That certainly looks like a twisted helix of DNA. But according to these particular researchers, they put no DNA down unless they sneezed on their samples. I mean that became a running joke of, you know, who sneezed on their samples. [JF2, 3/14/01]

In 1990-1, Lindsay, Binnig, and a former Siekhaus postdoc named Tom Beebe all brought to the community's attention HOPG's tendency to form long, meandering, strand-like defects that could mimic DNA, right down to the pitch of its helix. In a series of key articles, Lindsay, Binnig, and Beebe showed images of "DNA" that were indistinguishable from images of "clean" graphite (Clemmer and Beebe 1991; Heckl and Binnig 1992; Lindsay, et al. 1990). Likewise, the Travaglini group at IBM Zurich – which was embedding DNA in paraffin on top of graphite – found much the same thing. With these exchanges of evidence and counterevidence, the experimental vertigo that had afflicted air STM of graphite became thoroughly disorienting. Many STMers had known about various problems with graphite and DNA, had discussed them at STM conferences, and had begun devising workarounds. To have a star result like Baldeschwieler's called into question so publicly, though, meant that a more radical solution seemed necessary, one that spoke to the concerns of the disciplines whose expertise intersected that of the STM community. Many STMers (especially those who are now AFMers) look back on this as a bleak time of experimental and disciplinary misrule and distrust:

[12] This is somewhat reminiscent of the controversy surrounding Blondlot's "N-rays" (Collins 1992, 45; Ashmore 1993).

> There was a lot of pseudoscience that went on in biology in those days.... If you look around on graphite, eventually you can find anything you want. Reconstructions in graphite can resemble different crystalline materials, they can resemble long-chain molecules, literally if you look around long enough you'll see what you want to see. We had a term for that – "face of Jesus." If you looked around long enough you'd see the face of Jesus. But it took a long time for people to figure that out because of course they put these things down, they'd look around for a few weeks and they'd see what they wanted to see and they'd publish a picture. Everybody else'd say "wow, that's really neat" and they'd put it down and they'd look around for a few weeks and they'd see what they wanted to see and they'd say "yeah, it works." [MT1, 2/26/01]

"Closure" versus "Moving on"

STM of DNA was more controversial than any other research area in probe microscopy, then, because it was the focus of ever-greater difficulties handling interdisciplinarity in a rapidly-expanding instrumental community. DNA represented an attempt by the air STMers to make their work relevant to far-flung and well-established fields. Some of these disciplines, especially molecular biology, approached the instrument and the instrumentalists with skepticism. At the same time, more enthusiastic converts – especially physicists and biophysicists (some with the "bio" only recently attached) flooded the STM community. Like the well-established "builder" groups these newcomers generated data quickly, using sample preparation and image interpretation methods to which they often were not accustomed. Unlike the builder groups, though, they had virtually no pedigree in STM instrumentation to blunt the edge of any incautious results they published – i.e., they could not recast dubious scientific results as instances of technological innovation, and they did not have the builders' network of postdocs and collaborators to support them. Indeed, as the first commercial instruments became available, many of these people were buying, not building their microscopes. This set up a dynamic where biologists were inclined to be skeptical, and the new STMers gave them results on which to exercise that skepticism. Anxiety about newcomers and interdisciplinarity is evident even today, as bio-SPMers remember the DNA controversy:

> There were reports later on of imaging DNA that were possibly more valid, but still, in my opinion, it was physicists doing biology. Physicists are *smart*, I mean, that's not slamming physicists by any stretch. But their experimental design was quite different than a typical molecular biologist bench scientist design. [EH1, 6/22/01]

> Developing an instrument was done in physics groups. But you need a biological group. You get something from the biologists, put it on your substrate, and you will see it. You have to prepare it and for the preparation you have to find out the right concentration. If it's too much, it's overcrowded, you can't see anything anymore. If you have low coverage it takes too long to come to the next one. So you need always to discuss with the biologists.... Physics groups, this was always a problem for them. At least to get to a result you need very good communication between both biochemists and the physicists. [RG1, 11/14/01]

> Magnificent pictures have already been obtained with the STM showing atomic features on the surfaces of crystalline semiconductors or evaporated metal films. Nevertheless, not every STM picture is a revelation, and journal editors and referees will become more fastidious once the enthusiasm surrounding the new technique has settled. Biologists, with their desperate need for non-destructive high resolution microscopes, became immediately interested in the STM, but most attempts to obtain STM images of biological matter were met with frustration. Actually, most images of DNA, protein or supramolecular assemblies such as viruses look rather dull and uninformative and could stir up excitement only in those circles whose members are completely unaware of the state of the art in biomolecular electron microscopy. (Baumeister 1988)

For the more established builder groups, and for the new manufacturers with whom they were associated, DNA represented a catch-22. On the one hand, people like Binnig, Quate, and Hansma valued the openness of the community they'd constructed and strove to expand the SPM field; but, they were profoundly uneasy with controversy and possible threats to the credibility of the technique. Such threats were quite real, and the effects of the DNA controversy can still be felt today:

> When people started to leave out the metal coatings [on biomolecules] and still could get pictures, that was really hard to figure out. I don't think anybody really successfully figured out why the pictures showed through. People could also see the same things without any real materials on the substrates. Then nobody believed anything. I think what a lot of that stuff did actually was pretty much for a while remove probe microscopy from biology. There was a real sort of rush to belief in the beginning.... We went through bad times where the images were pretty much showed to probably be artifactual. That's held for quite a while. Because even in proposals now that I write to NIH – not this last one but the one about three years ago, they were still saying that

> "nobody believes AFM and this is not a biological tool and everybody knows it's artifacts." [JZ1, 3/20/01]

The effects of the DNA controversy were perceived as propagating far beyond the handful of researchers working on nucleic acids; therefore, pressure to come to closure quickly was much greater than for any previous SPM dispute.

One path to closure might've been to establish rigorous experimental procedures, to continue refining air/water STM until artifacts would disappear and the technique would seem credible to all. Indeed, the language of many probe microscopists in describing the controversy points in this direction: biologists, they say, were not being fair, every microscopy is prone to artifacts early on, people should look at the history of electron microscopy to see how long it took for artifacts to be dealt with there, etc. [JH1, 6/10/02; MA1, 10/12/01]. The means to vet bio-STM of its defects and transform it into an instrument in which biologists could believe were apparent to most probe microscopists in the early '90s; yet few people actually traveled that path. It's important, though, to highlight these die-hards, since their story emphasizes the contingent, yet inexorable, logic of closure.

As we've seen, one way to refine bio-STM would have been to look at biomolecular arrays, rather than individually adsorbed molecules that could be mimicked by defects. Work on Langmuir-Blodgett films was one step in this direction. Another, more closely connected to the DNA controversy, was to image monolayers of the DNA and RNA *bases* – guanine, cytosine, thymine, adenine, uracil. Mike Allen (working with Wigbert Siekhaus at Lawrence Livermore National Lab) and Wolfgang Heckl (working with Binnig at the IBM outpost in Munich) both made significant progress in this direction, particularly in figuring out proper imaging conditions and specimen preparation techniques (Allen, et al. 1992; Heckl, et al. 1991). Another tack would have been to borrow a trick from electron microscopy by coating biomolecules with metal to make it conducting. Electron microscopists trying to break into STM (such as Joe Zasadzinski, a Hansma collaborator at UCSB or Reinhard Guckenberger at the Max Planck Institute) tried this technique, with some success (Zasadzinski, et al. 1988; Guckenberger, et al. 1988). Both metal-coating and molecular arrays, though, risked dangerous comparisons with other techniques. STMers were never able to borrow just enough from crystallography or surface science or electron microscopy to make bio-STM look credible without also making it look uninformative relative to what was already known in those disciplines.

> There were some groups that went to metal coating for STM.... Matthias Amrein.... did some very good work on metal-coated rec-A complexes with the STM and showed that it's a lot of work and maybe it's not much better than electron microscopy. So people were saying that's good but maybe that's not unique enough, or not enough of an improvement. Most people gave up. [MA1, 10/12/01]

> The majority of biologists, though, would tend to say, especially in the early going, that you didn't learn anything new. Okay, and that was in fact usually an accurate criticism, that you didn't know anything that you didn't know before. I mean, so you can image DNA – what, you didn't know what DNA looked like? So you could see a DNA binding protein – you could do all of that by rotary shadow EM. So early on people got a lot of flak for "yeah, and, so what?" you know. And I think that people in the field really thought that well what's happening here is, okay, if we can get to where electron microscopy is now in just a couple years, then where are we going to be in 5 or 10 years? But the biologists didn't think about that. They looked at what they saw in front of them. They didn't see where are things going to be, which I always thought was just historically naïve. I mean why can't you look into the future and say "wow, you can do that now!" It's like, no, the response was "yeah, I could that with electron microscopy." [JH1, 6/10/02]

One answer to doubts about bio-STM of nucleic acids was an elaborate control experiment to discriminate between "real" and "artifactual" images. Stuart Lindsay followed this route using sequences of DNA known to have a particular shape.

> [STM] was just a mess for biology. But nonetheless, we produced quite a few papers. The "proving the point" paper about DNA on metal electrodes is one in which a guy at Columbia made DNA cis-platin complexes – cis-platin ... kinks the DNA, or is believed to. So he sent us DNA, with and without cis-platin, and he didn't tell us what was in what test tube.... It worked, you could see little kink things when you had the cis-platin, and you didn't when you didn't have the cis-platin. But ... my conclusion was that if you had to put a lot

> of work into proving that you *could* do it under special circumstances, it wasn't what you wanted as a routine assay in biology. [SL1, 1/6/03][13]

Finally, one could remedy bio-STM by tweaking specimen preparation and imaging parameters to produce pictures of even the most recalcitrant samples. Extremely low tunneling currents, for instance, allowed STM to operate even on thick insulating molecules like DNA; substrates other than graphite yielded fewer mimicking defects; and environmental conditions, such as humidity, could occasionally have a miraculous effect on bio-STM images (Heim, et al. 1996). Lindsay, for example, had always observed DNA on gold, not graphite. In doing so, he avoided the *direct* criticism of graphite work leveled by Binnig, Beebe and others; yet in the rush to closure, *all* bio-STM suffered from guilt by association.

> Tom Beebe's paper of course had a big impact. I think the fact that you can image DNA on metal surfaces is probably not widely recognized. So the papers of the Kawai group are probably not known about by the people that still today say "oh, you can't image DNA with an STM." Now, I know you *can*, but I don't think it's a worthy enough cause to get up on my chariot and say "look, I was right and you were all wrong, here's the definitive work," because there's more important stuff that I can do.... It was important at the time, when it was very controversial, it's important because it's damaged the credibility of techniques such as sequencing by probe microscopy.... But I think the community's been put off because of their knee-jerk – understandable – knee-jerk reaction to the Beebe paper. [SL1, 1/6/03]

Similarly, Guckenberger – and, to a lesser extent, Bustamante – continued looking for ways around bio-STM's problems well into the '90s. Guckenberger found that by using various amplifiers, he could image even with an extremely low tunneling current. This meant that his STM could measure even the very few electrons that tunneled through very thick molecules such as DNA; then, somewhat unexpectedly, he found that ambient humidity influenced the quality of his images. Something about the layer of water vapor that collects on any surface exposed to the air – water vapor that most STMers viewed as a contaminant – actually improved the tunneling mechanism. Although Guckenberger's work was known to many people in the bio-

[13] The article mentioned is Jeffrey, et al. (1993).

SPM community, by the time he published almost everyone had "moved on." Only Bustamante tried to replicate his results, and that only out of stubborn curiosity [MA1, 10/12/01; RG1, 11/14/01; CB1, 10/17/01].

The alternative to making bio-STM credible was to abandon it, usually for AFM. By leaving STM behind, and by putting rhetorical distance between STM and AFM, the new AFMers could cast criticisms of probe microscopy as outdated and relevant only to STM. Indeed, though abashed, many AFMers continued to try to win biophysicists over, and some, such as Hansma, continued to harbor the goal of using probe technology to sequence DNA.[14] Even a cursory look at the literature shows that, after 1992, air STM vanished, practically overnight. STM in fluids, too, disappeared, except for a very small community in electrochemistry. Again and again, journal articles refer to the move to AFM as a kind of unquestionable fact, against which hard-core bio-STMers like Guckenberger had little recourse:

> Efforts have been made, therefore, to image biological material without coating. The results, to date, have often been disappointing, and many researchers in biology have turned their efforts toward atomic force microscopy (AFM). (Guckenberger, et al. 1994)

> In no area has the excitement about this new generation of microscopes been greater than in biology, for the operation of these instruments is not restricted to artificial or unnatural environments.... [I]t has been in biology that the applications of STM have been most controversial.... [I]t is becoming increasingly apparent that further developments in the applications of STM in biology may be difficult, owing primarily to the low conductivity of the samples. In fact, STM may never leave the specialist's laboratory to become a tool of general use in the broader biological community. It is SFM [scanning force microscopy – i.e. AFM] that is currently yielding the greatest number of biological applications and is likely to continue to do so in the future. (Bustamante 1994)

In the '90s, continuing with air STM of DNA meant meeting biologists on their own turf, learning in-depth their methods and ways of arguing – i.e., a heavy

[14] One idea, associated with the Hansma group, is to feed a strand of DNA through an enzyme, with an AFM cantilever resting on top of the enzyme. As the DNA feeds through, the enzyme reconforms differently depending on the DNA base pair it is "reading." If the cantilever is sensitive enough, it can therefore use changes in the shape of the enzyme to tell the sequence of the DNA strand.

investment in time, energy, and disciplinary identity. You could continue in the old way, showing images of DNA without meeting biologists' epistemic needs – as, for instance, Baldeschwieler did. Doing so, though, meant disbelief from biology and marginalization within the STM community. Instead, the builder groups – and the physicists, biophysicists, and chemists who were largely following their lead – avoided becoming biologists by disowning STM. With AFM becoming more reliable, and commercial AFMs coming on the market, STM could be redefined as a dead end. Many former air STMers now saw tunneling microscopy as a limited technique:

> The scanning tunneling microscope is a wonderful microscope for ultra-high vacuum work, or work where you can keep a surface absolutely clean. Layered compounds ... are perfect compounds for scanning probe microscopy because you can cleave them to reveal a flat surface, and they're reasonably stable in air; they don't oxidize. But that's true of almost nothing, which is why I wanted to go beyond scanning tunneling microscopy, to reach the goal of having something that would be generally useful for imaging. I started to realize that the number of samples you could look at in air with a scanning tunneling microscope was vanishingly small. If you look around the room in which you're sitting, there's nothing except perhaps gold jewelry that you could look at with a scanning tunneling microscope. So when Gerd Binnig published his article with Quate and Gerber about the AFM I was very interested. With Calvin Quate's encouragement, I basically abandoned STMs on the spot and started working to make AFMs that would be practical instruments. [PH1, 3/19/01]

Thus, AFM offered new materials to characterize, and hence new disciplines to link with; but it also offered a chance to start over, to cut the tie to surface science and leave behind the botched link-up to biology represented by STM of DNA.

The last act of leaving STM behind was to redefine what an air STM had been doing all along. Here, in making their divorce amicable, the academic AFM builders and the corporate STMers could at last collaborate in creating knowledge. On the one side, the academic groups knew the experience of using an air STM backwards and forwards; on the other side, the corporate STMers had good surface scientific reasons for believing that air STM had never been reliable. The former knew that you could get a "tunneling" current even when you were scraping your probe against a surface;

the latter knew that in air there was always a contamination layer that would obfuscate any real tunneling. So a new picture of air STM emerged – not one that researchers ever fully investigated, but one that sufficed for justifying the redrawing of the field.

> It comes out that in graphite essentially you don't have tunnel current.... The tip is actually in contact, the tip is touching.... Well people pretty soon realized that .... you are sort of modulating, squeezing up and down your surface.... At the beginning, there was lots of, as you say, controversy. Today people don't talk about that anymore. [MS1, 3/9/01]

> Is the tunneling current,... from the tip through the sample to a carbon atom at the surface affected by what the carbon atom is sitting on in the second layer? There's a carbon sitting above the middle of the ring; is that passage of current going to be different than the passage of current through a carbon atom that is sitting above a bond or an atom below it? Yeah, that was discussed, whether it was actually ever resolved I'm not sure. Certainly a lot of empirical evidence but I don't know about the theoretical. It was a goldmine for theorists. I'm sure if you talked to ten people who worked on that they'd give you ten definitive explanations of it. I don't recall if there was ever a universal agreement on that. People kind of moved on. [JF1, 10/19/01]

Reordering the Community and Reinterpreting the Technology

Thus, as in Shapin and Schaffer's *Leviathan and the Air-Pump*, we can see the end of the DNA controversy as a moment when questions of political order (what are the boundaries of the SPM community? who are its leaders? what types of researchers should be joining it?) were settled simultaneously with questions of scientific knowledge (can an STM image DNA?) and technological practice (which is better, STM or AFM?).[15] The builder groups and the start-up manufacturers with whom they were associated had taken full advantage of the graphite gold rush while they could, but by 1990-1 had seen the dangers of letting the boom go unchecked. It is notable that bio-STM had always had skeptics, but that it did not become a full-fledged controversy until *after* Quate, Hansma, and the microscope manufacturers had made AFM reliable enough that it could be sold commercially.

[15] The idea of "coproduction" of knowledge and social order describes this kind of closure well. See Latour and Callon (1992) and Latour (1990) for early uses of the term and Reardon (2001) for a more extended analysis of its implications.

Once off-the-shelf AFMs could be bought, the core commitment to improving bio-STM and making it credible to biologists evaporated.[16] STM's poor, unreliable cousin suddenly became the instrument of choice. In its heyday, air STM helped build an extraordinary user base for probe microscopy – one ad from Digital Instruments (the Santa Barbara-based microscope maker) in early 1990 estimates 300 DI instruments in operation, "more than half of all the STMs in the world."[17] By 1992, the vast majority of DI's users had switched to AFM, as had most other STMers outside electrochemistry and surface science. Thus, the probe microscopy community effectively reorganized around: (A) the presence of instrument manufacturers; (B) the long-standing partnership between those manufacturers and a select few builder groups like Quate, Hansma, and Lindsay; and (C) a very sharp distinction between STM and AFM. This reorganization alleviated many of the frictions that had plagued the community early on. Reorganization allowed expertise to be seen to reside more squarely with the early builder groups, their close collaborators, and their associated start-ups; expertise, in turn, allowed the builders and their network to deal more methodically with newcomers and with "artifacts" (spurious images) in ways that made AFM's entry into new disciplines much easier than STM's had been. In particular, the manufacturers, rather than various builder groups, were now responsible for attracting newcomers, getting them started, and, in effect, taming them to produce the "right" kind of AFM image – a process we will examine in the next two chapters.

Interestingly, air STM still lingers on; yet the places in which it ekes out an existence tell us much about how complete closure has become, and how important commercial AFMs were in that closure. To understand the last remaining air STMers,

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[16] That is, the move from STM to AFM resembles the kinds of technological paradigm shifts seen in much recent history and sociology of technology (Constant 1980; Schatzberg 1999; Bijker and Pinch 1987).

[17] From Faseb Journal, v. 4, n. 13 (1990), p. 1.

it's necessary to point out that STM, by the end, had become an extraordinarily simple and standardized technology. Designs, schematics, and software were readily available to anyone trying to build one. There are very few moving or even delicate parts, and most of the components are cheap and readily available. AFM, on the other hand, is not seen as nearly as simple a device to construct. Very few build their own AFMs from scratch; the designs, schematics, and software to build one are harder to get access to (since almost all AFMs are commercial instruments); and the parts are seen as much more expensive and difficult to make (especially microfabricated cantilevers). Thus, in places where money is tight and therefore commercial instruments seem less appealing, homebuilt air STMs are still favored over AFMs in research. This is most true of China [PH1, 3/19/01], where a number of lab groups regularly publish results on air STM of biomolecules (Li 1999; Zhang, et al. 1994a;Zhang, et al. 1994b). Also, in Europe and North America, air STM has made a very small comeback not as a research tool, but as a science fair or hobbyist's project [NG1, 2/28/01]. Unlike the AFM, air STM is perceived as an intuitive enough concept and a simple enough technology that high schoolers can make them out of Legos and clay.[18] That is, air STM survives exactly *because* it cannot be bought yet is cheap and easy to build.

So what does this tell us about scientific controversy? First, it should be clear from this controversy, and from the events of the previous chapters, just how successful, and yet also how difficult, cross-talk between disciplines can be. Unlike many classic controversy studies, the actors in this chapter came from a wide variety of disciplines; perhaps the nearest equivalent studies would be Trevor Pinch's sociology of solar neutrino physics or Peter Galison's history of high energy physics,

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[18] If any of my readers would like to make one themselves, http://www.e-basteln.de/index_r.htm contains some good hints.

both of which highlight the travails and benefits of interdisciplinary dispute (Pinch 1981; Galison 1997). The biggest difference with probe microscopy, though, is its smallness; a collider facility stretching over acres, or a neutrino observatory buried in an abandoned mine, is a big, expensive, rare artifact/experiment/workplace. By comparison, a probe microscope is small, cheap, and common; even in the earliest days, the opportunity cost for building an STM was relatively low, and through the ’80s and ’90s that cost continued to plummet.[19]

Low barriers to entry resulted in (A) diversity of participants in the field; (B) diversity of instrumental designs; and (C) quick commercialization of the instrument (the topic of the next chapter), which dramatically lowered entry barriers even further. Early on, the diversity of designs was quite low (almost everyone did UHV STM), and, though practitioners came from different disciplines, they were all so focused on a common problem (resolving the 7x7) that their methodological differences were rarely an issue. As the technique became more routine and more widespread, though, methodological heterogeneity became difficult to ignore. For the most part, momentary frictions could be eased by what Michael Lynch and David Bogen call “sleaze.”[20] Sleaze – the fuzziness of possible narratives surrounding any action – allowed surface scientists and academic STMers to claim a common community in some settings, while casting mutual doubt in others; it allowed builder groups to move their experiments back and forth across the line between “applications” and “technological innovation”; it allowed air STMers to make valuable use of graphite,

[19] Again, the idea that the low opportunity cost of building an air STM enticed some people to continue experimenting in that area even after closure of the DNA controversy is similar to the ongoing cold fusion work described in Simon (2002).

[20] Despite the loaded terminology, “sleaze” need not be pejorative. I interpret Lynch and Bogen to be describing a kind of social WD-40, a lubricant that allows any action to be cast and recast in a number of lights, giving actors the flexibility to navigate complex situations, identities, and audiences.

despite what could have been paralyzing doubts about its suitability; and, in the end, it allowed many air STMers to reinvent themselves as AFMers.

Sometimes, though, sleaze can be called out as an inappropriate social practice. As we've seen, this transformation depends strongly on the cultures of controversy involved. The corporate STMers abided the extraordinary flexibility of method and identity of the academic builder groups without instigating outright controversy (though private carping was common). The builder groups tried (usually successfully) to tightly manage sleaze, deploying flexibly interpretable practices when necessary and reining them in when things became untidy. The extraordinary expansion of probe microscopy in the late '80s, though, momentarily spread the social lubricant too thin. The new STMers who started building and buying microscopes and publishing mountains of images could less plausibly portray their experiments in a flexible fashion. That is, when the newcomers showed a picture "DNA," their audience (particularly in biology) heard them making a claim about DNA; the alternative story, that their images of DNA were simply meant to advance the technique, went unheard. Without this flexibility, STMers had to argue on biologists' terms, with which they were largely unfamiliar.

AFM reintroduced the needed social lubrication by recasting a scientific question about the validity of DNA images into a technological debate about which technique (AFM or STM) was more "routine," more "user-friendly," more "general-purpose," and more relevant to a variety of disciplines. Maybe those STM images of DNA were all artifacts, but you could be sure AFM was "really" seeing DNA (and since you "knew" that air AFM could not obtain true atomic resolution, you could do away with "wild" claims about atomically resolved nucleic acids); or, maybe, you really could see DNA with an STM, but under such difficult and narrow conditions that it was not worth bothering. Either way, it did not really matter.

Commercialization reconfigured the ways in which probe microscopists could deploy the discursive flexibility of sleaze; for the vast majority who bought AFMs, probe microscopy became a site for the flexibility of the mundane. That is, for most *users*, probe microscopy today is not the locus of astounding revelation; rather, it is a mundane, unremarked-on laboratory technology, one among many – a lab group might buy one because they have money left over or because it is easy to have one student learn how to operate the instrument. With commercialization, it is easy to add AFM to the tool-kit, and to rely on the safety net of the manufacturer's applications department, and the AFM community, to avoid unintentionally using the instrument in rash or controversial ways. Most users have the discursive flexibility of saying that they are biologists or materials scientists or chemists who happen to do AFM occasionally, rather than dedicated AFMers who live or die by the technique.

For the builder groups, and the manufacturers, sleaze is still useful to navigate between "applications" and "instrument development." Today, though, this kind of discursive flexibility is the prerogative of elites in the probe microscopy community – the very few who still build their own instruments and need to justify building over buying. Discoveries are more commonly treated as astounding or spectacular within this group; yet these are exactly the kinds of innovations that can attract controversy. As we will see, these elites generally rely on each other for a safety net in case of controversy – either through close relationships between builder groups and various manufacturers, or through extended networks of collaboration like that surrounding the Hansma team. In the next two chapters, we will see how the manufacturers handle interpretive and practical flexibility, and how they tame their customers to forestall damaging forms of controversy.

Chapter Seven

Commercialization, Lab Culture, and Tacit Knowledge

As we've seen thus far, the STM and AFM were, from the beginning, embedded in commercial enterprises. In the corporate labs, probe microscopy was meant to draw recognition to the corporation, provide knowledge that might someday be relevant to the company's products, and serve as a research site for personnel who might eventually move to other projects within the company. Even in the academic builder groups, probe microscopy was a proving ground for students who often moved on to corporations like IBM and KLA-Tencor (a semiconductor equipment manufacturer); moreover, early academic STMers depended on the patronage of IBM and Bell Labs in building a sustainable probe microscopy community. Yet it was only in 1986 that anyone began thinking of STM as a product to sell; and, perhaps surprisingly, it was in the academic builder groups (particularly the ones in California) that this idea took shape. As we will see in Chapter Eight, the corporate labs only began halfheartedly marketing a few of their microscopes well after commercialization had transformed the landscape of probe microscopy.

Recently, historians, sociologists, anthropologists, and even philosophers of science have joined economists in studying the commercialization of scientific research. Historians of pre-twentieth century instrumentation, for instance, have gained new insights by looking at the commercial networks connecting scientists and artisans; and among historians of twentieth century instrumentation, commercialization is often a convenient narrative endpoint, a marker that a once-idiosyncratic tool has become robust and reliable enough to be black-boxed (Secord 1994; Jackson 1999; Hentschel 2002; Holmes and Levere 2000; Buchwald 1994). In a somewhat separate literature, scholars such as Derek Bok, Paul Rabinow, John Ziman, and Henry Etzkowitz have debated whether commercialization of research, and the perceived corporatization of the university, inhibit Mertonian norms such as openness and equality and therefore curtail knowledge generation (Bok 2003; Etzkowitz 2002; Ziman 2002; Rabinow 1996; Rabinow 1999).

Though the story of the commercialization of STM and AFM can be read in light of these literatures, I want to approach the process from a different direction. Instead of asking the normative question of whether commercialization disrupts science, I want to examine what characteristics of local experimental culture can foster commercialization, and how different parties to the process represent it as beneficial or disruptive; and instead of taking commercialization as an endpoint, I will look at what happens across this transition, and how an instrumental community responds to its transformation into a marketplace.

To do so, I draw on two literatures. One is the sociology of technology literature (especially the social construction of technology program) (Bijker and Pinch 1987; Bijker 1995a; Pinch and Trocco 2002; Rosen 1993; Mody 2000).[1] Much of SCOT examines how scientific research is steadily transformed, materialized, and repackaged into marketable products, though the products in question are things like fluorescent light bulbs and bakelite, rather than microscopes and spectrometers. Recent work in SCOT makes clear why stopping at the point of commercialization is inadequate; users and consumers are too influential in the continual reworking of technologies to ignore.[2] This seems particularly the case for scientific instrumentation, where, as we will see, consumers often threaten to become producers of technology, and producers must act like consumers in order to maintain credibility.

[1]. I particularly follow the methodological recommendations of Bijker (1995b).
[2] This point is made best in Kline and Pinch (1996). Although definitely not part of SCOT, Kathleen Jordan and Mike Lynch's (1992; 1993) studies of local reinterpretations of the plasmid prep clearly have something important to say about how users of commercialized instruments continually reopen black boxes.

The other is Peter Galison's work on so-called "trading zones" (Galison 1997; 1996). His *Image and Logic* contains rich descriptions of various kinds of intersections between science and commerce (for example, particle physicists and emulsion manufacturers) that resonate with the concerns of this chapter. More generally, though, Galison's "trading zone" concept describes well much of the growth of STM and AFM. I take the trading zone as a "place" where different kinds of practitioners meet, form more or less lasting relationships, construct local interlanguages for mediating those relationships, exchange artifacts, techniques, ideas, shortcuts, personnel, and other cultural materiel, and, in the middle of it all, generate knowledge. Actors' interpretations of the tokens of their exchanges, and even the exchange event itself, need not match up. Nevertheless, they can make themselves mutually understood (for the moment) in ways that may eventually transform the participants' disciplinary homelands. In the case of probe microscopy, we saw how the academic STMers, confronted with an epistemic and institutional dilemma, formed partnerships with representatives of a variety of disciplines – a paradigmatic trading zone. In this chapter and the next, we will see how commercialization meant the elaboration and expansion of this academic group form of life and the increasing literalization of the economic metaphor implicit in the *trading* zone concept.

Bricoleur to Boxwallah

I want to springboard from the trading zone by highlighting some of the colonialist undertones of Galison's talk about pidgins and creoles. I find it odd that Galison builds on an elaborate analogy between scientific vernaculars and trading languages without ever mentioning that the best-known pidgins and creoles emerged from mercantilist and imperialist encounters. I don't want to make too much of this – scientific disciplines, after all, are *not* nations or peoples – but there are interesting ways to play with this aspect of the trading zone concept, particularly in thinking about commercialization. Commercialization stories can sound much like colonialist tales, as told from the vantage point of commercializer/colonizer: the spreading of influence; the entangling of the fates of various communities with that of the center; the enrollment of key members of those communities in the mission of the center; the sending of missionaries out to inform/preach; the disciplining of embodied knowledge and practice to the standards of the center; the exoticization of people at the periphery as profoundly unlike those at the center; and so on.

I want to explore these commercialization narratives by focusing on two characters found in many stories about classical trading zones: the *bricoleur*, well-known from (colonialist) anthropology; and the *boxwallah*, a significant figure in the mytho-history of imperialism in India.[3] The bricoleur should be familiar to science studies, notably from Karin Knorr-Cetina's *The Manufacture of Knowledge*. As Knorr-Cetina quotes François Jacob, the bricoleur "does not know what he is going to produce but uses whatever he finds around him" and "gives his material unexpected functions to produce a new object" (Knorr-Cetina 1981, 34)[4] It should be easy to see this kind of bricolage in the resourcefulness and creative mistake-making that marked the builder groups at Stanford and Santa Barbara.

As Peter Redfield points out in *Space in the Tropics*, colonial projects of "modernization" and "development" often explicitly cite the need to replace bricoleurs with engineers, *bricolage* with *technique* (Redfield 2000). The center sees the "making do" of tinkering as antithetical to rational, planned, thoroughly modern technocracy. This is, indeed, very much how the scientists and engineers who commercialized SPMs see it. They will say that a commercialized microscope is cleaner, more sightly, more efficient, more flexible, more reliable, faster, more

[3] Bricoleurs are, of course, especially associated with Lévi-Strauss (1966).
[4] Bricolage can also be seen in Knorr-Cetina (1999, 88ff.).

friendly, and can do more "real science" for more people than an instrument made by a graduate student. Redfield illustrates, though, that in any real place where colonial plans encounter "underdeveloped" land, people, or ideas, there is continual slippage between bricolage and technique. In this chapter, I want to frame this slippage in the character of the *boxwallah*. Outside of India, boxwallahs are relatively unknown, though their influence on the development of imperialism in the Indian subcontinent is wholly incommensurate with their obscurity. The term refers to the commercial agents (first European and later Indian as well) who permeated the goods, technologies, and practices of Western capitalism throughout the subcontinent.[5] In this chapter, I want to cite the boxwallah as a character typically found in colonialist trading zones. He is the consummate commercializer who manufactures the center's rationalizing mission on the ground, though only by adopting and adapting the local practices, language, and beliefs of those who are being "rationalized."

As such, the boxwallah is a mediator, an agent caught between, yet also profiting from, different players in the trading zone.[6] In the Raj, as in India today, the commercial boxwallah often lags in status behind the ostensibly more learned bureaucrat. Yet, as George Orwell pointed out, the boxwallah's wares can spread influence more effectively than the bureaucrat's edicts:

> [Kipling] never had any grasp of the economic forces underlying imperial expansion. It is notable that Kipling does not seem to realize, any more than the average soldier or colonial administrator, that an empire is primarily a money-making concern.... His outlook, allowing for the fact that after all he was an artist, was that of the salaried bureaucrat who despises the "box-wallah" and often lives a lifetime without realizing that the "box-wallah" calls the tune. (Orwell 1946)

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[5] Naipaul (1991) contains some interesting, if polemical, descriptions of modern boxwallahs. Also, Kaushik Basu uses the *character* of the boxwallah (much as I do) in examining government/industry relations in India today (Basu, et al. 1997).

[6] Mediators and "boundary shifters" are becoming an important site for SCOT. See Boczkowski (2004); Kline (2000b); the final chapter of Pinch and Trocco (2002), and the essays in Oudshoorn and Pinch (2003).

Similarly, in science, commercializers of instruments often lag in status behind research elites. Certainly, in the case of probe microscopy, STMers at the most prestigious national and corporate labs were often skeptical of the motives and the practices of microscope manufacturers.

> IBM had lots of early shots at [commercializing the STM and AFM] and dropped it. The hilarious thing is when you actually go to academic meetings you can just feel it. We walked into a hotel bar in Hamburg for one meeting, the IBM guys were sitting there like Christoph Gerber ... [and] Heini Rohrer ... and they always give Virgil [Elings, founder of Digital Instruments] shit, he comes in the bar and they're like "where are the moneybags?" <laughs> The truth is, it hurts those guys. They did all the basic research and then they dropped it, they could've had the optical lever patent but they were so disconnected that Meyer and Amir actually published that paper in '88 and then they filed the patent over a year later. [JC2, 3/20/01]

Moreover, manufacturers' perceptions of, and resistance to, these elite attitudes often structured the way they designed and marketed microscopes, especially at Santa Barbara. Thus, being this kind of mediator in an instrumental community can drive creativity, but it can also lead to difficult choices and ambiguous identities. In the case of commercializing STM and AFM, this quandary – what I will call the "boxwallah's dilemma" – was twofold. First, probe microscope manufacturers sought to rationalize, standardize, and re-*engineer* home-built microscopes as commercial products; yet they continually found they needed to return to the academic builder groups' *bricolage* and anarchy in order to improve their products. Second, most manufacturers found they needed to strongly differentiate themselves from their customers to enable users to justify buying rather than building their microscopes. Customers needed to be convinced that manufacturers had special expertise and a distinct culture of innovation that allowed them to market much more sophisticated microscopes than users could ever build themselves. Yet the manufacturers also found they needed to strongly *resemble* their customers in order gain credibility with (and appropriate innovations from) their users. Users needed to be convinced that the manufacturers understood users' practices and communities, and could properly make the instruments speak to their concerns; often, to persuade users of this, manufacturers had to actually engage in the kinds of research being done by their customers. Different manufacturers faced this dilemma to different degrees, and engaged it in different ways. Yet none could completely avoid the problems inherent in the boxwallah's dilemma.

Facilitating Factors

The first STMs seemed unlikely candidates for commercialization. They were cumbersome, expensive, slow, unreliable, and took intense training to operate. IBM, though, was always interested in growing a large STM community, and one aspect of that desire was the attempt to make the microscopes more friendly – more standard, more reliable, faster. Big Blue instituted the first in-house proto-commercializations of the STM with Othmar Marti's Blue Box and the Demuth group's CSS instrument. With the advent of the tube scanner, air operation, and mastery of vibration isolation, researchers *outside* the corporate labs began to see that it would take fewer resources and less time to mass-produce an STM. Also, these changes in microscope design coincided with the emergence of an STM community, thereby providing a potential manufacturer with a ready supply of people to buy and use the instruments, and a pool of people to design and build them. Finally, the awarding of Binnig and Rohrer's Nobel Prize in 1986 brought enormous publicity for the tunneling microscope, and validation that the community had accrued enough expertise and finesse with the instrument that becoming an STMer no longer presented much of a risk.

Thus, this was the era when the first STM manufacturers emerged, associated not with the corporate labs, but with the West Coast academic groups. Several aspects of Quate and Hansma (and Baldeschwieler and Lindsay) group culture naturalized this leap to commercialization. Above all, since these groups styled themselves as instrument-*building* groups, they trained their students to design and make more microscopes than they could use – there were always several operating at once, none of which lasted very long since an instrument had little value once a student had shown he or she had mastered a new design innovation and could make the instrument work reliably. Microscopes were, in a sense, disposable; as a Hansma maxim put it, "one microscope, one journal article, and move on" [SG1, 3/27/01].

When left to their own devices, the instrument builders used this surplus of microscopes to conduct random, playful, undisciplined experiments. Soon, though, by choice and necessity, they made these microscopes available to postdocs and other visitors. With these visitors, the academic groups took on a more interdisciplinary character. As the visitors left, they took the microscopes with them, spreading them to new locales and disciplines. Thus, the academic groups were already bartering microscopes for knowledge and credibility and cultivating demand for their wares. Interdisciplinarity also led to a strongly felt division of labor, with "builders" making microscopes and "runners" forging new applications [JH1, 6/10/02]. This created a fairly efficient (even factory-like) turnaround in moving new microscopes from design, to construction, to use in characterizing new samples [MK1, 10/12/01]. Through collaborations, the academic groups built a large network of contacts, with whom they circulated people, materials, designs, methods, and preprints. That is, they constructed a vibrant, fast-moving "gift economy," of the sort Davis Baird has cited as a precursor of scientific commercialization (Baird 1997). For instance, Stuart Lindsay and others who built microscopes with digital controllers repackaged their control software and either gave it or sold it cheaply to fellow builders [SL1, 1/6/03]; likewise, the Quate group was well known for circulating its microfabricated cantilevers as a way to help new AFM builders [CP1, 3/19/01]. These donation networks both grew the community of builders and enhanced the donors' prestige within that community. Circulation and gifting spread knowledge, skills, and instrumental reliability, and made more likely the future transmission of (and desire for) more skills, knowledge, and reliability.

One last factor facilitated commercialization of STM and AFM from the West Coast academic groups – place. It mattered that Quate, Hansma, Baldeschwieler were all in California, although it mattered differently at either end of the state. In Palo Alto, the Quate group had always been well-integrated into the research life of Silicon Valley. Quate's career was closely tied to the pioneers of the Valley (Frederick Terman, Edward Ginzton, and the Varian brothers); his research interests moved smoothly from the first wave of Silicon Valley industrialization (microwave technology) to the second (non-destructive testing for the microelectronics industry); and many former students were embedded in the Valley's big firms (such as IBM).[7] In the '80s, as the Silicon Valley "phenomenon" emerged, the Quate group increasingly harmonized with a nascent (and somewhat stylized) Silicon Valley "culture" (Hall and Markusen 1985; Saxenian 1993). That is, Quate team members easily characterize their lives as woven into "traditional" Silicon Valley values: the "centrality of work," "positive feeling towards work as the opportunity for innovation," "entrepreneurialism," competition, individualism, and a tendency to work hard and play hard (Castells and Hall 1994, 21-4). Commercialization of almost anything, but especially high-tech instrumentation, had a long history at Stanford, and the Quate group saw itself as part of that history.[8]

Commercialization at UC Santa Barbara and Caltech, on the other hand, was stimulated by the vast Los Angeles military-industrial zone. For some students, the

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[7] Bill Leslie (1993) nicely narrates these transitions and the role of the commercialization of academic science in the development of the Valley.
[8] Lenoir and Lecuyer (1997) is an especially well-documented study of one such case that bears many similarities to the commercialization of probe microscopy.

nearby defense industries offered a place to work after graduation, and a chance to hone technical skills before starting companies of their own:

> [After my postdoc at Caltech] I took a job at this aerospace company called Aerojet Electrosystems. Surface science lab doing surface science. I was looking at materials for infrared detectors. XPS, Auger, that kind of alphabet soup. That was starting the buildup of Star Wars and there was a lot of money flowing into the aerospace companies and, I wasn't really happy there, it wasn't really appealing to me. So I didn't stay there very long…. I started a company in my apartment, and the idea was to make little piezoelectric translators to go in vacuum chambers. That's something where we thought there was a real customer need. I did that, I spent all of my money. After I ran out of money I went back, started talking to Baldeschwieler because I knew he was well connected with the money, and so we decided to start a company, and that was called Quanscan. [PW2, 3/30/01]

Often, the aerospace sector acted as a negative stimulus. Its presence made Santa Barbara-Los Angeles a high-tech haven, yet few of the Baldeschwieler or Hansma people imagined themselves working permanently in the defense industry. One veteraon of this era describes the employee pool of Digital Instruments, the Santa Barbara-based STM and AFM manufacturer:

> [Virgil Elings] was a professor at UCSB when he started Digital Instruments. There was a severe economic downturn in Santa Barbara county due to the Space Shuttle Challenger disaster, and a general recession in the military/industrial complex. Since Santa Barbara, postcards and red-tile roofs aside, was a military/industrial complex town there were all these very bright people coming out of defense layoffs and graduating from UCSB that were hungry for work, and he gave us jobs. He gave us something intellectually engaging to do that we could be excited about that wasn't the military. For many, it's tedious to go to work on weapons and build thing you hope no one will ever use. After a point, you're just not particularly proud of it. So he was able to pick and choose among these defense refugees, if you will, primarily from Hughes Aircraft and Delco and the UCSB graduates that would've fed into those places, and assemble a great talent pool. [JW1, 10/18/01]

Though many SPMers rejected the aerospace jobs they could have found in LA or Santa Barbara, they also valued the lifestyle of these communities. Indeed, in becoming refugees at the microscope manufacturing startups, many scientists and engineers crafted their lives to fit with what they thought those locales, and their new enterprise, symbolized:

> I had a lot of energy, I was young. It's ... the high-tech startup myth.... Ten, twelve people in the company,... there's this new potential, new energy.... One of the reasons I came down here was so I could run on the beach and wouldn't always have to wear shoes. I used to run on the beach every other day six miles right out from the university west to Coal Oil Point and back. Just that spirit, being in the sun and you know being able to go to the beach for two hours in the middle of the day and feel good and take a shower and come back. Just that energy, it not being a traditional engineering, conservative environment. That's what I needed in order to thrive. It was spun off from the university in kind of a rebel spirit, kind of a classic 'Rebel Without a Cause' type spirit on all of our part. [JM3, 10/18/01]

The Early Days

The first STM "start-up" came out of the Quate group in 1986. Doug Smith was in the waning days of his graduate work, with the eventual prospect of joining Binnig as a postdoc in the IBM enclave in Munich. He had had a checkered graduate career (recall the tempest over his vibrational spectroscopy results from Chapter Five), but had built a wide variety of STMs (and even AFMs), and had cultivated contacts across the STM community. This first STM commercialization was of a piece with the bricolage and hobbyism of the builder groups. Smith's STMs were not mass-produced, standardized microscopes, but rather hastily handmade kits, built in a garage and sold to a few of Smith's contacts who seemed to have the skill but not the time to build one themselves.

> CM: Tell me a bit about Doug Smith's STM company?
> MK: The Tunneling Microscope Company. Doug had come up with this really cheap way to make microscopes in air and this one guy in the navy wanted that microscope, so Doug said "well, for $5000 I'll make you the head." He needed some tips so I made them, plus some of the wiring for the electronics for the microscope. That was just my second year. He sort of did this thing at night and sold maybeva dozen of these things. Basically Dr. Quate said, I remember this line, "graduate students work, eat, and sleep, and most of the time they go hungry." You can't have a company and be a graduate student at the same time. So Doug had to finish up and move out. [MK1, 10/12/01]

Interestingly, like users of the Demuth STM, users of Smith's STM expected to have to tinker with it and adapt its design for their needs. The Smith STM was more of a kit than a black box. One of Smith's customers, John Foster at IBM Almaden, remembers having to build much of the instrument himself to get it to do what he wanted it to do.

> JF: It was the most elegant and simple little device that Doug, and I suspect a few of his cohorts at Stanford, had engineered....
> CM: By the way, were you doing all this with electronics that Doug Smith had provided with the STM or did you build new stuff?
> JF: No. I got the pre-amplifier from him but, no, the electronics – actually, this is an interesting point, that's a good question. No is the answer, I just got the object, the STM itself from him, which is literally just a bunch of plates, I think it's $5,000 for the stuff.... But then to drive such a thing, the electronics to do that, I mean I had also been a scanning probe microscopist myself so I understood how it worked extremely well. I mean you just go get a rack of electronics and just buy little bits and pieces, an HP thing here, an electronic thing there and you're done. The electronics for these things is ridiculously simple. [JF1, 10/19/01]

In Santa Barbara, meanwhile, a more serious venture was taking shape. One of Hansma's colleagues in the UCSB physics department, Virgil Elings, learned about STM from a visiting professor, Niko Garcia, a Spanish physicist who collaborated with the Zurich team, [VE, 3/20/01]. Elings had developed other instruments for the market, and he saw potential for a commercial STM. Accordingly, he asked Paul Hansma to help him found a tunneling microscope company. Hansma, though, was no more inclined than Quate to become *directly* involved in a startup company. Instead, he agreed to help Elings design a prototype and consult on further collaborations. Thus, Elings' STMs, from the start, had a UCSB look. Elings, though, had begun to face the boxwallah's dilemma. On the one hand, he approached the native practices of the Hansma group and the STM community with intense skepticism, seeking to rationalize and discipline the bricolage of tricks, recipes, and "superstitions" they had accrued.

> CM: Had you seen a lot of other people's machines at that point?
> VE: No, we never were people to go off and learn from other people. If you've done a lot of that in life you'll learn that it's a waste of time. Sorry, but that's it. Although we did borrow a microscope from Paul [Hansma], because we wanted to get something going. We didn't like it, so we redid his microscope. We knew we wanted to make something commercially. You can't deal with this stuff that the people do in the lab that takes a graduate

> student to operate and doesn't work reliably. So we changed a lot of that, put in some interesting things. We debunked lots of rumors about the things and that was sort of fun. [VE1, 3/20/01]

At the same time, Elings reproduced much of Hansma's hobbyism and undisciplined activity in order to replicate the instrument:

> Scanning tunneling microscopes in particular are not terribly difficult to build.... Virgil decided he was going build one, and his excuse for building one was his son's high school science fair. So Virgil and his son built an STM. That was actually sort of the prototype for the Nanoscope I. The display for it was a storage oscilloscope, it was very crude. They entered it into the Santa Barbara high school science fair, and they won third prize out of three. It was fully functional, they were actually able to demonstrate that they could see the atomic lattice of carbon atoms on graphite. The reason they won third prize out of three is because the judges, who were all high school science teachers, unanimously said "everybody knows you can't see atoms." [MT1, 2/26/01]

Having constructed a prototype, and having seen that, by the mid '80s, an STM was "not terribly difficult to build," in 1987 Elings formed a company, Digital Instruments, to design, build, and sell more of them. In doing so, he had two major advantages: experience with commercialization, and a nearby pool of people to join his company. Both stemmed from his experience as a physics professor at UCSB. In the '70s, as UCSB sought to transform itself into a major center of high energy physics (the area in which Elings had been hired), Elings' obstinacy and swagger brought him into conflict with his departmental peers. UCSB dealt with this by sidelining him into what most of the faculty saw as an uninteresting, if lucrative, backwater – instrumentation.

> When Virgil was a young professor at UCSB, he had a bad boy reputation. According to him, he was never "with the academic program" and was a problem for his department. I don't know all details but he had a bit of a tenure struggle because people knew he was brilliant yet he wasn't toeing the line in terms of producing papers and so on. He had this odd notion that students ought to be trained for jobs, and thought that would be a good goal for the University. So there was this Master's in Scientific Instrumentation program that gave him something to do (and I'm guessing a place for the department to put him). It was an excuse to get master's students into a physics program that had a complete lack of students during the anti-war and anti science years of the late '60s and '70s. Also, UCSB Physics was not yet established as a major organization yet. As we know, it is one of the best in the world now. So they had this program where they tried to rehabilitate (my

> term) people who got degrees that were fairly useless for getting jobs, like biology. I entered with a Masters in biochem, myself, and doubled my earning potential. And that was a damn good thing because the Department could get their master's degree fees, and we could get training that made us employable at high wages – win win. [JW1, 10/18/01]

The Master's of Scientific Instrumentation Program started out with a fairly orthodox pedagogy – teachers, lectures, labs, textbooks, final projects, etc. Occasionally, though, UCSB professors would bring instrumentation needs to Elings, which he would turn over to a student as a final project. Eventually, he saw that students who picked up these "real world" problems were learning the basics of instrument design much more quickly than those who sat through lectures or attended labs:

> This instrumentation program, we weren't sure what it was going to be, but we thought that you gotta know about the physics of instrumentation, so we'll have guys come in and talk about particle detectors and low temperatures, high vacuum, all those really important things in life that everybody should know about. Of course, in the real world, these things weren't important at all.... We'll have some nice labs and we'll have lab write-ups that people can read and follow and not use their brain, this'll really be good. Well, we had an NSF grant, the equipment was slow in coming and we didn't get the lab started on time, so some of the guys started doing real problems for chemists and biologists [on campus] who had some instrumentation problems.... When we got some of these "labs" of ours working it became really clear that the guys who were doing their own problems were learning so much faster than people reading instructions that it was pretty pathetic. We decided also at some point that if you're going to work on real problems you only have so much time in the day and we got rid of lectures. [VE1, 3/20/01]

In this way, Elings, like Hansma, saw that making many educative *mistakes* can lead to efficient solutions more quickly than trying *not* to make mistakes:

> I did this instrumentation program 15 years, we probably did 150 projects for people around campus. When we got to the end of every project, we realized that if you were going to do it over you wouldn't do it the way you did it. But the student isn't going to do it over, so it's a failure.... It's not like solving this problem in which there's one answer and okay you found the answer so that's success.... I realized, "you dumb shit, you think that's failure, and that's life. You've done this enough times now to realize that you're not going to get 'the right answer....' You keep thinking you can sit down and figure out a problem you haven't done before and then start doing it and thinking that the answer ought to be what you thought at the beginning." It isn't at all – it's completely different! You learn too much while you're doing it. So I said, "oh my god, we got to change." We have to change and do things in a way in which everybody realizes that the way they start out won't be the way they end up. [VE1, 3/20/01]

This is the essence of what I mean by the boxwallah's dilemma – the sometimes discombobulating understanding (audible in Elings' exasperated speech) that instrument manufacturers' rationalizing, standardizing, high-tech mission can best be achieved by resorting to the same chaotic, idiosyncratic, low-tech means they are trying to supplant.

For Elings, as for Hansma, this understanding was based in a particular view of the relationship between tacit and formal knowledge. His experience running the instrumentation program showed Elings that formal knowledge – i.e. classroom or textbook knowledge – counts very little in building an instrument. Instead, know-how and Polanyi-esque "personal knowledge" are key (Polanyi 1967). To Elings, conventional wisdom and orthodox educational training both seem irrelevant. His students and employees might use formal knowledge – equations, constants, textbook formulations – but the key was picking up these useful scraps quickly, individually, and informally, using raw intelligence rather than disciplined pedagogy:

> We had our smart guys. Smart guys are where it's at. We had this philosophy that you can't know what you're doing. I tried to permeate that through the business. People say "well, don't try that because I know that won't work." We don't put up with that crap.... In fact the university engineering department asked me one time in some condescending way, "now what courses could we have that would be useful for industry?" As a joke I always wanted to say "could you teach them how to put a nut on a bolt?" Because that's what they think we do in industry. No, I said "why don't you have a course called Inventing?" They said "oh yeah, what would that be?" I said, "well you guys do these goofy courses where every problem has one answer and I'll memorize the answer and everything's fine. Why don't you do a thing where each student has to bring in a problem, I don't care if it's a problem we can solve or not, but somehow he's got to come up with an invention to solve this thing." And of course they asked the really important question "how do we grade the students?" [VE1, 3/20/01]

For Elings, educational pedigree mattered little. If ability in making instruments came from *doing*, rather than from formal *training*, then one degree was as good as another:[9]

[9] There is now a large literature on "learning by doing" in economics; see Mowery and Rosenberg (1999) for the latest word. I have found the "communities of practice" literature in sociology useful in thinking about how learning and doing mutually evolve: Wenger (1998); Lave and Wenger (1991).

> At some point we started to let in people from outside of physics and brought in engineers, biologists, chemists, a guy out of psychology who was our best designer at DI. So you let these guys in to do instrumentation.... It became clear that the last four years of your life [in college] didn't make any difference. All the subject matter [you learned] didn't make any difference. A smart biologist on a particular problem can learn what you know in a couple weeks in order to do this problem.... Academia spends all this time doing course content crap. It turns out course content isn't important. I can't remember anywhere inside DI where we were solving problems in which we relied on course content that somebody had in order to figure anything out. We were relying on the brain to figure something out, not course content. [VE1, 3/20/01]

This devaluing of formal education meant the professional backgrounds of Digital Instruments early employees were quite heterogeneous. As we've seen, Elings' son was his "lab assistant" in building his first STM, and many of the people who became DI's leading designers joined as undergraduates at UCSB:

> I was just an undergrad studying electrical engineering at UCSB, and needed to pay my way through college, so I went to the job board and there was a little thing that says they were looking for somebody to solder circuit boards. Whatever, electrical engineering, I can do that.... So I go in there with my tie and suit on, and there's Virgil flinging boogers left and right because he likes to pick his nose, throw boogers at you. "Job, sure, all right, don't wear that stupid suit the next time you come...." I started working on some of the designs and figuring how to make tips early on, and then gradually once I got my degree I got a job and I've just been there forever. [DB2, 3/23/01]

Still other key employees were people who simply wanted to live in Santa Barbara:

> Actually I have a degree in history and I was a graduate at UCSB and was running a bookstore when I graduated, and figured I'd go into publishing.... One of my coworkers knew this ex-professor from UCSB who had this little shop that was making these things called scanning tunneling microscopes, so I went to the library and looked it up and it looked kind of interesting, so I went down and applied for a job and they told me to get lost, so I went back a week later and applied for a job, Virgil told me to get lost, went back a week later and he said "anhh, all right." ... Obviously at the time nobody knew that it was going to be as successful as it ended up being. It was just a collection of pretty strange people, building this thing that was used by pretty strange people. Present company included. It was kind of a garage shop operation. It had actually started out in a dentist's office. When I joined they had recently moved to what was an old bank building. So it was what I assume to be a fairly typical high-tech startup. [MT1, 2/26/01]

The most important *early* source of employees was the instrumentation program; there, too, students came from all kinds of educational backgrounds:

> My background was in humanistic psychology and Eastern mysticism, that was undergraduate, but as a kid I was kind of a little laboratory rat. So ... I kind of came back into science and got a master's degree in scientific instrumentation at UCSB.... It was a very independent program. I liked it. The university eventually phased it out because it was less theoretical physics and they wanted more of that. But it was a very practical program.... Most people had one big project which was their thesis, and I did also. I did some biomedical stuff and also a wind energy data logging system for use in Inner Mongolia, it was solar-powered.... So when I got out after three years in the master's program, I went up to northern California for a year, where I used to live, and then decided that I had a good thing going in Santa Barbara after all, and so gave Virgil Elings a call. He had already started Digital Instruments. [JM3, 10/18/01]

DI was not Elings' first commercialization effort. The instrumentation program not only supplied him with engineering talent, but also designs and prototypes for potentially marketable products, several of which he tried to capitalize on. Elings viewed management skill as similar to instrument-building skill – dependent on tacit rather than formal knowledge, something to be learned by doing, something not marked by possession of an academic degree such as an MBA. Notably, his earlier commercialization attempts came to be seen as educative mistakes.

> He had had previous experience with other business ventures. I don't know if you've ever seen the optical illusion that's two parabolic mirrors.... That was actually discovered by a graduate student at UCSB.... Well Virgil patented that and decided to produce them. But he couldn't make any money because he wanted to make them out of such good mirrors that of course they were ridiculously expensive.... The next venture was ... a company called Particle Sizing Systems, which basically took some existing technology and improved it through sensors and better software.... Virgil didn't find it particularly technically interesting, I mean once you build the thing there's not really huge improvements that you can do.... Third venture was a fluoroscope.... It would measure blood oxygen levels. That was a frustrating experience for Virgil because that was the first time that he was really dealing with non-scientists, he was dealing in fact with MDs. He just couldn't handle their mindset, because he'd bring his fluoroscope in, which was much more repeatable and much more accurate than the existing models, and ... the doctor'd say, "oh, but the output doesn't match what I'm getting from the existing device." He'd say "well, that's because your existing device is not accurate or it's not repeatable. Look at the lack of repeatability in the measurements that you're currently making," and the doctor would say "yeah, but it doesn't match what I'm currently getting." So Virgil would basically just lose it. [MT1, 2/26/01]

The centerpiece of the DI team was Elings' star pupil from the instrumentation program, John "Gus" Gurley. Technically, Gurley was a co-founder of DI, meaning he and Elings were the only employees to split the profits. Early on, DI had no venture capital money and all expenses came out of Elings' pocket; and Elings banked much of the early success of the company on Gurley's talent and drive.

> Gus was his best student probably and a very talented digital designer and software engineer. When the business started Gus was actually up in the Bay area working on flight simulation systems for the military.... The deal was that Virgil would bankroll the project and Gus would basically contribute a year of his life to getting this thing working, ... Anyway, Gus had been working in some dark hole someplace for a year of his life, literally you'd walk in in the morning and Gus would already be there and you'd leave at night and Gus would still be there and you wouldn't see him at all in between, he would just be holed up in this place working on the Nanoscope II. [MT1, 2/26/01]

Together, Elings and Gurley formulated a design philosophy that, again, drew on lessons from the instrumentation program. If, as Elings believed, all technical projects achieve their final form circuitously, through mistakes, serendipities, and culs-de-sac (where the most successful projects acknowledge and capitalize on mistake-making), then DI should be prepared to be surprised by new applications, innovations, and reinterpretations of what an STM was and how it worked. Just as a good instrument-builder picked up new skills quickly, with little formal training, so a good commercial STM would adapt to new applications quickly via a flexible architecture.

The key to this flexibility was powerful software and state of the art microprocessors. That is, Digital Instruments' STM would be a *digital* instrument – a radical advance in an era when the IBM PC was only five years old.

> When you make a new instrument you actually don't know what the "best" solution looks like. So therefore we learned that if you're going to do a solution you have to be flexible, so when one day you wake up and figure out what the hell it is you're doing, you can actually do something without saying "oh, it's too late, I already got the circuit boards done." So we decided it all had to be digital so that when we figured out what these things did we'd in fact be able to change the programming and make it do it right.... As a great instrumentation program operator I would always say "I don't really care what

> your problem is, I know what the solution looks like – input, microprocessor, output." [VE1, 3/20/01]

Elings knew that having the first mass-marketed commercial STM would be a powerful advantage over DI's competitors. An early opportunity to do so presented itself at the 1987 STM meeting in Oxnard (just 40 miles south of Santa Barbara). Gurley's digital controller, though, was still in development. Instead, Elings pressed his analog prototype (the modified Hansma design from the science fair) into service. This instrument, the Nanoscope I, was, by most accounts, a solid and reliable but fairly unsophisticated air STM. Though more complex than Doug Smith's STM, the Nanoscope I was still considered (by both designers and users) not quite adequate for "real" science unless tinkered with by the user.

> In the beginning I called it the toy business. There was no use for a scanning tunneling microscope, other than to show your friends that you could scan graphite atoms.... So what we were doing was supplying instruments to people who were going to make them themselves, our claim being that we could do it cheaper and better.... For us it was sort of lucky because later STM arrived at a decent place, and that was you got something out of it. As opposed to the toy business, where you were making things that had no use. I'm telling you they had no use.... As long as there's nothing new that people see with these things, I don't know exactly what we're doing. I remember some gal at Los Alamos saw spiral grains in these low temperature superconductors, that was the first time to me anyway that somebody using one of these things saw something they didn't know was already there. To me that was the end of the toy business. [VE1, 3/20/01]

The "toy business," for what it was, was quite successful. This was the era of the air STM gold rush, when the STM community was expanding rapidly and the community had yet to conclude what could or could not be done with an air STM. Recall from Chapter Six that in 1990, DI estimated around 600 STMs in the world, more than half of them its own. At the time, DI had a near-monopoly on commercial instruments, but Elings knew his "toys" would soon be eclipsed by the nascent startups in Berkeley, Phoenix, Pasadena, and Palo Alto. The first step out of the toy business, therefore, was Gus Gurley's completion of an all-digital controller, marketed as the Nanoscope II in 1988. Digital control meant a less cluttered, somewhat more flexible, more user-friendly interface. Notably, the controller's software offered *DI* greater flexibility; adapting the controller for new applications now meant changing a few lines of code rather than rewiring the instrument's electronics.

It's possible to see what digital control meant by looking at the transition from analog, home-built instruments to the Nanoscope II. By the late '80s, home-built analog STMs and AFMs had become both sophisticated and massively complicated. Racks and racks of electronics, with knobs and dials and switches, allowed trained users virtuosic control over tip movement and other parameters – at one point, the Hansma group's AFMs had 120 different knobs, switches and other controls, giving their users an extraordinary range of control [BD2, 10/18/01]. Yet having that many controls could also impede research if the user became hopelessly entangled in the analog jungle.

> I wasn't easily weaned off of the homemade instruments because I had become stubbornly attached to them and it took quite a bit of prying to get me to switch over. Once I did switch to the commercial instruments we found them to have a higher throughput in it was easier to use and just as good. So the commercial companies had sort of streamlined, made it easier for the users to spend less time fussing with the instrument and more time analyzing data and taking data…. I had learned the technology at a lower level and whatever the higher level changes that occurred in going from a low level homemade instrument to a higher level commercial instrument weren't much of a transition…. Life became easier with the commercial instruments which I suppose is what you would expect. Digital Instruments, you can tell just by their name – I was using analog equipment, purely analog equipment and so lots of knobs and oscilloscopes and stuff like that and that all got packaged into something that looked like a computer and so it did make life easier. Sure, there were still groups in the world that, that would only work with homemade instruments and that's true to this day. But there's probably several thousand commercial systems in the field now. [MA1, 10/12/01]

Thus, by eliminating many of the knobs and switches, and automating their functionalities, digital control made it easier and less time-consuming to learn to use an STM or AFM. For a small elite of avant-garde probe microscopists, though, digital control is seen as ceding subtle, sophisticated operation. DI assumed most users preferred spending less time learning to use their microscopes and would not object to automation. By making this assumption, though, Digital gave itself the leeway to build extremely opaque black-boxes where most of the details of operation were hidden from the user, and where users were highly constrained in their ability to tinker with the microscope. In other words, digital control was a disciplining tool that allowed DI to enforce particular kinds of behavior on its customers. This was central to one-half of DI's solution to the boxwallah's dilemma. By making instruments to which users had little interior access, DI heightened the epistemic and cultural differences between its engineers and its customers – a tactic aimed at making it easier to sell to subdisciplines with little instrument-building tradition.

STM to AFM

The end of the "toy business" also brought a tightening of the bonds between DI and the Hansma group. Early on, relations with UCSB had often been distant and occasionally even strained. Several factors contributed to awkwardness: Elings had a precarious reputation in the UCSB physics department; the Hansma group had no tradition of commercialization, so relations with the startup were initially without precedent; and, Hansma had moved on from STM to AFM. For years, Hansma tried to convince DI to commercialize the UCSB AFM design, but Elings resisted so long as the group's AFMs bore too many marks of unreliable, student-built bricolage.

> At first, when Digital Instruments was just Virgil and a few people, we would invite him over whenever we had something new. We went through seven tunneling AFMs that worked in the hands of students, but he wasn't tempted to commercialize them yet. He said, in effect, "If I come into a lab and somebody shows me great results they got recently, that's one thing. But if I can come in a lab and see an AFM running and getting good research results while I'm in the lab, then I'll see something that can realistically be commercialized." When he came in the lab and saw it working well while he was there, he realized that this might be a commercial product. [PH1, 3/19/01

Elings remained, as usual, intensely skeptical about the AFM. After all, it had lower resolution than the STM, used a more complicated detection scheme, and had a reputation as a difficult instrument.[10] Demand for AFM seemed to be growing, though, so he assigned an engineer, James Massie, to work with Barney Drake to commercialize the Hansma instrument. As Drake describes it, commercializing the Hansma AFM was a difficult process; instruments made by graduate students for a leading AFM lab could afford to have idiosyncracies, small unreliabilities, and a tendency to break apart [BD2, 10/18/01; JM3, 10/18/01]. After all, one of the pedagogical methods of the Hansma group was to have students rebuild broken microscopes so they could understand the instrument's working better. DI believed, though, that a commercial AFM should be more rugged – they didn't want users to be continually breaking it and sending it to California for repairs – and more user-friendly – users should be able to understand what the controls do without having to go through the process of building the instrument themselves.

With the decision to develop a commercial AFM, DI and the Hansma lab forged a licensing agreement giving DI rights to the AFM patents, in return for fees and a stream of DI products to UCSB [PH1, 3/19/01; VE1, 3/20/01]. Eventually, this agreement made the Hansma patents some of the most profitable in the history of the University of California; it also made the Hansma group prosperous in equipment, helping secure its elite position in the probe microscopy community. Also, the agreement blurred the boundaries between DI and the Hansma lab. Hansma's students migrated to DI in greater numbers, Hansma and DI personnel consulted more frequently, visitors to Santa Barbara could see cutting edge science done with DI instruments in the Hansma lab, and the circulation of knowledge, people, specimens, and equipment that typified the Quate and Hansma groups expanded to include DI as

[10] Recall from Chapter Five that modern AFMs use a complicated optical lever scheme that require significantly more manual operator adjustment than a typical air STM (although less than the older STM-detection AFMs).

well. This story from a Hansma postdoc gives the flavor of these quick and informal interactions in the early ’90s.

> We’d go down there and just do stuff at DI. Once Jason and I were working on a calibration method for the cantilevers, and we needed to get a collection of cantilevers. So we went down and there was a place where they put cantilevers that had been returned or had minor defects or whatever, it’s like a table just full of these things. What we really needed were cantilevers that had slightly different thicknesses for this calibration. So we went down there and we just sat there and we broke out cantilevers from 20 different wafers, which you would never be able to do anywhere else.... You’d go down and you could talk to the people doing all the development. I remember having many discussions with Gus Gurley about “well, software should do this, software should do that” and things like that. Some things they did and some things they didn’t. There are things in there now that I remember I specifically suggested to them. But it still had very much of a startup flavor to it, even if it might have had 50, 60 people. [JH1, 6/10/02]

Hansma and Elings and are astonishingly different people – Hansma projects openness and generosity, Elings irascibility, contrariness, and skepticism. Yet they built organizations that, after the AFM agreement, meshed in a dynamic and productive (if also difficult) way. For Hansma students who moved to DI after graduation, the transition could be jarring; but there was much in the DI “way” that resonated with the Hansma group’s methods [CP1, 3/19/01; JC1, 3/20/01]. Elings and Hansma might have treated their underlings quite differently, but they both peppered their conversations with guiding maxims and proverbs (“do everything as poorly as you can” or “you can’t know what you’re doing”), both structured their organizations around the primacy of tacit knowledge, and both drew on personnel of diverse educational backgrounds; thus, with practice, Hansma veterans could easily apply the lessons and the work ethic of graduate school to their jobs at DI.

Importantly, both men encouraged off-task, extra-experimental activity to stimulate tacit skills. In the Hansma group this meant self-cultivation: hobbies, sports, travel, meditation. At DI, employees congregated for weekly “inventing sessions,” brainstorming technical problems unrelated to probe microscopy:

> One of the important things is to have no structure, so that there's nobody who can say "let's do it my way because by the way, I'm the boss, if you don't do it my way I'll fire your ass...." It's interesting in the company, once you start churning away and everyone's busy there's no time for inventing. So we used to hold these inventing sessions once a week. We'd do it for an hour and we'd just bring up random stuff and do inventing because everybody's too busy during the week to even think about stuff. What was sort of interesting after a while is that if Elings was sitting back there and I *didn't* say "that's the worst shit I ever saw" they would feel bad because it meant I wasn't even interested. So it's interesting. Now I thought, strangely enough, it didn't matter what the subject matter was, that I was teaching these guys inventing. [VE1, 3/20/01]

Through hobbies or inventing sessions, people at both DI and UCSB accrued tacit skills, and applied those skills in offbeat ways. At both UCSB and DI, this led to playful uses of the microscopes, and undisciplined methods of sample preparation:

> We didn't do a lot of applications and we never have. Our idea was to supply instruments. Sort of like the instrumentation program. We educated students in making things that do something, all right. But then to use the thing to do something, that's somebody else's job.... We had this weakness that every morning I would go in and run the damn microscope and get hooked on it, and so it wasn't til about 2 o'clock in the afternoon I would start to try and do something real. But we always cranked the scan down, try and see atoms on anything and we would be surprised. Just a weakness I guess. My son showed atoms on a gold surface in a junior high science fair before it was published by IBM. [VE1, 3/20/01]

> [Virgil Elings] can be a bit of a crusty guy, or rather put on the act that he's a crusty guy. It was quite a shock for me coming from Paul Hansma's lab, because Paul's very genial and friendly and when you had an accomplishment in Paul's lab he would make it a point of shaking your hand and saying congratulations. You could create gold out of the air or you could levitate something in front of Virgil's hands ... and he would look at it and go "ah, that's a piece of crap." So we brought him over to show him this AFM that we had been operating and of course he said "ah, looks like a piece of crap, what are you imaging there, looks like shit to me...." Then he reached over and grabbed a Polaroid picture and got a pair of scissors and cut off a little piece of the Polaroid picture, said "IMAGE THIS." <laughs> I had the same mindset that the people at IBM had in the early days, that the things that you image should be atomically flat, precision surfaces, and the idea of taking a piece of paper and sticking it in was just crazy. I was like "well, I don't know if this'll work" and he goes "TRY IT. This is what our customers'll do, they'll put any kind of shit under there." As I got used to Virgil's style I realized that he needled and teased us to challenge us to do more than we though was possible. [CP1, 3/19/01]

This last quote points to a picture of the customer at DI as someone who might do irrational, inexplicable things that good microscope designers should plan for (and attempt to tame). This stylized distinction between builders and users embodied DI's way out of the "boxwallah's dilemma," in which customers and builders were assumed to live in very different cultural systems – though a few amenable customers might be "progressive" enough that they could be used to explain to their subdisciplines the proper way to use the instrument.[11] Let us examine some consequences of this distinction between engineers and customers, as well as some alternatives offered by DI's competitors.

Youth and Exuberance at Park

So far, I have made my story about STM and AFM commercialization revolve around Digital Instruments. There are good reasons for lavishing attention on DI before moving to the other startups. First, DI is today, by a wide margin, the largest, most successful probe microscope manufacturer. Understanding DI is vital to understanding the social contours of the probe microscopy field. Second, the local methods and philosophies of Hansma and Elings present special nuances for science studies that are only echoed implicitly by other startups. Finally, DI was the first major manufacturer of STMs and AFMs; later startups clearly had DI in mind as they crafted their design philosophy, self-presentation, organization, and user base. Our examination of DI echoes similar analyses carried out by Digital's competitors. Obviously, I risk whiggishness in placing so much emphasis on the company that is counted the most successful of its kind. It is true that I have occasionally mentioned factors that I believe were ingredients in DI's success. In this and following sections, though, I will outline ways in which DI's advantages could have turned into handicaps. DI's ultimate success was in no way guaranteed, though the field is littered with companies that tried to prove that point.

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[11] My thanks to Chris Henke for discussions about agricultural extension agents' similar picture of a "progressive farmer" who will spread news about innovations to other farmers (Henke 2000).

The company that, early on, stood the best chance of overtaking DI was, unsurprisingly, associated with the Quate group. In 1989, two former Stanford postdocs, Sung-Il Park and Sang-Il Park (no relation) formed their own probe microscope company, Park Scientific Instruments. In many ways, Park and DI resembled each other, and this resemblance fueled competition between them.[12] Though Park and DI often did not compete directly for particular niches or individual customers, symbolically they saw themselves as vying for preeminence in the SPM community. *Differences* between them, however, highlight much about the technical culture at each. Park, for instance, started out closer to the Quate group than DI did to the Hansma group [MK1, 10/12/01; JN1, 6/28/01; BP1, 2/3/04]. Quate veterans founded Park, and other Stanford graduates funneled into the startup from the beginning, while Hansma graduates migrated to DI much later. This meant that, Quate group designs moved to Park more or less intact, while Hansma's designs were subjected to Elings' critical gaze followed by much reengineering before Digital commercialized them.

Also, the Parks, unlike Elings, had no experience running a company or commercializing instruments. Park veterans often describe this inexperience by saying the Parks styled themselves as "gentlemen scientists," i.e., they saw commercialization as a gentlemanly activity, to be performed with a sense of fair play, and with an orientation to "best science" rather than the fiscal bottom line [MK1, 10/12/01; DB3, 4/3/01; JA1, 10/15/01]. As a result, Park was supposedly less shrewd in its negotiating and marketing than the more canny Elings. There were, certainly, occasions when Park miscalculated the treachery of ordinary business.

> Virgil was a very aggressive salesman, he focused on the air products and had a good margin and did a really good job with very, very kind of vicious sales

[12] I will follow general usage in the probe microscopy community and use "Park" to refer to the company rather than to either of the founders.

> tactics. It seemed vicious at the time.... [Tactics like] “well have them run this or have them run that,” or “their specs stink here” and “make sure, keep them honest” kind of things. It was a negative sell, “this is what the competition does wrong.” And then he exploited things like his patents. “Well, look, if they do that they’ll be violating my patent” and telling customers that. Which is a no-no actually. Not supposed to do that. [Park Scientific executive]

One important mis-step centered on the availability of AFM cantilevers. The Quate group led the development of mass-produced, standardized, cheap, reliable cantilevers by taking advantage of the Palo Alto technoscientific network and adapting lithographic techniques used in the semiconductor industry. The Stanford cantilevers soon became part of the pre-commercialization gift economy and circulated to UCSB, IBM Almaden, and elsewhere [TA1, 3/14/01; MT2, 10/15/01]. In this way, silicon (later, silicon nitride) cantilevers and tips became standard equipment for most AFM groups. Yet the techniques for *making* these tips spread much more slowly than the tips themselves. Park, as the Quate group’s commercial heir, had sole access to the cantilever-production technology and thus effectively controlled the distribution of the cantilevers.

Veterans of DI and Park believe, and I think it plausible, that Park could have edged out DI had they maintained a tighter grip on the cantilevers and the means of producing them. By controlling a crucial AFM technology, they could have kept in front of Digital in AFM development; and, by selling cantilevers to their competitor, they could have made a tidy profit on every sale of a DI instrument. Out of inexperience, though, Park let the cantilevers slip out of its fingers.

> We were the only company at the time to really make cantilevers. It turns out to be very capital-intensive. Big mistake on my part, we ended up selling them to Digital Instruments. It was such an enabling technology that we probably could have beat them at AFMs had we not sold to them.... I was scared that you couldn’t restrict trade. But it turns out you can. You don’t have to sell, you can offer cantilevers exclusively to the customers that buy your microscope. It’s perfectly legal. But I didn’t seek adequate legal advice at the time, and also I was intimidated. It was just stupid. I thought we could make money selling these things, and it turns out that yeah, you can make 10%, we would do really well in that market, but the market on the AFM was 20 times more expensive. If you could block them from selling that you turned off their

> revenue stream. So we did not have a well-defined strategy in that regard and really screwed up. [MK1, 10/12/01]

There was a subtler sense, though, in which business inexperience mattered for Park. Unlike Elings, the Parks and their team had not dealt with instrument users as *customers* before. Some had *collaborated* with potential users (surface scientists, materials scientists, etc.) in graduate school; but these had been more or less equal partnerships in knowledge production. Elings' experiences with customers (and with academic scientists) led him to think of most DI clients as irrational, unpredictable, and profoundly unlike DI's microscope designers and builders. Thus, DI's instruments were designed with this image of the customer in mind. The Park team, though, built up an idea of its customer base from scratch, and, like many in that situation, they used themselves as a template.[13] Thus, Park's instruments resembled microscopes people in academic builder groups (or their collaborators) would use. Compared to the DI instrument, Park's was a little less user-friendly (in terms of amount of time to train and intelligibility of the interface), but, early on, had a more open architecture, could be tinkered with a little more easily, and *looked* more home-made – the interface, for instance, could be made to resemble an analog oscilloscope much more readily than DI's [DB3, 4/3/01]. Also, Park showed a willingness to make very small batches of specially modified microscopes ("one-offs") if they thought the science or the engineering of the modifications was interesting; DI, from the start, had scorned one-offs as a fool's game:

> We weren't in the business of kissing people's ass [by making custom modifications]. I'd waste the rest of my life. No, come on, we had good stuff. There was nothing better. We sold only standard instruments. "Oh, you want a special instrument, I would suggest that you make sure you have a couple hundred thousand dollars in your wallet and then go build your own." One guy

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[13] That is, Park saw itself both as a producer of quality instruments for research communities, and as a consumer of "interesting" research and technical innovation. This analysis resonates both with Dornfeld (1998) and Pinch and Trocco (2002), which also describe producers' use of themselves as a template in imagining consumers. My thanks to Christina Dunbar-Hester and Trevor Pinch for discussion on this point.

> said "look, I could make an AFM for $25,000." I said "Wow! You ought to go do that. The last one we made cost us a million dollars to make. I wish you luck." The development costs the same whether you're going to make 50 or you're going to make one. So we're not going to make one. Never did. Never did make any custom stuff. [VE1, 3/20/01]
>
> In terms of custom modifications, for particular customers, that was something that actually Virgil just philosophically didn't want to get into. He didn't want to be a prototype house, he didn't want to just do one-offs for people because if you do that, obviously your engineering efforts are very scattered. You're always working on this one little widget for this one little customer. Regardless of how much you can charge that one customer, you're really never going to get rich that way. Pretty much I think Virgil had a quite clear business model in his mind when he started the company. He wanted to build general purpose instruments, not one-offs. [MT1, 2/26/01]

As we will see here and in Chapter Seven, questions about who customers really were, and how to deal with customers who wanted to buy or build unusual modifications (and thus how open to make the instruments' architecture) lay at the heart of the boxwallah's dilemma, and so cropped up persistently in the commercialization of STM and AFM. DI eventually dealt with these questions by orientalizing most customers, while securing close relationships with a few, elite SPM groups; Park, on the other hand, made the distinction less sharp both between itself and its users and among various users.[14]

Youth and Exuberance at DI

When DI and Park started out, though, the differences between them were relatively slight. At first DI, like Doug Smith's Tunneling Microscope Company or Joe Demuth's CSS STM project, built instruments for people who had the skill but not the time to build one themselves, so the sharp distinction between builders and customers was more blurry. Even after the introduction of the Nanoscope II (which was not meant for people who would normally have built their own instruments, since

[14] We will examine further DI's relationship to elite groups in Chapter Seven. One way to picture them, though, is as a kind of colonial elite. On the one hand, they collaborate with DI's projects and lend DI authority, thereby enabling the sale of microscopes within their communities; yet they are troublesome for DI in that they continually threaten to lead their communities off in new, uncommercialized directions, or even to start up their own, competing regimes of authority and commercialization. See Prakash (1992) for a study of colonial elites and science.

Elings saw digital control as too sophisticated to be home-built), DI was still cognizant that its user base was early adopters who showed unusual initiative in buying an unproven commercial instrument.[15] Thus, customer support was minimal; users were expected to take their microscopes and do what they could with them.

> MT: DI at that point had almost no marketing effort. We really didn't have to sell them to people very heavily. People came to us because they wanted to do research.
> CM: How were people hearing about it?
> MT: Well, we put ads in magazines. We had no salespeople. Jerome [Wiedmann] was the closest we had to a sales guy and he never traveled. He would go to a show occasionally, usually things that were fairly close by. Or were in places that Jerome wanted to go, one or the other. But we had no field sales people. It was mail order STMs is what it was. That sort of stemmed from Virgil's earlier experience with his other companies, that a sales force ate up too much of the pie. If you developed a large field sales force then you had people who didn't know what they were talking about out there selling things that you couldn't produce, and it was just going to ruin your reputation and cost you a lot of money at the same time. At that point our marketing efforts were pretty much restricted to producing brochures and placing the occasional ad in *Physics Today*.
> CM: What were people at DI learning about how to transport and install and train people on these things?
> MT: We weren't learning anything about it because we didn't do it. Literally, we put the things in the boxes, we put them on the Fedex truck, and we sent them to the customer. Now, due to the simplicity of the design, the technical acumen of the early adopters, there's a number of things that you can attribute it to, we didn't need to install them. The fact is that you could literally get this thing up and running – the record was a user at NIST who went from having the thing in boxes on the floor to having atoms on graphite in an hour and fifteen minutes. So we didn't install them. [MT1, 2/26/01]

Even so, DI did a brisk business:

> CM: How many of the toy microscopes did you sell before it made that turn to something useful?
> VE: We started selling in the summer of our first year, sold a million dollars of them in the first year, and kept half a million profit. We had put $50,000 into the business, so not bad to get that back in the first year.... I'm telling you, we ran an ad "An STM for 25 grand and atomic resolution." I could watch *Physics Today* get mailed across the country from the phone calls we got. We got phone calls as this thing went across.... I think in our first month we recouped the 50 grand. So after the first month of selling we were never in the

[15] People throughout the SPM community use the language of "early" and "late" adopters routinely. Ironically enough, the diffusion of this sociological term into other fields is unclear, but it may come from Rogers (1983).

> red again. It was a fun business. But after that the job was really to make it useful. [VE1, 3/20/01]

It's instructive for a moment to look at some of Digital's early ads, since they give a taste of the startup's home-grown culture. DI advertised primarily in publications with a multi-field readership – *Science, Nature, Physics Today, American Laboratory,* and occasionally the *Journal of Vacuum Science & Technology*. Many of the early ads are breathless ("nothing compares to the performance of a NanoScope II, *nothing!* The proof is in images like these which have been obtained only with the NanoScope II") and self-confident. One well-remembered campaign, for instance, showed a Nanoscope sitting on a pile of copies of *Science,* on the covers of which are STM and AFM images made (presumably) with a DI instrument, with the tag line "When You Need To Do Science" or "We Have Science Covered" (see Figure 7-1).[16]

We can see these ads as an unpolished yet highly effective expression of the laboratory culture of DI and the Hansma group. The advertisements emphasize many of these group's guiding ideals: for example, a sense of playfulness, as well as the ability to image "found" objects, with little or no sample preparation (see Figure 7-2):

> "Atoms on the Surface of Table Salt (NaCl)
> fresh from the shaker at Digital Instruments
> A New Era in Microscopy
> Now both insulating and conducting samples can be imaged quickly and reliably with atomic resolution. This scan of table salt is an example: one of our employees did it out of curiousity [*sic*]"[17]

Also, many ads were meant to enroll new disciplines (and to display credibility to ones already enrolled) – "The NanoScope II is constantly being made more powerful and easier to use, and it is opening new doors in Physics, Chemistry, Geology, Biology, Semiconductors, Optics and Material Science."

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[16] For instance, *Journal of Vacuum Science and Technology – A*, 11.3, p. A7 or *Physics Today*, April 1991, p. 21. Most of these early ads were written by Jerome Wiedmann, one of Elings' former master's students – who, in keeping with the culture of tacit knowledge at DI, had little or no formal training in marketing though he was responsible for all of DI's publicity.
[17] From *Physics Today*, September 1990, p. 100.

Figure 7-1: "We Have Science Covered." From *Journal of Vacuum Science and Technology A*, 11.3, p. A7.

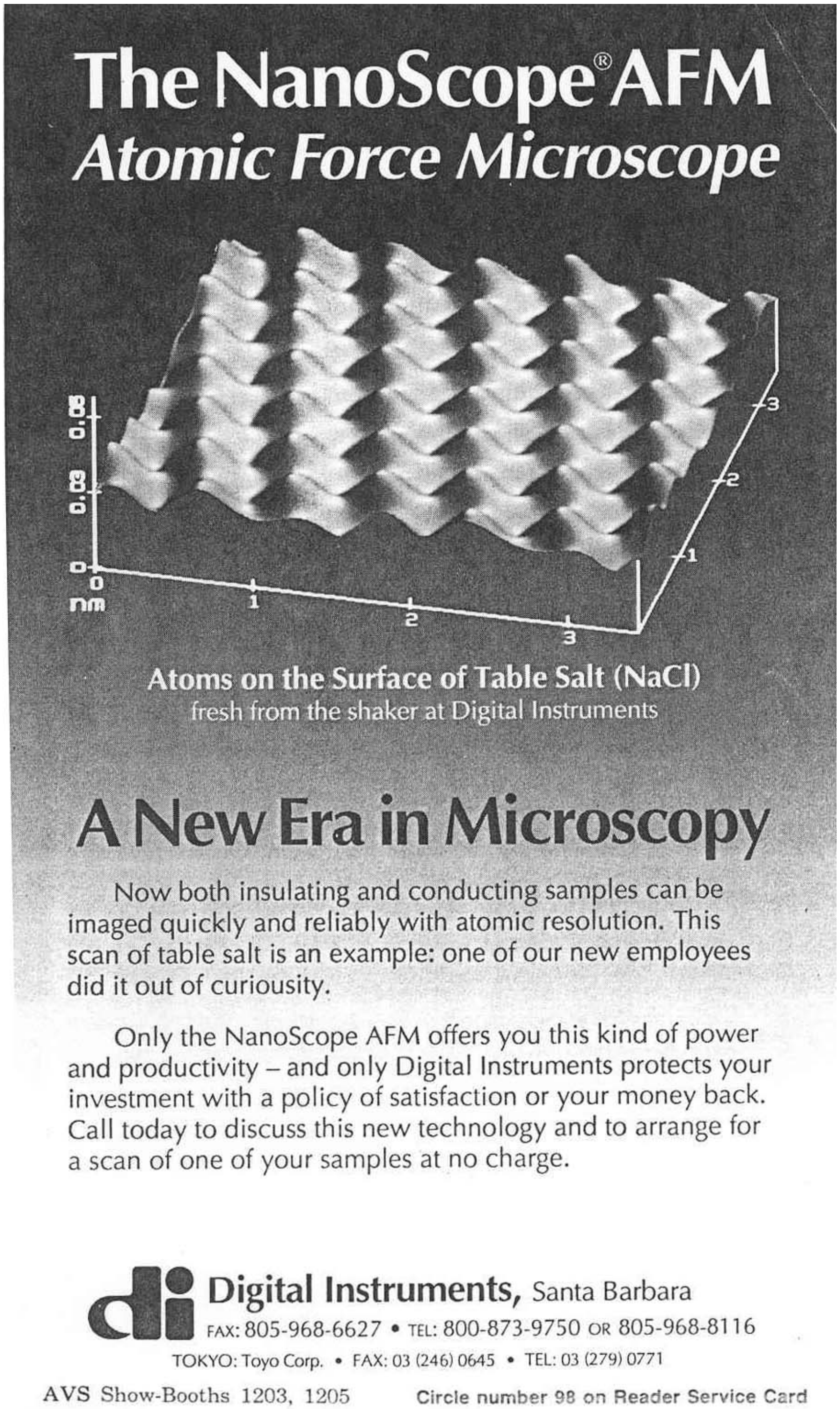

Figure 7-2: Straight from the shaker. An early advertisement for the Digital Instruments Nanoscope AFM, showing some similarities between DI and the Hansma group. From *Physics Today*, September 1990, p. 100.

Many ads featured images referencing articles in other issues of the journals in which they appear. The images in the ads, though, were often made by DI employees using samples provided by the authors of those articles, rather than the authors themselves. In one ad this process is actually described (see Figure 7-3):

> This month's image is an atomic resolution scan of a single crystal of Bi-Ca-Sr-Cu-O superconductor. The atoms form a rectangular lattice which is 45 degrees with respect to a larger structure, reported by Kirk, et al, which has a period of 27 angstroms. The image was taken at room temperature in air. The sample was provided by D. Mitzi and A. Kapitulnik of Stanford University. The image was made, as are all of the Images of the Month, at Digital Instruments on the Nanoscope II. This image was made by Kevin Kjoller."[18]

Sometimes, samples were sent to DI by researchers wanting to know whether it was worth buying a Nanoscope; many ads suggest potential customers "call today to discuss this new technology and to arrange a scan of your sample at no charge." Other samples were sent in by researchers who had bought a Nanoscope, but could not get it to work on the materials in which they were interested. These samples wound up in the hands of Kevin Kjoller or Matt Thompson or DI's other proficient runners, who generated images that would then be sent back to the researchers, along with some explanation of what the image meant and how to operate the instrument to produce high resolution with that sample [MT1, 2/26/01; KK1, 3/23/01].

At the same time these images would go into DI's next advertisement. Since advertisements are not peer-reviewed and have a shorter turnaround than articles, many of these images appeared in journals like *Science* and *Nature* long before DI's customers could publish any results on the very same samples. Researchers to whom this happened tell the story with a tone of irritated amusement; DI clearly wounded

[18] From *Physics Today*, November 1988, p. 149. "Images of the Month" was a DI ad campaign in 1988-9. Kevin Kjoller is an especially proficient AFMer at DI. This particular ad is more descriptive than most, partly, I think, to get in a dig at the competition – Mitzi and Kapitulnik were Quate collaborators ("Kirk et al" is a Quate group paper). Any instrument DI sold at Stanford (often achieved through large subsidies and incentives) was taken as a major victory over Park; selling to Quate's coauthors would have been even sweeter.

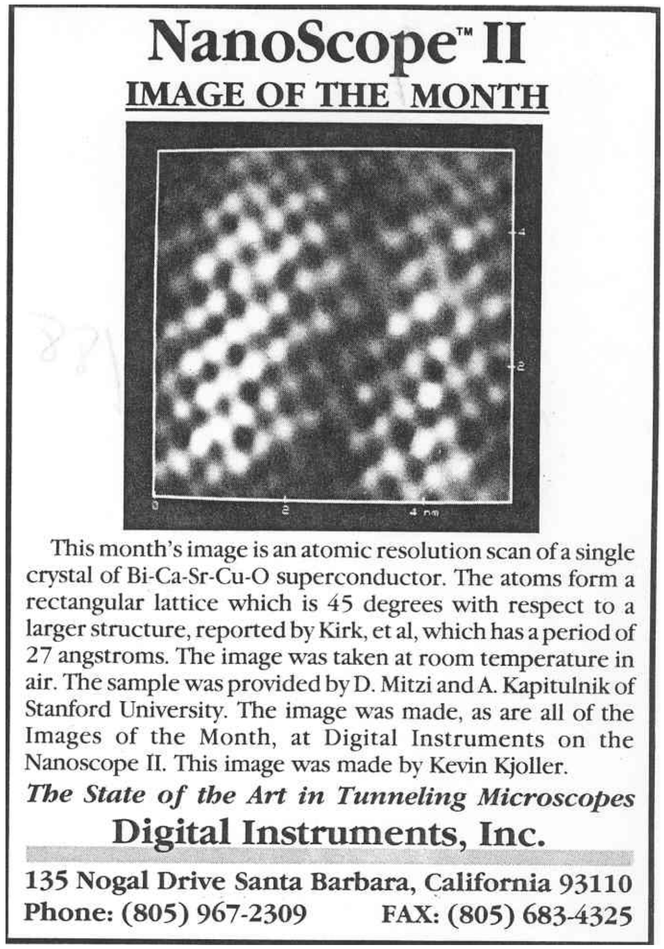

Figure 7-3: DI Image of the Month. From *Physics Today*, November 1988, p. 149.

their pride by publishing images of "their" samples first. Being scooped by an ad, though, does not seem to have mattered as much in the moral economy of academic science as being scooped by a peer-reviewed article. Such ads do seem to matter, though, in the cycles of credit and credibility operating at the intersection of the moral, political, and commercial economies of science (Latour and Woolgar 1986, ch. 5). As Latour and Woolgar show, cycles of credit are crucial to instrument *users* – new instruments help generate articles, which foster credibility, which secures grants, which allow the purchase of new instruments. The same cycle, though, operates from the instrument *maker's* vantage as well; instruments sold mean articles generated, which means increased credibility among the colleagues of the authors of those articles, who may then buy more instruments. Applying pressure at *any* point in the cycle can be beneficial; thus, DI and the other SPM manufacturers put their efforts not just into selling instruments, but into generating articles and securing credibility by any means possible. DI did so by scooping customers with ads, forming special relationships with pioneer groups (especially Hansma's), coauthoring papers, and bringing in "six month" visitors and postdocs just like the UCSB team.

Changes and Proliferation

This is, again, the boxwallah's dilemma. As much as Elings or other instrument makers saw their customers as profoundly unlike themselves, the cycle of credit often calls on instrument manufacturers to adopt the trappings of their customers – to publish articles, to do new research, to establish credibility. In Act One of STM and AFM commercialization, this was done in an *ad hoc*, laissez-faire way, since instrument-savvy "early adopters" themselves resembled the instrument makers (by building and modifying microscopes, using them in avant-garde ways, and spreading word about the technique to their communities [MT1, 2/26/01; VE1, 3/20/01; DC1, 9/5/01; AG1, 6/25/01]).

As the ’90s began, though, instrument-makers’ methods for generating credibility became more formalized. With an expanding customer base, new users could no longer be assumed to be “early adopters” or have “technical acumen.” Manufacturers (especially DI) began developing new applications themselves, selling these applications to potential users through marketing, and training new users once they bought the microscope.[19] Also, recall that the explosion of new users was fed in part by the gold rush surrounding air STM of DNA on graphite. The debates ensuing from this gold rush also expedited the end of DI’s honeymoon; the collapse of air STM as a viable technique in 1990-2 called DI’s main product into question. Thus, as air STM ran into problems, DI distanced itself from the technique. In doing so, though, the company needed new products to offer as substitutes. To meet this need, DI formed partnerships with researchers to develop STM variants to open new markets; Bruce Schardt, for instance, an electrochemist at Purdue, came to Santa Barbara and developed an electrochemical version of the Nanoscope (a collaboration mentioned in ads). DI also accelerated its appropriation of the Hansma group’s personnel and patents (especially AFM).

The transition away from air STM, however, was neither rapid nor inexorable. As we’ve seen, Elings was highly suspicious of many STM innovations, particularly those pioneered outside DI by academic researchers. When Drake and Massie first began turning the Hansma AFM into a commercial instrument, Elings scoffed at the idea that AFMs might supplant STMs as DI’s core business [JM3, 10/18/01].[20] So, DI did not switch to AFM and electrochemical STM overnight, but instead designed its instrument so that its capabilities could be widened or shifted to encompass the new techniques. That is, AFM and EC-STM were envisioned not as new *instruments*, but

[19] Lecuyer (1999) contains interesting material on application notes in the semiconductor industry.
[20] Elings’ dismissal of AFM is, today, an oft-repeated joke among Hansma group and DI veterans.

as new *modes* – add-ons to the basic Nanoscope II to increase its power and flexibility. A 1992 ad for an electrochemical microscope, for instance, says "The NanoScope ECSPM comes in both AFM and STM versions and is an option that can be added to the NanoScope at any time."[21] The first ad for a DI AFM headlined "The NanoScope II Scanning Tunneling Microscope, *Now with an Atomic Force Microscope Option.*"[22] Importantly, in moving smoothly into AFM and electrochemistry, DI reinterpreted the new crop of probe microscopes not as distinct instruments, but as "bells and whistles" to package into the Nanoscope architecture.

The final straw in pushing DI to give up its *ad hoc* approach to marketing and applications was the appearance of a new wave of STM and AFM manufacturers. These companies were all smaller than DI, but their appearance brought many changes in the SPM marketplace. Most of these newcomers differed from DI in their design philosophy and market focus, and they found different ways to balance the horns of the boxwallah's dilemma – some by integrating themselves fully with their customers, some by offering high-end research *and* instrumentation to small market niches, and others by trying to turn AFM into a tool the "masses." Their presence gradually pressured DI to change its organization, its marketing, its innovation process, and even its ownership. A quick tour of the newcomers should illustrate how these companies tried to carve up (but also expand) the field of probe microscopy.

Angstrom Technology/Molecular Imaging

The same West Coast academic research milieu that spawned DI and Park also incubated two of those companies' most prominent competitors. In Phoenix, a former Hansma collaborator, Stuart Lindsay (a physicist turned biophysicist), was methodically turning his lab at Arizona State into a prominent builder group. As such,

[21] From *Journal of Vacuum Science and Technology A*, 10.1, xxiv.
[22] From *Physics Today*, November 1989, 126. Italics in original.

his team generated many of the same ingredients that fueled commercialization in Santa Barbara and Palo Alto – innovative solutions to problems of microscope design, widespread contacts in the STM/AFM community, graduating students with expertise in building microscopes, etc. As at other places, commercialization in Phoenix was preceded by a kind of pre-commercial, gift economy phase; Lindsay's group, for instance, had its own home-built control software that they offered for a nominal fee to other researchers.

Eventually, two local entrepreneurs, the McCormick brothers, offered to sell the Lindsay microscope commercially, and to set up the same kind of know-how-for-microscopes trade that DI forged with Hansma [SL1, 1/6/03; JA1, 10/15/01]. Today their company, Angstrom Technologies, is largely forgotten, but their products did contain some important innovations which eventually found their way into other companies' microscopes. Indeed, their products may have been too innovative. The underlying electronics were based closely on an ever-evolving and extremely sophisticated design used at ASU; by not limiting this sophistication and continual evolution, the McCormicks wound up with an unreliable and user-unfriendly product.

> Angstrom just never could build things well. Uwe Knipping over here was the totally untrained, totally unqualified but technical genius technician in my colleague's lab who'd built these digital systems, just built up and up and up, so by the time Angstrom got to production he had a box that contained 12 transputer chips.... *Unbelievably* sophisticated. A transputer, for example, took care of the electrochemistry, another transputer did video graphics, another one to the servo and what have you. Unbelievably sophisticated. Of course it didn't work. I mean it would work for so long and then in the middle of a critical experiment – crash.... It was hopeless, because the machines were *so* unreliable. Uwe would never stop developing, and so they never defined something for production. So the machines in a sense were *amazing*, and there isn't a machine like it now in many ways, but they were just hopelessly unreliable. So the day came when I said to Larry McCormick "sorry Larry, sorry Darryl, you guys are fun, but I need reliable machines" and I bought a Digital Instruments machine. [SL1, 1/6/03]

After Angstrom folded, Lindsay waited a couple years and then re-entered the market on his own. Seeding the new company, Molecular Imaging, with his postdocs and students, and forming a close but evolving relationship between startup and academic lab (with the startup designed to become more independent in time), he embarked on something resembling the Quate/Park model. Starting in 1993, Lindsay saw little chance to beat DI in the general-purpose STM/AFM market, but he saw particular areas (especially electrochemistry and biophysics) where DI was weak.

> We'd been using DI machines, the controllers, which were just fine, very solid, very simple, but they worked. We didn't like the *microscopes* because they didn't have any environmental control, you couldn't do chemistry in them easily and so on. The microscopes were built, really, as a toy – "look, we can do this thing in air, and, oh, well we'll put one of Paul's rather finicky liquid cells on *if* we can." But they'd missed the point of scanning probe microscopy, which is looking at *in situ* processes. I used to talk to Virgil about this at great length, because I wanted to convert DI. Virgil was a smart businessman, saw the market in semiconductors, and he just wasn't about to get swept along about all this airy-fairy stuff about biology and chemistry. So DI never did produce a good chemical microscope. So we made ever more sophisticated chemical microscopes, and we made a couple of machines that we gave to colleagues or gave them plans or whatever. At the time I had a wonderful postdoc, Tianwei Jing, who just was magic with his hands.... So, I said to Tianwei, "well, look, people want our microscope, instead of giving it away, why don't you build them and I'll start a company." [SL1, 1/6/03]

For Lindsay and other biophysicists, DI's highly rational design philosophy forced its customers into a kind of Weberian iron cage of bureaucracy (Weber 1992).[23] That is, for DI and for many users, the Nanoscope seemed like an optimal instrument; but its design was less well-suited for subdisciplines such as electrochemistry – those communities being, in DI's view, too small to justify radical design changes. Lindsay and others worked around these "irrationalities of rationality" by home-building elaborate add-ons to the DI Nanoscope. Molecular Imaging commercialized this

[23] In particular, Weber's observations on the transition from traditional putting-out of textiles to a highly rationalized, capitalistic production of goods is reminiscent of the transition from academic builder groups to microscope manufacturing. We will examine this topic more in Chapter Eight, but it is not too much of a stretch to think of DI as "McDonaldizing" (Ritzer 1996) instrumentation for its customers; in large part this has been successful, though has created noticeable frictions both between the company and certain kinds of users, and even within DI, where some designers have feared routinization of their own work.

practice by forming a partnership with Digital to sell specially modified electrochemical microscopes run by the DI controller.

Thus, Lindsay was trying to convince other biophysicists and electrochemists to *buy* his workarounds rather than to *build* their own. To make this a plausible option, he ran a two-track program: a leading academic research group securing credibility and pioneering new applications, linked to a manufacturer providing off-the-shelf versions of the instruments used to build the academic group's reputation. At the same time, this two-track solution placed MI in a precarious position with respect to DI. MI's existence highlighted the limits of DI's rationalizing program, and demonstrated that building rather than buying could sometimes be a better option. Yet the partnership with MI yielded sales in markets DI had written off as esoteric; if DI were to compete directly for those markets, it would have to gain credibility in those disciplines through the time-tested means of hiring researchers, writing articles, and developing applications.

Quanscan/Topometrix

The other major West Coast academic spin-off emerged from the Baldeschwieler group, and, like Angstrom/MI, it took two tries to get going. Paul West, a former Baldeschwieler postdoc, long nurtured the idea of commercializing the Caltech STM. Indeed, this sense of long planning and determination to found a startup gave his efforts a distinctive cast. Where Park cultivated "gentleman science" and DI encouraged quasi-anarchic skill-building, West's company, Quanscan, adopted the trappings of modern business: venture capital, MBAs, slick advertising. Eventually these became common for SPM companies, but when Quanscan started, applying the disciplined knowledge of management to selling STMs and AFMs was unusual. Initially, it was something of an ill fit. To keep Quanscan afloat, West spent most of his time seeking venture capital money and SBIR (Small Business Innovation

Research) grants, selling the company as "innovators in nanotechnology" and cooking long-term plans for various applications of SPM technology, rather than in marketing usable microscopes [PW2, 3/30/01].

In 1988, though, "nanotechnology" was a disreputably futuristic word. West's funders, eager for a real product line with present-day profits, pushed him to market his prototype microscopes. It is possible Quanscan could have run with these microscopes; after all, it had many of the same things going for it as Park and DI. The *differences* with DI and Park, though, are interesting and crucial. Under other circumstances, Quanscan's use of modern business methods might have proven an advantage over the *ad hoc* management of its competitors. Certainly, Quanscan's advent motivated the adoption of a more businesslike approach at DI. As it happened, though, the extra layers of hierarchy and bureaucracy needed for such a management style were Quanscan's undoing. Eventually, infighting among the venture capitalists and MBAs with a stake in Quanscan halted the flow of money and put the company under the gavel. Ironically, Elings could have bought what was left of Quanscan; both DI and Park sent representatives to the auction. West, however, rounded up investors to buy the company back, changed the name to Topometrix, hired a new CEO, and started over. This time, West seemed to hit his stride. Like Park and DI, Topometrix aimed for the general research market by producing instruments with built-in flexibility for a wide range of intended users. The company's large, aggressive marketing department flooded journals like *Physics Today* and *American Laboratory* with glossy ads and, before long, generated enough sales to rank alongside Park. Topometrix answered the boxwallah's dilemma by selling more flexible software, which users could have access to and even modify. Thus, customers acted as out-sourced loci of innovation for the company – any modifications they made could be appropriated and turned into products. In Chapter Eight, we will explore further how the competition between DI's closed and Topo's architectures played out in the mid-'90s.

McAllister

One last West Coast manufacturer exemplifies the ambiguities of commercial microscope making. As we've seen, Berkeley was an early runner in academic STM, but quickly dropped out of the race. In some ways, this abortive effort was replicated in the race to commercialization. Stanford, UCSB, Caltech, and ASU all had leading SPM groups, and all were associated with significant startup manufacturers. By 1990 Berkeley itself had little in the way of STM, but at Lawrence Berkeley National Laboratory, Miquel Salmeron was building STMs and AFMs as part of traditional surface science work. Two (interrelated) commercial STMs spun off from Salmeron's group, both hovering in the strange liminal zone between gift economy and full commercialization. Bob McAllister, a former technician for the Berkeley surface chemist Gabor Somorjai, had set up a small instrument-building service for local research groups, making unique tools for labs that lacked the time, personnel, interest, and/or skill to build equipment themselves. Eventually, McAllister started collaborating with an instrument maker in Detroit, Adam Kollin, on commercial EELS spectrometers – McAllister supplying the hardware, and Kollin's company (RHK Tech) the electronics.[24]

> STM came along and everybody could see it was hot stuff so both Bob McAllister and Adam Kollin were interested in getting into the STM business, because they rightly figured that a lot of people were going to want one. So again they worked out a deal where Bob would build the mechanical part, the actual vacuum part, because the idea was the vacuum STM was going to be what people wanted. Adam Kollin and RHK would make the electronics. This was generally based on what we [Salmeron's group] had built here at Berkeley.... So we sort of provided them with all the details and ... we sent out lots of copies of the plans and circuit diagrams of what we built to whoever

[24] "EELS" is electron energy loss spectroscopy. A beam of electrons is directed at a sample, and the energy spectrum of the scattered electrons is used to determine the composition and bonding of the material.

> asked, or also to people who came and stayed for six months or a year to see what we were doing and took away the plans with them when they left. [FO1, 10/24/01]

Eventually, McAllister moved to Idaho, effectively ending his collaboration with Lawrence Berkeley National Lab and RHK. From Idaho, though, McAllister continued building custom STMs for researchers. The rhetoric of his corner of the industry is illuminating. His ads (see Figure 7-4) claim extreme *value* ("As your budget gets tight, every dollar needs to stretch further and further. Sometimes painfully far. Call us. We'll help you stretch your dollar") combined with extreme *specificity* ("Discover the Freedom of Custom Instrumentation ... when "off-the-shelf" just won't do the job. At McAllister Technical Services we guarantee you'll get <u>precisely</u> what you want. We are known for our collaborative approach.").[25]

That is, McAllister emphasizes that he offers instruments that allow users a high degree of uniquely subtle, sophisticated control, backed by his long-term commitment to customers – "McAllister's STM electronics, software, and hardware work in concert so you can control <u>all functions</u> from your computer keyboard."[26] Indeed, these issues have become indistinguishable in SPM manufacturing. Customers who want their instruments to have very specific functions are seen as wanting greater control over the operation of the instrument; yet in providing control and specificity, manufacturers feel obliged to expand their commitment to service and training. In this, McAllister lies at one end of an artisanal spectrum among SPM manufacturers (with DI at the other end).

McAllister and DI approach this aspect of the boxwallah's dilemma from opposite directions. DI separates the innovation culture of its engineers from the user culture of its customers, then re-engages customers by allying with elite users.

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[25] From *Journal of Vacuum Science and Technology A*, 9.4, xvi and *Physics Today*, August 1990, 165. Ellipses and underlining in original.
[26] From *Journal of Vacuum Science and Technology A*, 8.1, 729. Underlining in original.

Figure 7-4: Ad for McAllister Technical Services. From *Physics Today*, May 1990, p. 114.

McAllister finds would-be elite experimentalists who want a microscope with very specific and sophisticated capabilities, then becomes (temporarily) a part of his customers' lab groups – making his expertise part of their internal division of labor, rather than (as with DI) an external resource. For both DI and McAllister, though, there is a continual slippage between instrument maker and instrument user. McAllister's customers generally come from disciplines with strong instrument-building traditions. His customers know very specific things they want to do with the microscopes, and why they cannot do them with other commercial instruments. Thus, he must closely collaborate with them to design their instruments, and after building the microscope he continues serving his customers as a kind of out-sourced technician. DI, on the other hand, sees many customers (particularly in biology and in industry) as coming from disciplines with very little instrument-building tradition. Thus, DI believes it must coauthor papers with scientists from those disciplines, and even employ its own researchers in those areas, in order to create the credibility for other practitioners to hear about, and want to buy, a Nanoscope. For both DI and McAllister (and all other SPM companies), the lines between user, builder, and manufacturer are continually renegotiated; yet such lines are necessary for customers to see that they could not (or would not want to) themselves build what is offered them.[27]

RHK Tech

Once McAllister left for Idaho, the partnership between Salmeron's group and RHK grew both closer and more formal. RHK began marketing more complete STM systems, and drafted Salmeron's group into the manufacturing process. In particular, the software for Salmeron's STMs (written by his technician, Frank Ogletree), became the kernel of RHK's control software; in return, LBNL became RHK's beta testers,

[27] This is similarly to the argument in Bowker (1994, 106ff.) about Schlumberger's need to align itself with its customers while also differentiating itself from them.

receiving free instruments to try out and debug. As Ogletree continued writing software to accommodate ever more subtle and sophisticated control algorithms, his code continued to flow into RHK's systems [JG4, 6/29/01; FO1, 10/24/01]. This, in turn, bent RHK's marketing toward research groups like Salmeron's, i.e. groups planning to use their (bought or built) STMs in subtle, unorthodox ways (often for surface science). Like McAllister, RHK emphasizes control, service, specificity, and collaboration between manufacturer and user. The company's ads target customers who ordinarily build their own instruments, and whose instrument-building can be appropriated:

> Achieving results with STM once meant building your own equipment. Now, the AtomScan SysTeM 1000 lets you spend your time on research, not system development.... Individual components are also available to suit your specific requirements.... Providing you with the finest equipment available is only the beginning of the commitment.[28]

Again, this is a more or less profitable, but often difficult position to maintain. As we will see in Chapter Eight, RHK relies on the building prowess of its customers to feed it innovations. This means, though, that RHK has trouble expanding its product line to full STM systems, since its whole marketing philosophy is based on customers who want to build a significant part of their STMs; and it has to work very hard to demonstrate technical prowess and a full understanding of the research its users want to do, else many customers will return to building rather than buying.

Burleigh

The West Coast startups were spun off specifically to sell probe microscopes. RHK, McAllister, and a few smaller firms, though, have tried to *add* SPMs to their existing product lines. One is Burleigh, a piezoelectric components maker in Rochester, New York. Burleigh's piezo products received a boost in the mid '80s, as researchers began buying them for home-built STMs. With time, Burleigh started

[28] From *Review of Scientific Instruments*, 59.6, 1.

packaging more of the piezoelectric guts of an STM together, until they decided to sell complete systems. Like most companies in DI's shadow, Burleigh wanted to find a niche in which to differentiate itself [DF1, 5/29/01]. Eventually, through local academic connections, they turned their focus to the college teaching lab market. Doing so could have meant comfortable profits – there are, after all, many many teaching labs; but designing for this market pulled Burleigh away from its core design philosophy of building extremely high-end tools for very small research markets. Plus, at $15,000, an educational STM was beyond the reach of most demonstration labs. So Burleigh added claims that their educational STM also had research capabilities.

> Imagine what a powerful learning experience it would be if your students could actually "see" atoms. Well, now they can. The new Burleigh Instructional STM was developed specifically for use in college labs and classrooms, and it can dramatically enhance the way your students learn about atomic and molecular level phenomena.... The Burleigh Instructional STM is designed for education, but it could, in fact, function nicely as a basic research instrument. After all, Burleigh manufactures sophisticated probe microscopes for research. We offer complete systems as well as components that enable you to build your own STM.[29]

As this quote shows, Burleigh tried to market simultaneously to demonstration labs, to experimentalists wanting to use an air STM in their research, and to builders of high-end microscopes. This represented, I think, an ill-timed or ill-conceived attempt to short-circuit the boxwallah's dilemma. Interestingly, Burleigh's strategy was guided less by the history of instrumentation than by the history of personal computing; they saw their cheap, simple microscopes as the counterparts of the PCs that flooded the market and edged out big, powerful mainframes in the '80s. This stripped down, mass-marketed SPM, though, had trouble competing with the more capable and flexible Nanoscope. The Burleigh instrument did not have all the "bells and whistles" of the DI Nanoscope – it had fewer modes, fewer add-ons, could operate

[29] From *Physics Today*, April 1992, p. 20.

in fewer environments, etc. For Burleigh's people, the bells and whistles were largely extraneous – the vast majority of probe microscope research could be done, they thought, with the modes included in their instrument. As it turned out, though, for academic customers trying to secure tenure or stay ahead of their fields, the bells and whistles of the Nanoscope looked like a welcome safety net in case the direction of probe microscopy changed and some modes became hotter than others.

Today, the Personal SPM seems quixotic, but, after all, optical microscopes have (after several centuries) reached the stage where both high-tech, specialized and low-tech, casual versions of the same product exist side-by-side. Someday, SPMs may reach that point, but in the early '90s, credit and credibility still mattered in ways that they do not for mass-marketed, low-tech instruments. For an optical microscope in a high school lab, cycles of credit still operate, but in a more constricted manner; if students do not see what they're "supposed" to see, they might fail a test, or the teacher might lose face, or the manufacturer might lose an account, but it is unlikely that the textbook will be rewritten based on what the students *did* see. For SPM in the early '90s, matters were not quite as certain; there was not yet general agreement about what a probe microscope could do, or which of its capabilities were most relevant to users. More importantly, there *was* general agreement about the lack of closure. That is, most SPMers believed startling new applications were around the corner, though they did not know which modes and functions would enable those applications. Having the "bells and whistles" was a way to be ready for those applications. Burleigh's Personal SPM, in stripping down functionality, left itself open to attacks (mostly from DI) that it stranded users without the proper tools.

Omicron

Finally, Omicron is a German firm that made UHV spectrometers for surface science before commercializing the STM design of Hans Neddermeyer's group at

Bochum [TB1, 11/19/01]. For Omicron, the Neddermeyer STM represented a way to expand its range of products for the UHV/surface science market. Thus, Omicron has concentrated largely on surface scientists and has adopted a strategy of offering integrated systems (rather than stand-alone microscopes) of STMs in concert with LEED, Auger, XPS, UHV chambers, and sample prep. Omicron products are large, surrounded by other instruments, and engineered to an extraordinarily high standard. Surface scientists are the very people who, in the ’80s, built their own STMs, so the Omicron instrument is a “builder’s microscope,” and Omicron engineers are seen as having the same investment of technical identity in their microscopes that surfaces scientists do in home-built instruments.

> To my surprise actually, a company, Omicron in Germany, started coming out with commercial machines – I couldn’t believe it at first – that could seemingly do what my machine could do. Then it became clear they could do it and they could do it better. So there was a weaning from my machine, which was painful to use, just a pain in the ass, it broke every time you looked at it, and I could never train anyone to use it.... They continue to make some really excellent instruments. They are the worst company in my experience to deal with, difficult guys. They treat their machines as perfect and if anything goes wrong it must be because you, the idiot customer, has mistreated their machine or something. Anyone I’ve met who uses their machines will tell you the same thing, they’re very difficult – but they make a very good machine. So we’ve moved away from using the homemade machine, which is now totally retired, to commercial machines, and just now, for the first time in years, I’m building a new machine that’ll do things that you can’t buy. [BW2, 5/22/01]

Omicron represents a solution to the boxwallah’s dilemma particularly suited to surface science. As the quote shows, Omicron customers are continually close to departing and building their own instruments; yet by a mixture of quality engineering, possessiveness, confidence, and arrogance, Omicron levels the playing field of credibility and convinces them to remain customers. In this context, it is notable that Omicron has had less success in selling AFMs and NSOMs, users of which come from different instrument-building traditions than STMers and surface scientists.

Conclusion

With Omicron, we conclude our tour of the major SPM manufacturers of the ’90s. There were others, most short-lived, some based in large companies like JEOL (a Japanese electron microscopy firm), others spun off from academic labs. Their stories fill out the spectrum of approaches to the boxwallah’s dilemma that I’ve sketched here. Most previous commercialization stories in S&TS have ended at this point, with the advent of manufacturers and the successful transformation of a homebuilt tool into a commercially available device. That is, indeed, an important turning point in the existence of an instrument-oriented community; SPMers recognize the advent of DI and its rivals as crucial, and their memories are especially vivid of the first time they used a commercial instrument, or saw SPM vendors at the STM conferences, or had to make a choice between building or buying a microscope.

Neither end of the commercialization narrative is sharply delineated, however. Even before there were dedicated SPM manufacturers, commercialization was, in some sense, always already a fact of life for STMers. No one makes from scratch *all* of the components of even a home-built instrument. Indeed, early on, one major topic of STMers’ gossip was where to *buy* components like piezos, tips, op amps, and substrates. These materials, though, were manufactured for purposes other than probe microscopy; the work of the instrument builder was to locate and combine these parts to make a microscope – a paradigmatic process of bricolage, especially when the parts were safety razors or pawn shop diamonds. Today, the rationalizing mission of SPM companies has swept aside much of this bricolage. Manufacturers substitute their own products and experience for ever larger portions of the practice of instrument-building research groups. The various manufacturers and their customers vie over just how much substitution is permissible, but the lower bar is constantly creeping up.

Instrument builders increasingly find it hard to justify *not* buying dedicated SPM software, electronics modules, environmental chambers, cantilevers, etc.

Even with well-established SPM firms in place, though, the boxwallah's dilemma refuses to disappear completely. That is, not everyone yet accepts the manufacturers' rationalizing mission. On the one hand, there are still researchers in relevant disciplines who have not yet taken up STM and AFM. Indeed, since the manufacturers talk about a day when AFMs will be as common as centrifuges and optical microscopes, they are likely to be dissatisfied with the diffusion of their instruments for some time to come. For these recalcitrant users, companies like DI try to lead by precept and example; they send vendors to the relevant conferences, they set up applications labs so their employees can author papers in the relevant disciplines, and they circulate applications notes that trumpet the work of early adopters in those fields. With respect to these potential users, the challenge is to make SPM seem mundane, yet still potentially exciting.

On the other hand, there are still those who then try to bypass the instrument manufacturers by building their own microscope. In general, manufacturers try to pull these people back in by building microscopes that they will want to buy. Usually, this necessitates taking on much of the technical identity of experimentalists in these disciplines and displaying the skill and tacit knowledge of the home-builder (Haring 2002). Still, since many in the SPM community, including manufacturers, believe (and hope) that closure has not quite been reached on these microscopes, some experimenters continue to build their instruments for idiosyncratic or esoteric applications; yet it is exactly these instruments which could be the next hot thing. The boxwallah's dilemma demands that manufacturers insinuate themselves very closely to this elite and be ready to appropriate and rationalize their innovations. As we've seen here, and will explore further in Chapter Eight, doing so means returning to the very practices of chaotic bricolage that manufacturers claim to overcome, a strategy that presents opportunities and anxieties for manufacturers, users, and elite builder groups.

The tug and pull that commercializers of instruments feel between elite builders and customers is only partially described in previous work on trading zones. The contexts described by Galison in *Image and Logic*, for instance, might be called first-order trading zones, where the boundaries of the trading zone, though evolving, show no tendency to expand and morph without end. Participants come from a handful of backgrounds, and they are able to maintain long-term interactions through the social glue of an interlanguage or pidgin (which slowly turns into a full-fledged creole). Situations where the interlanguage is wrapped around an artifact or technique which has activist sponsors that would like to diffuse it into a number of communities, though, might be called second-order trading zones; these exhibit the phenomena seen in Kaiser (forthcoming-a), as well as in this chapter. In these situations, trading zones are much more fluid; and the glue that holds them together is often not an interlanguage but a particular social group – the boxwallah, commercializer, or mediator. Indeed, commercializers often have to learn pidgin forms of a variety of different technical languages, as did colonialist boxwallahs. Commercializers and boxwallahs *inhabit* the second-order trading zone, without turning it into a stable proto-discipline as in first-order trading zones. Indeed, commercializers *need* the fluidity of their situation, and they construct complex ecologies, filled with different types of boxwallahs and auxiliaries (engineers, application scientists, elite customers, etc.) in order to make the trading zone a habitable and profitable place. The boxwallah's dilemma is to make sure that the trading zone is a place other groups will continue to need to come to – by making sure the goods and services sold within it are desirable, and that the trading zone is the only place they can be found. As we have seen, there are many different ways to construct a local culture within a trading zone; in the next chapter, we will see that the durability of boxwallah cultures can bring both triumphs and disruptions for their members and trading partners.

Chapter Eight

Probe Microscopy in the Era of Commericalization

The advent of commercial microscopes irrevocably changed what it meant to be a probe microscopist and what counted as good probe microscopy. Probe microscopy today is in a transition that has rarely been documented in science studies. Numerous historians of science have examined the role of home-built instruments in the formation of technical subcultures; anthropologists and historians have illustrated the process of bringing an instrument to the market; and sociologists of science have often noted the importance of already-commercialized technologies in laboratory culture (Buchwald 1994; Galison and Assmus 1989; Rabinow 1996; Rasmussen 1998; Woolgar 1988). Few, though, have pointed to the curious problems and opportunities presented to a community in which commercialization is *becoming* widespread, and instrument-building is *becoming* less common.

One way to think about commercialization is as a species of Weberian routinization (Weber 1992, 66ff.). We saw in Chapters Two, Four, and Six that the early cultures of invention, development, and commercialization were very much in the charismatic mode (Weber 1947). "Builders" like Binnig, Quate, Hansma, Elings, and their associates saw themselves forging something novel and surprising; and the cultivation of charisma, eccentricity, and personal knowledge was central to their success. The dilemma of commercialization, though, is that it survives on charisma even as it aims at routinization; though the manufacturers need builder culture, their products make that culture less tenable. In this chapter I follow routinization right into the design of the instruments, where we find an interesting pairing between Weber's iron cage and Bruno Latour's black box (Latour 1987, 2). For Latour, a black box is a scientific fact materialized, a piece of knowledge so secure that it is no longer questioned or even examined. A black-boxed instrument incorporates such universally accepted, unquestioned facts – which means that the instrument can be standardized, and its inner workings can be hidden away. In Latour's view, users can insert the passage through such an instrument unproblematically into the career of their samples without continually having to renegotiate the tricky mutual calibration of sample, knowledge, and instrument needed with a more "transparent" or "translucent" box. Note that commercialization of an instrument fits nicely into this schema – two of an instrument manufacturers' central aims are usually to standardize the tool, and to streamline or hide away the local, idiosyncratic bricolage of an instrument built by postdocs or graduate students. As we will see, the black box is an actors' category, and one over which SPMers endlessly argue. A box's opacity is seen as defining how routinized its use is, and how idiosyncratically customers can shape its design. That is, a thoroughly black-boxed instrument may enclose users in an iron cage of rationality, where they are molded to operate the microscope in ways that conform to the manufacturer's vision of an "average user;" while a more translucent box is seen as offering a more porous but problematic kind of cage.

Finally, another way to think about the era of commercialization is in terms of the experience of different kinds of SPMers and the role anxiety this era induces. As we've repeatedly seen, builders, runners, and other kinds of probe microscopists staked a considerable part of their identity on the success of the instruments with which they were associated.[1] For some, the cultivation of technical identity through instrument-*building* was particularly intense. Commercialization has brought forth new kinds of identities associated with probe microscopy; but it has also brought troubling questions about why some kinds of builder identities should persist.[2] The

[1] My analysis of technical identity owes much to Haring (2002).
[2] My analysis of the strategies of SPMers in crafting post-commercialization identities resonates with Mulkay, et al. (1975).

story of probe microscopy in the late ’90s is primarily one of tracking how various SPMers continually construct and reconstruct technical identities that accord with the realities of commercialization.

DI’s Response: Modes and Microscopes

By the mid-’90s, researchers could buy STMs and AFMs from any of a daunting array of manufacturers. Moreover, these companies had carved up the SPM field into market niches: Molecular Imaging concentrating on electrochemists and biophysicists, Omicron and RHK on physicists and surface scientists, Burleigh going after casual and educational users, and Digital Instruments, Park, and Topometrix focusing on more general users. This burgeoning market segmentation was in part a move by smaller firms to co-exist with DI; in turn, success, and the presence of the new firms and their specialized modes, led DI to become more formally organized, and to develop more diverse product lines. We can track this process through the ’90s by looking at what capabilities DI packaged into its research instruments. For a decade and more, the Nanoscope has accrued a toolkit of modes and capabilities. Today, probe microscopes are commonly referred to as the “Swiss Army knife” of scientific instruments; commercialization, and the academic-commercial nexus that turns dissertations into products, has greatly accelerated the proliferation of tools that make up this knife. To understand the social contours within the jungle of microscope users and manufacturers, it is necessary to understand the meanings attached to the proliferation of modes; and few sites have been more crucial in forging those meanings than the culture of innovation at Digital Instruments.[3]

[3] I use “culture of innovation” here in much the way that Diane Vaughan (1996) uses “culture of production.” That is, Vaughan traces how macro-ethics of production played out at the microlevel of teleconferences and launch assessments. Here, I trace how various ethics of innovation at DI – skepticism, tinkering, generative anarchy, etc. – played out in the creation of new modes and instruments.

Tapping for Gold

As we saw in Chapter Six, openness to new modes and instrumental variations was integral to Virgil Elings' initial decision to make a *digital* instrument incorporating flexible electronics and software. Thus, DI employees were continually encouraged to tinker with the instrument and produce new modes; yet the company also cultivated an ethic of skepticism, in which new modes – whether invented internally or externally – had to fight hard to survive. A major part of DI's innovation culture (and the most shocking difference between DI and UCSB for former Hansma group members) was a tradition of disagreement (and even disparagement), harsh debate, strong (if mixed) emotions, and occasional lapses into infighting followed by lapses back into camaraderie [JM3, 10/18/01; JW1, 10/18/01]. Designers often had to work behind the backs of their managers to create new products, and many of DI's most successful technologies started out amidst intense managerial opposition.

One source of inspiration for DI's new modes and applications to package into the Nanoscope were a series of temporary collaborations with outside researchers. Sometimes, as with the importation of the AFM from the Hansma group, these collaborations resulted in more or less straightforward technology transfer. In these cases, there might be some negotiation and redesign at either end of the pipeline, but all parties basically interpreted the transferred technology in the same way. Other collaborations resulted in something more like a "technological dialogue" (Pacey 1990), where interpretations varied widely, and where interactions between DI and other groups acted more to inspire than to import new technologies. One such innovation – the so-called "tapping mode" – has been crucial in forming DI's success and shaping the SPM field; more generally, the story of tapping mode is illustrative of how SPMers view mode proliferation and how they structure their research around innovation.

The roots of tapping mode can be found in the first AFM paper in 1985, where Binnig, Quate, and Gerber described two ways of operating a force microscope – contact mode, where the tip scraped the surface, and non-contact, where the tip hovered above the sample (Binnig, et al. 1986a). For several years, only contact mode was viable, but some IBM AFMers – those associated with the Quate group – vigorously pursued non-contact through the late ’80s and early ’90s. Recall that, unlike corporate surface scientists, Quate’s IBMers focused their research much closer to IBM’s product lines than to basic science – investigating things like wafer preparation or new data storage techniques. For these applications, neither STM nor contact AFM was particularly desirable – most technology-oriented surfaces (e.g. silicon wafers) have a non-conducting layer at the surface, meaning no STM; and most have valuable and fragile surfaces, making the scraping action of contact AFM unwanted. Quate came to STM from a non-destructive testing tradition, and probe microscopy’s potential in this area impelled his research, so it was natural for his students and collaborators at IBM to turn to non-contact AFM as a way to look at technologically relevant, non-conducting surfaces without damaging them.

This had several ramifications for the development of AFM. For instance, these researchers –Dan Rugar, Kumar Wickramasinghe, Gary McClelland, Tom Albrecht, Bruce Terris, Gerhard Meyer, Nabil Amer, and others – were among the first to use optical detection methods for the AFM. In non-contact AFM, the interaction between tip and substrate is so small and subtle that it is usually necessary to integrate that interaction over time until it becomes large enough to detect; one way to do this is to wiggle the cantilever over the same spot on the sample and see how the sample modifies the cantilever’s resonant frequency or driving amplitude. This kind of algorithm is particularly difficult with an STM-based AFM detection scheme (as described in Chapters Three and Five); but an optical detection system can draw on a long tradition in optics and electrical engineering (with which Quate's students were familiar) dealing with resonant frequencies, beats, bandwidths, and quality factors.

So devising new optical detection schemes for non-contact AFM, and the mathematical machinery to go with them, became a cottage industry at IBM in the early '90s. Making these schemes work reliably on real-world samples, though, was a painful and elusive process. DI and others viewed non-contact AFM as a pipe dream – mathematically elegant, but time-consuming and ugly in practice. In the early '90s, though, DI become more interested in industrial applications and "real-world" samples (and in developing ties with IBM), so non-contact became a more pressing issue. Interestingly, Elings directed some of DI's most junior, least experienced people to look into it – expecting, probably, not to find anything, but ready to capitalize on discoveries made through his employees naïve style of experimentation:

> There was getting to be more and more noise in the community about non-contact, so Virgil figured we'd give it a try. He figured it wasn't really going to come to anything, but he also figured the same thing about AFM. When we first came up with the first AFM he was like "ah, nobody's going to buy these things...." So here we are a couple of years later and we're hearing noise about non-contact. Virgil decides, all right, we'll build a non-contact AFM just so we can convince ourselves it doesn't work. So, we have a guy named Colby Bowles who was a junior at UCSB at the time, he's working there for the summer. Colby's kind of your fairly typical Santa Barbara skateboard rat, he'd come in with bandages on his head and his elbows and all this kind of stuff. Really smart guy but perhaps not a paragon of patience. So in some ways kind of an odd choice to get non-contact working, because non-contact is really a tough thing to make work because it's a very unstable feedback technique, so to get it to work at all does require a lot of patience. Anyway, the hardware's been developed, Colby's really just primarily working on getting the technique to work. Well, I figure Colby lasted about two days, and he said "screw this," and turned the amplitude way up, and that was one of the things that was absolutely defined about the IBM patent on non-contact was you had to keep the amplitude of oscillation of the tip very very low.... Well, Colby does this and lo and behold we start getting some beautiful images. It took us a couple weeks to figure out we weren't doing non-contact, we were doing something completely new.... That became tapping mode. So that was patented and Colby became a hero. Again, somewhat serendipitous that we discovered this. It was not everybody sitting around saying "what can we do to improve non-contact?" [MT1, 2/26/01]

Thus was "tapping mode" (sometimes jokingly called "hammer mode") born. DI's recipe of developing black-boxed user-friendly software and electronics, and popularizing new applications of its modes in various fields, has made tapping mode virtually ubiquitous in all kinds of industrial and academic labs. One key to making tapping mode indispensable to DI's customers has been the addition of "phase imaging" – the discovery that the detection scheme can feed back on changes in the phase of oscillation of the cantilever as well as changes in the resonant frequency and amplitude. The invention of phase imaging shows, again, how DI culture makes designers fight for their innovations, but capitalizes on them once the fight is over.

> My big contribution for the AC techniques was the Extender, which was basically the electronics for measuring phase and frequency. There's another milestone in the company, it opened up a whole new area of things that you could see and experiments and measurements you could make.... Even James [Massie] had tried to make a circuit to look at phase and it wasn't very conclusive. That made Virgil a little skeptical but I didn't think he'd done it right so I decided to give it a shot. So basically it was me and Todd [Day], working at night, because it didn't really have a lot of popularity at the time.... There were other things I was working on at the time that were deemed as more important and this was just basically some improvement thing that I wanted to try because it interested me. So, working mostly after hours and on weekends and stuff we'd just come in and work on this thing and it wasn't long before we got this thing to work and it was actually pretty cool.... We decided that we needed to make it available on the Multimode and so Dennis Colby sort of kludged, well kludge is too polite a word, but basically they got this thing to work on a Multimode and we started selling it as the Extender. [DB2, 3/23/01]

Phase imaging has proven important in a number of areas, but particularly magnetic force microscopy (MFM), now a large niche for DI and a major addition to the Multimode. Again, MFM started with Quate veterans at IBM, where understanding the microscale magnetic properties of materials is important to data storage. As with other modes, Elings was initially skeptical, but when talk about MFM in the SPM community became loud enough he brought in a new staff scientist, Ken Babcock, to get DI into the game:

> I remember the first day I started at DI – this'll give you some sense of the place – I walked in, I met the person at the front desk and said "is there a place I can sit, where's Virgil?" He was gone so they stuck me off in a cube, I had to walk around and find people, "can I have a computer?..." The next day, Virgil walked by the cube and stuck his head over and looked down at me and said "so, magnetic force microscopy." Before I could look up he was gone. That was my beginning of communication with Virgil and I realized that most of the things he said you should sit and ponder. It was like a zen master, okay, my mission is make magnetic force microscopy into something, but it's up to you to figure out how to do that. That was wonderful. I felt my way along and it was just the funnest time I've ever had in a professional sense. That was really great – it went from there and within a few weeks, getting some interesting data, connecting with customers and developing this into a real area. I had a lot of ownership for that and I had a lot of motivation and DI at the time was a very easy place to get people to help you out and get things done, so that was really a magical time for me. [KB1, 3/23/01]

From these microscopists' stories about innovation culture at Digital, we can piece together the typical career of a new microscope, mode, or application at DI. First, the company usually found new modes such as the MFM in one of four places: (A) academic research groups, usually a "builder" group like Hansma's; (B) national or industrial labs like IBM or NIST; (C) one of DI's competitors; or (D) DI itself. Then, in most cases, personnel were exchanged: either a postdoc or scientist from the group that developed the new technique (or another group in the same subdiscipline) came to DI, or someone from DI visited the developing group to learn more [DA1, 3/23/01; SM2, 3/21/01]. Finally, various internal negotiations (between applications scientists, technicians, engineers, marketing people, and programmers) massaged the technique in preparation for commercialization. In this way, the Nanoscope has accrued MFM, "lift mode" and phase imaging, lateral force microscopy, imaging in fluid, scanning capacitance microscopy, scanning spreading resistance microscopy, electrostatic force microscopy, tunneling AFM, force pulling, nanoindentation, and other capabilities. Other, more esoteric techniques have also developed along these lines, even if they are not ordinarily part of the Nanoscope package: scanning near-field optical microscopy [BD1, 1/2/01], scanning thermal microscopy [AM1, 3/9/01; KW1, 2/23/01], ballistic electron emission microscopy [BK1, 10/23/01], scanning electrochemical microscopy, etc.[4] Indeed, there are now so many different kinds of probe microscopes that the SPM community faces the problem of forging naming conventions for them all (Friedbacher and Fuchs 1999).

Open and Closed Architectures

Beyond naming, mode proliferation raises an array of questions for both manufacturers and users. In particular, companies must negotiate which variations to follow up, which innovative research groups to ally with, and when to stake the firm's credibility on one mode or another. One recurring commercial SPM design issue – the question of "open" versus "closed" instrument architecture – nicely illustrates this process of negotiation. An open architecture is one in which the manufacturer designs the black box to be more "translucent" – electronics schematics and operating software are available to the user and both hardware and software can be tinkered with by the customer rather than just the manufacturers' engineers (Jordan and Lynch 1992; 1993). A closed architecture is more like the traditional Latourian black-box – its workings are enclosed and secret, and customers are discouraged from modifying them or even thinking too carefully about how they work. This does not prevent idiosyncratic local practices from springing up around the instrument, but it channels the nature and meaning of those practices in particular ways.

Recall from Chapter Six the paradox common to many instrument makers (the "boxwallah's dilemma"). The manufacturer must cast itself as being culturally distinct from its customers in order to allow them to justify buying rather than

[4] As explained in Chapter One, these microscopes differ primarily in the method of exchanging information between the tip and the sample; secondarily, they differ in the shape and material of the probe itself, as well as in their best resolution and the types of samples for which they are most useful. In NSOM, the probe is usually a small fiberoptic waveguide, or sometimes a cantilever with a very small aperture, that allows the instrument to measure evanescent light waves near the sample; SThM is similar to an AFM, with a special cantilever for measuring thermal effects; BEEM is a modified STM that feedsback on electrons that are tunneling out of buried metal or semiconductors layers, rather than the surface layer; and SECM is similar to electrochemical STM, but with the tip much further from the surface.

building; yet the manufacturers must cast itself as culturally similar to its customers to allow them to believe the company's promises. Starting with the introduction of the Nanoscope II in 1988, and even more so after the DNA controversies of 1990-2 (reviewed in Chapter Six), Digital Instruments made a very closed architecture central to its solution to the boxwallah's dilemma. Indeed, Digital is famous for hiding the workings of its microscopes from its customers – by, for instance, filing off the serial numbers of electronics components so that users cannot figure out what exact circuit is being use. Some users and rivals attribute this behavior to DI's paranoia that someone else might copy their products. DI people, though, dissolve the architecture issue by pointing out that (A) replication through reverse engineering is too difficult to be worth a competitor's time; and (B) "open" and "closed" are relative values, and Digital's instruments are open enough to meet customers' needs.

> CM: Were you catching flak over having a closed architecture?
> VE: Why?!
> CM: I talk to some people and they say "yeah, DI's closed and we wish we could tinker with it more."
> VE: There was nothing you could do with [an open architecture]. I'm sorry, okay, we give you the source code then we're not going to answer your questions, okay.... You can't understand that shit. We're not going to answer your questions so it's a joke. Topo[metrix] would say "well, we're open." All right, open one of their instruments, call them up and see what you get. The answer was you don't get shit. You're dealing with people who want to take your money because that's business. They'll say whatever.... Our point was we don't lie ever and we're going to do things in a way that's beneficial to the customer. It's not beneficial if I say "oh by the way here's a program and you could do something with it," because we know you can't. *We* can't even do anything with it. Are you kidding? If we changed one line of this code it all blows up. [VE1, 3/20/01]

Saltiness aside, this is an interesting quote. It describes open architecture as an illusion and a marketing gimmick – only disreputable companies would try to convince customers that they would have any luck tinkering with an instrument as complex as a commercial SPM. Moreover, this complexity is daunting even to the manufacturer – all the modes and capabilities crammed into a Nanoscope, and the layers of its evolving hardware and software, make the architecture too interrelated for any customer to deal with [KB1, 3/23/01].

Indeed, the chaos of DI's culture of invention was one of the major arguments within the company for not having an open architecture; openness would present an enormous management problem in that the code is so littered with little innovations done here and there, with very little documentation, that making the black box transparent is nearly impossible. Also, the tricks and shortcuts embedded in this code could present a credibility problem if they were presented to users.[5] The Nanoscope code takes a very noisy signal and smoothes it heavily to present to the user [JG3, 6/21/02; PM1, 10/18/01]. DI people take great pride in this code, but other builders often complain that the level of smoothing is "illegitimate," that the tricks used are "cheating." In some ways, a Nanoscope's signals resemble sausages and laws – users might not want to see them being made. For DI, there's no reason why ordinary users *would* want to see what happens to the signal; but for some probe microscopists, an open architecture (like food labeling and transparent government) protects the consumer by allowing them to see how knowledge is made.

Thus, the open versus closed question reflects differing visions of the user (Woolgar 1991). For DI, customers have plenty to do figuring out new ways to *use* the instrument, while Digital assumes responsibility for modifying and innovating the instrumentation. Customers are not exactly passive – they can influence design indirectly in a number of ways – but they should be well-behaved. The decision to make a closed architecture also stemmed from DI's own vision of itself (early on) as a company of smart, inventive people designing microscopes, rather than a

[5] I'm indebted to Ann Johnson for thinking of the programmers who write the Nanoscope's code as the modern equivalent of the early modern "invisible technician" (Shapin 1989). Programmers make the instrument reliable and user-friendly, yet their invisibility to customers is necessary to inspire confidence in its data.

bureaucratized organization with departments dedicated to handling customers rather than building instruments.

> It was part of the business decision that Virgil made. We *can* make an open architecture system, but an open architecture requires support. If we do that then we need a software engineer basically just for support. That was at a time when we had a *total* of three software engineers. So they said "this is not cost effective, this is not what we really want to do." It was really part of the same decision that we're not going to build one-offs for people. We want a general purpose, multi-application microscope that people can use easily. Giving people access to all aspects of the software, source code, schematics, just wasn't part of that model. [MT1, 2/26/01]

Obviously, though, with an instrument that has so recently been commercialized, and which is being used in disciplines with a tradition of instrument-building, there has been disagreement with DI's vision of the user.

> The relationship with the STM community was certainly good. Well it sort of broke down into two groups, actually. The relationship with the part of the community that always thought you had to build your own equipment was always somewhat strained, partially because of the decision early on with the digital control system not to provide source code. It was kind of a black box. So that strained the relationships with that side of the community. On the other side of the community was the people who just wanted a microscope that worked, weren't really interested in the details of building a scanning tunneling microscope, that wasn't what they were after. What they were after was to be able to use a scanning tunneling microscope to learn something about what they were really interested in. With those people we had very good relationships because we provided an STM that you could get running in an hour and a half. [MT1, 2/26/01]

Several DI competitors have developed open architectures specifically to market to certain kinds of researchers. At Park, for instance, building customized and/or customizable instruments accorded well with the self-image of the "gentleman scientist" absorbed more with interesting technical problems than with market share.

> Park had some fantastic technologies that we developed, our reliability wasn't the greatest but we were always known as a technical innovator as a company. DI didn't do many specials, Park did. But with their reliability, and their software was very clean, so they sold a lot. Because, just doing what it did, their instrument was very easy to demonstrate. But in terms of who had the largest field of view, who could scan the fastest, who had the most linear scans, all that sort of thing, Park was consistently ahead in being able to demonstrate those things. But it always turned out to be harder to use and it was more for the Ph.D. kind of person.... So we would give customers the source code, we

> would give them the electronics schematics. Digital Instruments was very closed, it was called a black box. They actually had a very arrogant way about them, you could have any color you wanted so as long as it's black. We would be much more open about it and got some sales from doing that. We did better in the universities than DI did, but they did much better with the corporations who have the money, and so therefore grew faster. [MK1, 10/12/01]

At Topometrix, open architecture was a core marketing position meant to draw in customers dissatisfied with DI's black box.

> Topometrix certainly got started with the open architecture, I would say before everybody else. And why? One of the reasons to do it there was that we were the underdog. We had some nice products, but we certainly weren't known in the industry. So how do you get people to want to try your product? You look at where a competitor is weak or at least is not doing something you could do. At the time Digital Instruments tried to keep everything very proprietary and not allow people to have open access. So we said, "well, what do we have to lose? We have nothing to lose. Let's just make our electronics so that it's open for people and make the software so it's open for people.... So yes we got sales, we certainly got some people who were angry at competitors because they wouldn't allow them to do those things, but once again it's pretty much the researchers who want to play with everything, and that's like 1% or 2% of the market, or maybe 0.1% or something. But if you have no sales that's a pretty nice way to start. [PW2, 3/30/01]

The architecture debate is simultaneously about the nature of the user, the proper relationship between user and manufacturer, and the locus of innovation.[6] Proponents of open architectures see them as letting users advance the technology more quickly; and a quick manufacturer can enroll innovative users in the development of products.

> But if you look today we actually have customers doing things that are really exciting and getting other customers excited that we would have no way to spend enough money to do.... That's pretty much how a lot of this stuff gets born. So the open architecture certainly helps advance new areas. I would say DI has had very closed software, but their microscope itself is open enough that people could try a lot of new experiments. So that advanced the hardware side. But at Topometrix and at Park the software was much more open, so people could advance the hardware and software together, and a lot of the new advancements will be done that way. [GA1, 3/12/01]

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[6] The open architecture debate in probe microscopy bears some resemblance to the open source debate in computing; indeed, many of the people I interviewed were quick to bring up those resemblances as a way to structure their narratives. For some analysis of open source cultures in computing, see Nissenbaum (2004) and Lee and Cole (2003).

The crux of the open/closed architecture debate is this – for every microscope manufacturer, microscope users are potential customers, collaborators, and competitors. Customers *might* use the product in an innovative way that the company could repackage and market; but many such avant-garde researchers always seem on the edge of building their own microscopes or even starting their own SPM companies. The trick for established manufacturers, then, is to corral users as collaborators, not competitors. A more open architecture shapes the manufacture/user relationship as one in which practically every customer is a collaborator of some sort. The open architecture is seen as allowing customers to modify and innovate more of the instrument than a closed architecture; thus, early on, a Park *customer* could tinker with their AFM in ways that only the *engineers* at DI could with a Nanoscope [BP1, 2/3/04]. Thus, an open architecture lets companies out-source R&D for new products.[7] Open architectures are less standardized, since customers modify them to their needs, causing headaches for both manufacturer and user, but surveillance of users' changes also creates efficient channels for reimporting innovations.

> To be honest all we do if you buy a system and you want open architecture, you'll get the software, you get a way to compile it, you get a manual about it, and you can talk to our programmers about it, but we won't help you do your specific code. Just impossible. The minute you take whatever we have and start modifying it, there's no way for us to ever reconcile how to fix ours relative to yours. Now, there are times though when customers do exciting things and they'll say "would you carry this back to your production software?" If it's definable then we will do that. We will literally take that capability. We'll say "well that's pretty interesting." Usually you can't use their code because they've done it in some way that you can't carry into production code. But as long as they can explain what they did, and they've already demonstrated it so you know the proof of concept's good, then you can just recode it in your own software. That part has really been very good for us. [GA1, 3/12/01]

Today, the only major manufacturer to specialize in open architecture systems is RHK; not surprisingly, RHK sees its customer base as coming from surface science

[7] This is similar to the story told in Lindsay (2003).

and other fields with strong instrument-building traditions. Indeed, RHK started out by, and still gets by on, marketing *pieces* of a complete UHV STM system, allowing customers to build the rest of the instrument themselves, to their own specifications and for their own targeted applications. This puts RHK in a delicate position. On the one hand, they want to sell larger, more expensive systems that package together all the pieces of a UHV STM. On the other hand, they want to continue targeting a subdiscipline with an instrument-building tradition, and to capitalize on customers' participation in that tradition to appropriate their innovations. Over the years, RHK's system has accrued sedimented layers of functions and capabilities through its extended dialogue with its customers; often, as with most SPM companies, one or two exceptional users contribute the bulk of these new modes.

> Someone would come to us and say "geez, I'd like to modulate the bias because I want to do a dI/dV instead of just an I-V curves." So we'd figure out how to put an input into that line, filter or isolate it appropriately so it wouldn't induce more noise. Eventually we just kept evolving more and more inputs on the back panel, and ways of bringing signals in and out. So we're constantly adding little nuances into the software to do these very specific kinds of research.... The other half of this is Miquel [Salmeron]'s initial relationship in working with us on the control. He had a staff scientist there, Frank Ogletree, who was an extremely knowledgeable scientist with electrical design and software skills. He's written our software and continues to write our software. That relationship has been really key to the whole company.... Part of the agreement with Frank is he writes all of their specific codes for their applications and that all gets incorporated into the commercial product we sell. So we end up with a beta test site, and a scientist running our software who really understands all the nuances of the technique, which gets a product to market much faster than our competition. [JG4, 6/29/01]

Such exceptional users both offer innovations that can be repackaged and marketed, as well as signal other potential customers that the company's systems are not so standardized and inflexible that they will be useless for avant-garde users who might otherwise build their own.

For DI (and other closed architecture manufacturers), managing users is a more layered and mediated process. For instance, DI has a large applications department that handles most ongoing relationships with customers. "Applications" covers both the people who do setup and training, look at problem samples, and run in-house microscopes to help in the development process, as well as the scientists brought in as post-docs or permanent employees to research the use of the company's microscopes in new areas. Note how this replicates the division of labor of the Hansma lab; indeed, Paul Hansma's graduate students often become DI engineers, and both Paul and Helen Hansma have had postdocs move to DI as applications scientists.

Applications people fill many functions for DI. They role-play users, by putting the instruments through all the idiosyncratic paces that users might. They help with marketing, by translating DI's rhetoric for specific disciplinary audiences and being missionaries for the technique:

> I ended up traveling a lot and being a diplomat. I think there was some marketing involved. I was involved with the marketing department in terms of making the brochures coherent to biologists. Putting together talks and going out to different biology departments all over the world. Giving seminars, sometimes at conferences, just showing what we were talking about, the convincing stuff, where the field had been, where it is now, where it's headed, and who was doing the work and what the new improvements in hardware were, etc., etc. They [DI] saw a future in biology, they wanted to bring in someone that was a biologist that also knew AFM and was young and foolish enough to travel a hundred days a year. But I also got some research done. [MA1, 10/12/01]

Applications scientists do in-house research, simultaneously forging new techniques (and hence new markets) and increasing the company's credibility as a source for the generation of new knowledge. This makes them *colleagues* in a strong sense of the people to whom DI is selling its microscopes – they go to the same conferences, look at the same samples, publish in the same journals (Manalis, et al. 1995; Babcock and Hopkins 1999; Magonov and Heaton 2000; Moller, et al. 1999). This makes it easy for them to interact with customers (and non-customers) in the same field, to survey how customers are using products, to locate new innovations or to bring back to DI's designers a user's-eye view of changes that need to be made. Of course, this does not guarantee those changes, but it does give users a voice within DI (and DI a voice within various disciplines).

> MA: I would tour labs and be able to see how they were applying the instruments and they might have some questions. They would always have a list of things they wanted changed for me to take back.... "There's a bug in the software version when I try to do this filtering," or "wouldn't it be great if you could scan blah blah easily," "why can't I move the tip over to this spot if I want to, how do I do that?..."
> CM: How were those requests for customizing regarded back at DI? What would it take for a request like that to actually end up in a product?
> MA: Well you go down to the corner store, buy a six-pack, and take it to the software engineer. I found that out late when I was there that's how you get things done. But usually, if you wanted something done you do it yourself. I mean you physically go and find somebody and you do it. The software group would have a list and there were certain priorities assigned. It is a tough problem because you want to make an instrument that's of maximum use to the maximum number of people. And so you're always going to have a few people that want to do it differently and if you go changing it to make everyone happy it ends up being a mess. [MA1, 10/12/01]

One can see Digital Instruments' approach to the boxwallah's rationalizing mission in this applications scientist's use of DI's oft-repeated slogan of the instrument "of maximum use to the maximum number of people." DI builds just enough capabilities into its microscopes for the "average user" (whom applications scientists are expected both to speak for and mold). Building in more capabilities – to accommodate users who deviate from the average – would, in the view of many at DI, actually make the instrument less usable for the masses [MT1, 2/26/01; MA1, 10/12/01]. The perennial problem for all manufacturers in the age of mode proliferation is to know which modes, variations, capabilities, and functions. will be worthwhile to commercialize and market. At DI, applications scientists act as filters between the community and the company to constrain the number of variations and ameliorate resource allocation; but applications people are also interested participants in this process, and their specific interests have helped shape what a Nanoscope looks like and what it can do.

As we saw with RHK, microscope manufacturers also shepherd customers by giving special attention to particularly valuable "exceptional users." At DI, applications people locate users with special applications or modifications, with whom the company then forms partnerships. Interestingly, *enrolling* these customers can mean a slight opening of DI's black box. For instance, this researcher who was building a near-field scanning optical microscope with DI's help:

> RD: We were working back then with Digital Instruments and so we were trying to use as much of their equipment as we could. So we used their software, their electronics, and just kind of tricked some of their signal into thinking it's their own.... Just because they're good at making things easy-to-use and reliable.
> CM: But DI is sometimes kind of secretive about how their stuff works, right?
> RD: Yeah, we had an agreement with them. The deal was that they would help with stuff and in return they would get first rights and refusal on anything we developed. They were interested in making a near-field microscope, so it was a collaboration. But you're right, they don't like to give it up. But they reluctantly would send us schematics of their electronics and stuff so that we would know where to get the signals, which helped tremendously. The software, we would call them and they would make changes but they don't have an open architecture, they won't give you any of their code. The electronics, they would send us schematics on things that we asked for, usually. Sometimes we would have to go back and forth a couple times. But this is back in the days when Virgil Elings was still running the show, and it was kind of nice because once you got a rapport with Virgil, since he owned the whole thing, he could say what he wanted, it would go. So you'd just call up Virgil and chat with him for a bit and the next day there'd be packages arriving or their head coder would call you and ask you what you needed. [BD1, 1/2/01]

For exceptional users who develop exciting new *applications* (rather than *design innovations*), DI often forms a more research-oriented collaboration – groups are invited to write applications notes, which look very much like journal articles and which are distributed to DI's customers.[8] There are rebounding gains in credibility for both manufacturer and user here – the user gets a widely distributed publication (without the hassle of peer-review), while the manufacturer gets the endorsement of a group that is seen as doing cutting edge research. Publication can flow the other way as well; it was common, for instance, for very early buyers of DI microscopes (many

[8] For instance, the authors of Quist, et al. (1996), a DI app note, are from Uppsala University; the authors of Heinz, et al. (1998), from a Johns Hopkins group headed by a former Hansma postdoc; the author of Goken (1998) is at the University of Saarland; and the authors of Lutz, et al. (1997) are affiliated with Dow Chemical and EK Consulting.

of whom continued to have exceptional user relationships) to list DI personnel as authors on their first few publications (such as Manne, et al. 1991) generated with the DI device.

Users and Identity: The SPM community in the '90s

Commercialization has proven to be a dynamic, changing, evolving process – new modes, new functions, and new applications are added constantly, new markets come in view, new companies come and go. Importantly, the manufacturers' view of differentiation within the probe microscopy community has been one of the most dynamic parts of this story. How the manufacturers think about differences between themselves and their various customers, and how they think about differences among customers and non-customers, continually and profoundly shapes and reshapes the contours of the community. For the first builders and runners, this fluidity has been central to staying at the forefront of the technology; yet the ambiguities of identity induced by commercialization have also presented major challenges. Each builder or runner group has developed different strategies for dealing with post-commercialization realities.

Today, the early days of commercialization are seen as a golden age for the academic builder groups – their students had jobs to go to, they were able to barter expertise for equipment, their vision of STM and AFM was being branded, and hundreds of groups were buying instruments based (more or less) on their designs. It was a heyday for runners as well – some found work in the manufacturers, while the rest founded their own STM or AFM groups, brokered sweet deals with the startups, and headlined the introduction of probe microscopy into various disciplines. As commercialization facilitated the explosion in the number of SPMers, builders and runners both reaped the rewards. They were no longer outsiders trying to flog a new and unproven technique. Instead, they became the honored pioneers of a hot field, allowing them to take up leadership positions within the ever-growing probe microscopy community by, e.g., running STM conferences or becoming the SPM-savvy associate editors for various journals.

As commercial microscopes (especially AFMs) have become routine tools in a variety of industries and academic disciplines, though, builders and runners have faced certain existential problems. In the days when almost everyone built their own instrument, the technology changed quickly relative to the number of people in the field; as information came in about what other groups were doing, builders changed directions on the fly. The speed with which the AFM proliferated after the initial crude attempts at Stanford in 1985 is a good example – even though the early AFMs were incredibly difficult instruments to use, many groups saw what Binnig and Quate had done and quickly started building their own. Even after commercialization, the technology still changed rapidly because builder groups could funnel a backlog of innovations into their associated startups.

With time, though, the startups grew and became much larger than any single academic group; also, the designs of, and practices for using, commercial STMs and AFMs stabilized. Probe microscopy technology continued to change, but via variations on the commercial architectures – the design of the commercial instruments anchored what could be done with an SPM, and what users could expect to do with their SPMs.[9] There were debates about just how much leeway users should be given, but even the most open architecture is more standardized and structured than the home-built instruments of the previous era. Moreover, builder groups began to face a serious question – who, exactly, was the audience for their innovations? If no one could replicate what they were doing on a commercial instrument, then their only audience would be other builders – and, with the number of builders dwindling both as

[9] A kind of post-closure variation, as described in Rosen (1993).

a percentage of the SPM community and even in real numbers, playing to that audience began to entail a loss of credibility.

Builders can, of course, simply accept that times have changed and forego being builders – either by buying commercial instruments, or by leaving probe microscopy entirely. Other options are to work on less radical changes that fit within the framework of the commercial instruments, or to push outside the envelope of probe microscopy by developing related (but non-microscopy) technologies. Quate's group, for one, has done the latter, working more and more on AFM-inspired data storage technologies rather than microscopy [MT2, 10/15/01; SM1, 3/13/01]. Hansma has split the difference by developing ultrasmall cantilevers for ultrafast AFM, and by pioneering force puller technology that uses stationary cantilevers to pry apart complex molecules [PH1, 3/19/01; HG1, 11/14/01]. Many builders who continue to build see themselves now, I believe, as mediators between the SPM manufacturers and the rest of the SPM community. That is, they continue tinkering with the technology and developing new modifications and functions, while realizing that the exigencies of commercialization mean that very few groups will replicate what they have done. Rather, they work to display their innovations to both the manufacturers and their customers, hoping to create enough demand that their innovations will be commercialized.

Former runners face similar choices. Once, they could coast on their close relationships with both builder groups and manufacturers, and ride the prestige of having introduced the technique into their discipline, thereby winning acclaim from two different communities [JN1, 6/28/01; JH1, 6/10/02]. As commercialization matured, though, use of STM or AFM in their disciplines became routine; and other researchers caught up enough to advance the technology and forge "exceptional user" relationships with the manufacturers. To remain "exceptional," some runners have drifted into building microscopes (or SPM-related technologies) – with the difficulties this implies.[10] Other runners realize that their "exceptional" status is, in all likelihood, ephemeral, and that someday they will merge back into their home communities as ordinary probe microscopists. Adjusting to these changes in role, though, can be difficult. Many of the early builders and runners look back on the '80s and early '90s as a thrilling time, and ending that phase of their careers is unappetizing. So some try to ride the wave of innovation for as long as they can before becoming less "technology-driven."

The Fall and Rise of the STM Conferences

Probably the most acute symptom of these anxieties about role can be seen in the changing attitudes of builders and runners to the STM Conferences. Remember that the early conferences were about trading ideas for building STMs and AFMs, and getting newcomers up to speed on how to construct and operate the instruments. Today, very few people build their instruments, and welcoming newcomers is a job for manufacturers. Moreover, as STM and (especially) AFM have spread to more and more disciplines, it has become difficult to keep all the techniques' practitioners under a single umbrella. For many, the STM conferences have lost their reason for being:

> We'd go to this thing called the STM meeting. They were really fun for a while because they were really small.... It's a conference that has largely outlived its usefulness. It used to be there was some common theme. It used to be small enough that you could pay attention to everything that was going on – biologists, chemists, physicists, everything. There'd be three hundred people in the room and that'd be it. Once it got up to a thousand, with parallel sessions, it started being like any other conference. I suppose that you could argue that there would still be room for a small conference focused on new developments in instrumentation, but as far as I know there isn't really that sort

[10] Two probe-based techniques that were hot in 2000 were multi-probe data storage and force pullers. The former uses an array of AFM cantilevers to scan across a surface, occasionally striking the surface to leave behind a mark that represents a "bit" of data. The same array can then be used to read out data produced in this way. Force pullers work by attaching one end of a biomolecule to a cantilever and the other end to a substrate. As the cantilever is moved up, it pries open bonds within the molecule, which can be sensed as changes in the deflection of the cantilever.

> of conference. It becomes part of this STM Conference where people are just presenting results in a diverse range of fields. [JN1, 6/28/01]

Yet, for diverse reasons, a number of groups keep these conferences rolling. For manufacturers, these conferences offer an extraordinary concentration of their customers, who are often feted and schmoozed, offered announcements of new products and developments, and given glimpses (or even trials) of new technologies; thus, for users, conferences offer a chance to have their voices heard by the manufacturers and give feedback on their systems, as well as to see where the companies are going. Conferences give manufacturers a chance to attract new users, or to poach customers away from their competitors by setting up booths in the vendor exhibit; like many such meetings, though, the STM conference often has trouble getting attendees to take a look at the vendors' booths. At an STM conference I attended, not only were manufacturers giving away the usual complement of free tweezers, pens, slinkies, key chains, and candy, but the conference organizers, having noted a paucity of attendees in the vendor exhibits halfway through the meeting, resorted to giving the vendors coupons for free beer that they could distribute to anyone inspecting their wares.

Perhaps more important for the manufacturers, the STM conferences present opportunities to see and be seen. All of their would-be competitors are on display, so that their rivals' new products and innovations can be inspected. Also, in the late '90s, with the demise of some manufacturers, and spinning off of others, many former colleagues now work for rival companies. The STM Conferences and other meetings are a place where they can meet on more or less neutral ground and engage in friendly competition – indeed, the vendor exhibit is the site of much horseplay and collegiality between people in the middle ranks of the manufacturers.[11] In conference talks,

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[11] There is some literature on vendor behavior at trade shows and similar situations: Hinrichs, et al. (2004); Palmer and Forsyth (2002); Clark and Pinch (1992).

manufacturers engage in similar networking, display, and observation. Their applications scientists, for instance, give papers showing how to adapt STMs and AFMs for particular uses or samples. The company's scientists demonstrate the utility of the instrument, and the validity of knowledge produced with that instrument, in a public setting where they can interact with, and be questioned by, their colleagues. Each company's stable of "exceptional users" fulfills a similar function by presenting their work while highlighting the role played by their commercial STM or AFM (and directing potential replicators back to the manufacturer).

At the same time, those who wish to *become* exceptional users also present papers, and manufacturers' scientists and engineers circulate around to talks that seem promising as sources of new products. In this venue builders play out their role as mediators between ordinary users and the manufacturers, since both audiences are present and watching both the talks and each other. New techniques from established or up-and-coming builder groups attract large crowds; and promising techniques may become the "buzz" of the conference. At an STM conference I attended in 2001, Hansma-type ultrasmall, ultrafast cantilevers seemed to be the buzz; in other years, near-field scanning optical microscopy (NSOM) or carbon nanotube probes or force pulling were similarly hot topics. Once an application accrues this kind of momentum, it becomes difficult for even the most jaded manufacturer to ignore.

Funding agencies and professional societies, too, see benefits from prolonging the STM Conferences. In many ways, the interests of these organizations resonate with those of the manufacturers. Granting agencies find a dense concentration of their grantees at these meetings, and their representatives attend to survey progress and get feedback; they can make themselves available to people who want grants; and can observe new directions in the field. Professional societies gain memberships and dues from these meetings, while reifying the discipline itself. After all, the "STM

Conference" is the most visible symptom that there is such a thing as an "SPM community." Indeed, the American Vacuum Society (which hosts the STM conference in North America) is a society in search of a community; and for some, the STM conferences provide the kernel of a community to bring into the AVS' fold.

Finally, the STM Conferences are also propelled by elite SPMers.[12] These people plan and organize the STM conferences, play the most vocal and regulatory roles at the meetings and in the literature, their work is most widely acknowledged as exemplary by other SPMers, and their discourse focuses most exclusively on SPM. Most of them still build their own instruments, though a few are various companies' exceptional users. This group faces its own kind of identity problems in the commercialization era, and their response has been, in part, to reconstitute themselves as a "craft elite" of the SPM community. Unlike some of their colleagues, they have decided that their investment in STM or AFM is so great that they wish to tie their professional identities more to the technique and the instrument than to their home disciplines. This means they constantly tinker with their microscopes and develop new advances in the technology, rather than work on a range of samples or applications on which they can report to other physicists, chemists, biophysicists, or surface scientists. Indeed, commercialization has pushed some of these people into building extremely specialized, sophisticated, esoteric microscopes for specialized, sophisticated, esoteric applications such as single molecule vibrational spectroscopy, magnetic resonance force microscopy, or atom manipulation (Ho 1998; Eigler 1998; Ferris, et al. 1998; Viani, et al. 1999; Mamin and Rugar 2001).

The positions of these people could be extraordinarily unstable – it is never quite clear who their audience is or who might replicate what they are doing. The existence of an STM Conference, though, nicely stabilizes their identity issues. It

[12] For a classic analysis of the role of such elites, see Mulkay (1976).

presents them with an audience that can appreciate the high quality of the instruments they build and the results they achieve; it reifies a group with a history that they can be seen to have been pioneering; and it makes their research seem *advanced* rather than *esoteric*. Still, it takes a great deal of sociotechnical work both to remain in the elite, and to justify elite work. On the one hand, the craft elite meticulously builds highly idiosyncratic, often quite expensive, microscopes that little resemble what most users work with; hence, many in the elite are found at the more prosperous national and corporate labs, though academics who build up enough resources are also major players. These groups also work hardest at presenting highly rendered, artistic images, in contrast to ordinary users of commercial instruments, many of whom do not stray from their microscope's default color palette.

In organizing the STM meetings, this elite is more or less self-selecting:

> CM: How did you get to be conference chair?
> RW: I don't know. I volunteered to help out and the next thing I knew I was the chairman.... Like in any field, there's a kind of clique or something of leading people, and things get shared around. So people like Feenstra and Hamers and Wiesendanger, and other people have had their turn in various capacities.... Frankly I don't know how I hand this off to the next person, I don't know how to call a meeting. There are sort of invisible structures in place. So it's just a cliquey thing and, you know, everyone's welcome. Everyone who wants to step up and be in that clique is kind of welcome. I think you get looked over as you come to the door and it's a gradual thing. "Who's he and are we going to let him in?" But then inevitably anyone who's willing to do some work is partly already in the door, and then you might not get fully allowed in if you don't have the sort of oomph to back you up, it's a very vague thing. [BW2, 5/22/01]

We will see in the epilogue how this elite, and others tied to the STM Conferences, have drawn on nanotechnology discourse as a way to stabilize the problems of audience and identity surrounding these meetings in the commercialization era.

The Big Machines

The STM Conferences are a synecdoche for the simultaneous success and disruption of the probe microscopy community in the commercialization era. In some ways, no part of the community has been left more uncertain by widespread commercialization than the manufacturers responsible for it. Probe microscopy in the ’90s expanded rapidly in numbers of users, in places employed, and in types of disciplines and communities to whom it was made relevant. As we’ve seen, this wrought many changes in how academic groups viewed the instrument, and how they viewed themselves as builders or users. Somewhat less dramatically, though more lucratively, probe microscopes (primarily AFMs and MFMs) crept into the industrial workplace – especially quality control and reliability labs in a variety of manufacturing sectors. Today, anyone who manufactures products, the quality of which may depend on nanoscale properties (e.g. adhesives, photographic film, ball bearings, liquid crystal displays, microelectronic components, and so on), likely owns several commercial AFMs.

At the beginning of the ’90s, many probe microscope companies turned to this industrial market; the time seemed right both to build interest among potential industrial users, and to harden the commercial instruments to make them more suitable for industrial use. Symptomatic of this new focus were the first commercializations of probe microscopy activity at IBM and Bell. IBM, of course, had long led research in STM, AFM, and MFM, but the early ’90s marked the company’s first efforts to make real money off the technology. In general, the early ’90s recession forced IBM to find overlooked sources of profit. At IBM Research this meant, initially, an increased willingness to defend Big Blue’s patents. As we’ve seen in Chapter Three, Binnig’s research style made patent protection difficult, but IBM started to press its case anyway, particularly with DI and Park. This led to a surprising collaboration between the two rival start-ups; though Park and DI recognized that IBM might have a right to some royalties, they did not recognize the demand for a blanket 1% of all sales that Big Blue was demanding. Thus, they cooperated to research which exact pieces of an

STM or AFM had originally been patented by IBM; in doing so, the weakness of some of Big Blue's patents put the start-ups in a good negotiating position, even with respect to the biggest of corporate giants:

> It turned out we [Park and DI] worked together a lot on intellectual property stuff because of IBM. It was funny, Park Scientific and DI were the #1 and #2 companies, we were pretty serious competitors, but because we had this, not a common enemy, but a common problem in IBM wanting to assert their royalties, we worked together on understanding the patents and how IBM came to have all these patents. There was a lot of monkey business going on in these patents.... You have to go back to 1952, oddly enough. IBM was found guilty of antitrust. As a result they had to open their patent portfolio. They suggested that as an appeasement for this going forward. So IBM told any company that operated in their field, or any company that was likely to infringe their patent, "look, we'll license all our patents to your for 1% royalty, if you license all of yours to us." Just we're-open-you're-open. If you use our stuff you pay us 1%. And they never enforced it. But the appeasement had a 40 year life, I think. Starting in 1992, because it was 1952 to 1992, they started sending out letters saying "we're looking to collect our 1%, and you guys make STMs and AFMs. We hold the fundamental STM patent, we hold the AFM patent." Our companies had been in business now 4, 5 years, and it was going to cause a big big problem because we were going to owe back royalties, we were going to have to pay IBM this big lump sum. 1% is all bottom-line stuff, right.... But IBM was really playing hardball because they had a new internal decree that IBM's intellectual property would earn the company a billion dollars a year, and they were really pushing that, and this meant a million or two in royalties, so it wasn't chump change. In the end we had to pay them but we really battled it down to quite a bit less than it was going to be. [MK1, 10/12/01]

IBM also involved itself in the "big machines" – AFMs for industry. With its stake in both magnetic data storage and semiconductor manufacturing, IBM had an abiding interest in non-destructive, high resolution AFM for processing and reliability studies, as well as process-line quality control. Thus, a former Quate lieutenant, Kumar Wickramasinghe, was easily able to convince his managers to let him develop a large, factory-line (non-contact) instrument for IBM use. Crucially, IBM decided to develop the so-called SXM internally and also sub-contract it to DI [KW1, 2/23/01].

This came when DI was about to expand into the industrial market anyway, so the IBM offer was hard to refuse. Elings, though, was skeptical of non-contact mode and of what they perceived to be IBM's high-science approach. DI's whole design philosophy was based on developing easy-to-build, easy-to-use modes that could be made workable in the hands of the largest number of users; the company saw its customers as people with little time to understand finicky techniques like non-contact [MT1, 2/26/01; DB2, 3/23/01]. So the partnership with Big Blue sputtered, though DI used it to springboard into designing industrial instruments. Wickramasinghe, meanwhile, finished his SXM and soon IBM was batch-producing them for its own semiconductor lines. Later, with the recession in full swing and IBM management pushing researchers to contribute to the bottom line, the SXM was farmed out to Veeco (a Manhattan Project-era vacuum engineering company, one of the corporate founders of the AVS, and now a manufacturer of tools for semiconductor processing) to sell externally.

At Bell Labs, the company's slow decline following AT&T's breakup in 1984 led to a similar restructuring of SPM research around more applied topics. Joe Griffith, a former surface science STMer, used the company's new, more commercial orientation, as a starting point for designing a factory-line metrology instrument relevant to many of the same applications as the SXM.

> I had my own [STM] instrument and we worked on that for a few years. It worked out well. But times began to change, and I guess it was about 1987 Kumar Patel who is the executive director of my area got very interested in proximity x-ray lithography.... So I could see this coming. One day I showed some data to my director and he did not like it, "this is not a good thing to be working on, you should be working on x-ray lithography." And it became clear that either I did that or I better go find some other place to do my work. So I went back to my office and thought very hard about it for several months in fact and considered for a while doing x-rays, as a way to do metrology on the patterns. One thing that was becoming obvious was that they were having greater and greater difficulty measuring the dimensions of the features that they were making, and we needed to have a way to measure those. So I decided that metrology would be an interesting area for me to get into. Originally I thought about it in terms of x-rays, but .... I went back to thinking about my probe microscopy and got to playing around with the idea, maybe I could do the metrology with a probe microscope. One day I presented that idea to Kumar, and he liked it. Well he said "yeah, this is a good thing to do, go do that." So I was still in probe microscopy but now applying it a little bit more directly to the metrology. [JG3, 2/28/01]

As with Wickramasinghe's SXM, Griffith's big machine, though developed for Bell Labs' own requirements, was soon farmed out to a manufacturer to build, sell, and generate licensing revenues for the corporate lab.

> In order to make this work in my lab I had purchased a little electronic box from a company called Atomis.... As the larger manufacturers were getting more and more sophisticated tools, especially as they went over to Windows, they were locking up the source code so that people couldn't get access to it. The experimenter was forced to go and beg the company to make modifications to the code if you wanted to do something new. And I really wanted to do the code, I knew how to write code and these guys at Atomis were willing to give me access to the code. Well just after I had bought the thing from them they told me that they were being bought out by a fellow named Chuck Bryson who .... wanted to get into the probe microscope game. So as I was buying the tool from Atomis he was buying the company. We quickly struck up a relationship and hit it off pretty well, and it turned out his ambitions were a little bit bigger than just making a probe microscope tool, he really wanted to make a metrology tool for the semiconductor industry, he wanted to go all the way, and very quickly got interested in our technology and the possibility of licensing it from us. So that's how all of that got started, it was a little bit of an accident. On my end I had management who was eager to pursue this and his end he had the desire to go do it. [JG3, 2/28/01]

Several sociotechnical considerations are crucial in turning a research AFM into this kind of industrial tool. First, these "big machines" are much larger than research AFMs, partly because they need to accommodate a larger sample. In order for an industrial AFM to have a decisive commercial advantage over tools like the scanning electron microscope, it must be non-destructive. For example, in the semiconductor industry, so much money goes into putting integrated circuits onto an individual wafer that manufacturers would prefer to be able to sell, rather than destroy, all of the wafers that they test [CP1, 3/19/01]. This means that relatively little (preferably no) sample preparation must be done – i.e., the sample must remain its normal size and shape; again, in semiconductor manufacturing, a wafer is tested at several points in its fabrication, so the entire wafer must be able to be moved into and out of an industrial AFM. In research, samples can be cut down to small sizes to be placed into small microscopes, but in industry (where samples may be very costly or where every product may undergo some quality checks), samples can be quite large (in semiconductor manufacturing, for instance, wafer sizes have crept up from 6" to 8" to 12" to 16" to keep pace with Moore's Law).[13] Large samples mean important changes in design. In a research AFM, the scanning of the tip relative to the sample is done by moving the sample back and forth and keeping the tip stationary (thereby making the optics of deflection sensing less cumbersome). In a large-sample AFM, though, scanning the sample would be slow and mechanically difficult, so AFM builders had to figure out a way to keep the optics simple while scanning the tip back and forth.[14]

Also, industrial users are seen as having their own peculiar problems and expectations. Time, money, and skill mean quite different things in industry than in an academic research environment. Academic users are seen as not particularly caring if the instruments take a few weeks to learn how to use, or if they occasionally break down; graduate student labor is cheap and long-term. Industrial users are seen as wanting tools that are more reliable and can be operated by anyone, not just Ph.D.s or Ph.D.s-in-training. This last was one of the crucial weaknesses of the SXM:

> [The SXM] is just a stunning achievement. But I think part of the problem was that it was too complicated for the fab environment.... An issue they [Texas Instruments] ran into was it took a very high level operator to make the thing work, especially in the early days, because it was just a very very touchy tool. There was a period in which Herschel wouldn't let anybody else do certain measurements with it because he was the only one who could make it work. They were looking very very hard for somebody who wasn't a Ph.D. who could run the thing. And they finally found one. They finally found a guy who was able to run the thing and his record said he wasn't a Ph.D. Well it turned out that the record was wrong, because the guy had lied on his employment application. He had a Ph.D. and didn't tell them. Under normal

[13] One of the many variants of Moore's Law predicts a regular doubling of the number of transistors per wafer. This is achieved both by decreasing the size of transistors and by increasing the size of the wafer (Moore 1965; Mackenzie 1996).

[14] Recall that with an optical detection scheme, a laser bounces off the cantilever into a detector (usually a photodiode). This system was chosen over the STM-based detector partly because it has fewer moving parts. If the tip, rather than the sample, has to be scanned, though, the laser must somehow move so that it stays focused on the cantilever moving beneath it.

> circumstances that would've been grounds for firing but he was so good that instead they just moved him to another area. So that tool ran into difficulties because the fab environment is such a demanding environment. What they want in a fab is a machine where the operator pushes a button and either something happens or a number comes out. That's all they want. They don't want to know what's going on inside. With all of these tools it has been a real struggle to get up to the point where you could get it to that level of simplicity. [JG3, 2/28/01]

At DI, this need for "push-button" tools sparked a decade-long program to develop robots and software for scanning algorithms to do all of the things a human operator does with a research microscope – replace samples, replace tips, know when tips need replacing, scan the sample, find interesting features, and assess/interpret the image.

In making their AFMs fit for industrial use, manufacturers have had to adjust to customers who are seen as demanding more support and quicker service; but SPM makers with an interest in the industrial market have also had to adjust to being the *kind* of company that has the organization and ethic for taking care of such customers.

> The last three or four years I've been really focused on these automated systems. We found the customers to be extremely demanding and they also have big dollars to throw around so we put a lot of attention into figuring out how to make the systems they can use.... You have to know what the users' expectations are, how they expect to interact with the machine, the reliability they expect, the ease of service they expect, how the measurement is going to be output. Every time you don't know one of those things then you'll get a call later saying, "what do you mean this feature is missing, every tool in our fab has this feature, how could yours not have it?" We learned some of that the hard way in the early days because we evolved from a scientific instrument-only company to a scientific instrument and automation company. There was a lot of background information that we needed to learn, there was a steep learning curve. [CP1, 3/19/01]

Live and Let DI

Thus, the big machines made significant demands on DI – suddenly, the company needed a bulked up service and support section to deal with industrial customers, a more sophisticated marketing department to entice those customers, and armies of programmers and engineers to construct the robots and code needed to build the tools those customers wanted. DI's resources drained into these projects, drawing attention away from development of its research instrument. In the end, this was an enormously successful strategy, in that DI was able to attract vast numbers of industrial users and dramatically expand its sales; by the end of the ’90s, there was no question that DI was the premier AFM manufacturer. Yet neglecting the research market also opened niches for its competitors and caused friction within the company.

Though not everyone says so publicly, there is a good consensus inside and outside DI that the company “abandoned” the research market for much of the ’90s, and that that strategy significantly affected the contours of SPM commercialization.

> I get more and more inquiries from people now who are talking about controlling an older DI head with our controller.... They have no desire to upgrade their DI, because it doesn’t do what they want to do. The common phrase I hear is DI has abandoned research. I mean that’s a pretty general feeling in the industry is that they have gone commercial and they don’t care about researchers. [Executive at a DI competitor]

Here’s how an engineer/manager at DI described it:

> We had lost our reputation, people thought we didn’t serve the research market anymore. To some extent that was true. For three and a half years, I counted, we didn’t come out with one significant new research product. To me that was really putting us at risk. [KB1, 3/23/01]

Still, retaining the research market was a high priority, even if development of DI’s research instruments was not. The probe microscopy community, though, developed such that the commercial research SPM market thrived on new modes, new functionalities, and new applications. DI’s applications department pressed the company’s advantage in some fields (primarily by pioneering tapping mode for a variety of applications [MA1, 10/12/01]), but this was only temporary salvation, especially since DI’s competitors were finding ways around its tapping mode patents. Digital felt it needed to block its competitors in the research market while continuing to focus its resources on the industrial sector.

Aside from obvious sales and marketing techniques, DI used a number of interesting tactics to discourage competitors and remain at the helm of the SPM community. First, it quietly exited from its few cooperative activities with other manufacturers such as IBM and Molecular Imaging. Second, it began policing its intellectual property more fiercely. With tapping mode gaining popularity, several of its competitors sought to market similar techniques. For DI, this was obvious patent infringement – they had discovered the advantages of intermittent contact (albeit perhaps by accident) and had aggressively patented and marketed the technique. It is safe to say that intermittent contact would not have become nearly as widespread without DI's marketing campaigns; and it is probably also safe to say that the specific benefits of intermittent contact in a wide variety of applications would have lain dormant much longer without Digital's discovery of tapping mode.

The patentability of tapping mode, though, is widely contested, not only by DI's rivals but also by many builders and runners. Binnig and Quate, in their original AFM paper, had described both contact and non-contact modes, and many researchers point out that intermittent contact is therefore an obvious (and thus unpatentable) intermediate state (Binnig, et al. 1986a). Others mention that academic articles describe something very similar to tapping mode well before DI's patent (although they also admit that no one articulated the dramatic advantages of tapping until after Digital packaged it into the Nanoscope). The power of tapping, combined with ambiguity over its meaning, made it an obvious site for patent litigation; competitors saw tapping mode as an easy steal, while DI saw patent protection as an easy way to preserve its share of the research market without diverting resources from development of industrial AFMs.

Of the companies that developed their own intermittent contact modes, only Topometrix inspires real moral outrage from Digital veterans.

> Topometrix knew who they were. They knew they weren't Digital Instruments with its hugely superior technology and 30+ patents. But Topo believed they had these other ideas about how to be in business – things like a business plan. DI never had one. Some of their technical ideas were just stupid and some were reasonable. But their products were not well-executed. The closer I

> looked at the Topo instrument, the more repelled I was by the way the thing was put together and the attitude of "throw instruments over the transom and see if anybody bites." Another important thing about Topo was that they allowed investors to play with them. We would get calls every week at DI from people with money wanting to buy in, but we turned down all of them. Topo gave investors a chance to participate – and most investors don't know a good high tech product form a bad one. So we had a competitor. [JW1, 10/18/01]

It's easy to dismiss this comment as the rhetoric of one manufacturer about another; but something about Topometrix – both its instrument designs and its business practices –offends the sensibilities of DI people. Topo's use of modern business methods, I think, threatened DI's ethic of iconoclastic skepticism; indeed, it was partly because of Topo's success that DI became more formal and businesslike. As we will see, this transition has sparked some friction at Digital. Thus, pursuing Topometrix for patent infringement became an emotionally charged issue with DI, and one that, ironically, ended up draining resources and dissipating much of the company's focus.

> Then we got involved in this patent dispute. And instead of going into a cross-licensing thing, or giving them any credit for their intellectual property or whatever the hell else one normally does we fought like a pit bull. Looking back, we could have cut a deal and and let them die on the vine by dint of their own incompetence. But, we were going to take the fuckers down. We were bloody-minded, and this gets back to Virgil's heart-head fusion. In his view, they had offended our sensibilities by polluting our market with such an inferior product that they didn't deserve to breathe the same air as us. The result was that a huge amount of management's time and energy and focus was consumed by this holy crusade against the evildoers of Brand X. [JW1, 10/18/01]

For Topometrix, DI's patent was themselves a source of indignation – how could you patent an obvious extension of earlier work? Indeed, for Topo, the lawsuit was an opportunity to unite different strands of indignation about Digital across the probe microscopy community; in the end, though, few microscopists were willing to voice their complaints in court, even when they might be germane to the case.

> Topometrix, historically, was doing quite well through 1993-1994. When it got involved in this lawsuit it just pulled the wind out of the sails.... During that time Topometrix had gone from being a $12 million to being a $9 million company. Barely surviving. And Digital Instruments had grown from being say, at the start of the lawsuit they were an $18 million company, they had

> gone to $40 or $50 million.... The patents they were alleging that we infringed were not valid patents. Flat out they weren't valid patents. They were patents for things that had been done for years.... Customers don't want to get involved.... There were people who could've made a difference.... They could've stood up and said, "I did that 3 years before the patent was filed. I'll sign a declaration. It was too stupid to publish in a paper." But they wouldn't do that because they didn't want to get involved. [PW2, 3/30/01]

The suit dragged on for three and a half years, ending in a draw – DI and Topo settled out of court by agreeing to drop the suits and counter-suits and cross-license their patent portfolios. In truth, though, Topometrix was decimated – its customers had been scared off, its R&D had been derailed, and its finances had been drained. DI, meanwhile, was expanding at a dramatic rate – its industrial microscope, the Dimension, was selling rapidly, and its hold on the research market was still firm.

This disparity in fortunes allowed DI to begin eliminating its biggest rivals through purchase. Between 1997 and 2001, the probe microscopy marketplace saw a remarkable streamlining (or constriction) as various competitors merged and submerged. First Park sold itself to Thermo Electron, an instrument manufacturing holding company. Then, Park (now christened Thermomicroscopes) hired Topometrix' former CEO, Gary Aden, to reorganize the failing enterprise; Aden convinced his employers to purchase Topo and merge the two former rivals [GA1, 3/12/01]. Next was DI's turn – through the mid '90s, Elings had been looking for someone to buy his company. An initial approach from Wyko (a semiconductor manufacturing equipment company) turned sour, but eventually he started negotiations with Veeco, the company that commercialized Big Blue's SXM.

Elings' deal with Veeco (which he and DI/Veeco's managers call a merger, but many see as a buyout) has had many ramifications, but the reasoning at the time was simple. First, Elings personally received a couple hundred million dollars. Second, DI benefited from Veeco's entrenched position in the semiconductor market; Digital had the microscope, but Veeco had the contacts and experience in the closed community of chip manufacturers. AFMs for semiconductor process-lines can sell for more than $1 million each, so each sale is crucial; and Veeco and its subsidiaries have been selling process-line equipment since the founding of the semiconductor industry in the '50s. Third, Veeco offered DI more orthodox management methods, and a structure for surviving the inevitable retirement of its charismatic leader. Elings had built DI on personal rule; on both the technical and business sides, almost everyone reported directly to him [PW2, 3/30/01; JW1, 10/18/01]. Since he had never groomed a successor with the same kind of charismatic authority, Elings felt DI needed more by-the-book management to survive his departure.

Being part of Veeco made possible the final phase of DI's triumph. In late 2000, Thermomicroscopes, slow in recovering from the Park/Topo merger, sold itself to DI/Veeco and became a subsidiary, TM Microscopes. It is amusing and a little sad to watch DI employees adjust to this addition. On the one hand, (as of mid 2002) they sometimes sound awkward and faltering in praising instruments that for a decade they had excoriated; on the other hand, they also express a shocked wonderment that, after all, there were some really smart people with some really good ideas at Thermo.

Post-consolidation Proliferation

The "rationalization" of the Big Three manufacturers – their consolidation into one company, the streamlining of product lines, and the institution of highly rational business practices – has had some benefits for both the companies and their customers. The iron cage of consolidation, though, has also produced irrationalities and anxieties. So far, customers have complained little – some are uneasy, but content to wait and see how their lives are affected by the decrease in competition and the gradual elimination of open-architecture AFMs. The anxieties of employees at DI and its former rivals, though, have broken into the open. The best evidence for this is the exodus of employees from these companies in the late '90s. At Park, many experienced engineers and scientists began drifting away even before the merger with Topometrix; after, even more moved on, since Thermomicroscopes eliminated Park product lines in which many in the company had a deep personal investment. Interestingly, former Park employees have formed an enclave at KLA-Tencor, a Silicon Valley semiconductor manufacturing equipment company and one of DI/Veeco's major competitors in the wafer characterization business. Thus, while the Park people no longer have their own company, they do still work together in an organization that competes with DI's parent company.

At DI, the Veeco merger sparked unease and secession. With the arrival of the Veeco management team and their rationalization of DI's operation, many of Elings' people felt alienated or sidelined. The transition from charismatic leadership to routine management was jarring; and some saw Veeco's philosophy as robbing DI of the spontaneity and chaos that made it successful. Thus, employees drifted away:

> A couple years after the merger with Veeco, I would say the small AFM business was teetering on disaster because we lost, I did an analysis, about half of our AFM experience in years on the side of the company that dealt with the small systems.... The management really changed and there was a lot of uncertainty. [KB1, 3/23/01]

In part, we can see these tensions as mirroring the role instabilities faced by builder and runner groups in the academic world. With SPM commercialization now firmly entrenched, and the pace of innovation for the research market slowing, it was no longer clear what it meant to design and build commercial research STMs and AFMs. With consolidation, the moral economy of selling SPMs changed, and some DI engineers saw influence and respect shifting away from them.

> Ed Braun [a Veeco executive] comes in, he's a little short guy, stands on a stool and says "you guys are doing everything right, why would we ever want to change anything, you guys are making money doing a perfectly good job, we're not going to do a thing, we're just going to leave you alone." Of course, they take over and one of the first things that happened that got the core group of people talking was they started giving the sales force commissions. That may not sound like a big deal because pretty much every company does that.

> DI actually never did for what I think is a very good reason, in that a sale, at least in a company that sells high-tech instrumentation, actually involves a lot more people than the salesman. I mean the salesman will make the contact and maybe open and close the deal, but he'll also bring the person back and somebody from applications will run the samples and get the data and do all this stuff, and maybe the guy's going to have some technical questions, he'll talk to some engineers, and he'll need a new feature added so he'll talk to some software people. Basically a sale involves a bunch of people not just a salesman, so why should that guy get a huge cut of a sale for basically not having any, I don't want to say non-productive, but I mean from an engineer's point of view what a salesperson does is non-productive, it doesn't contribute to any new products or anything like that. [DB2, 3/23/01]

Like their academic counterparts, many commercial SPM builders look fondly on the late '80s and early '90s, when life was more exciting, work was more chaotic and less managed, and their individual contributions made a greater difference. Accordingly, a group within DI began planning ways to revive the lost world of the SPM startups. The name of their venture – Asylum Research – is, of course, a double entendre. On the one hand, Asylum is a refuge for those marginalized by Veeco; on the other hand, an asylum is a place for misfits and eccentrics, and the secessionists clearly want to cultivate the chaos and "craziness" that they see as underpinning DI's success.

> While [Elings] hung around he ran a shield between DI and Veeco in New York, but the cultural chasm that existed between those two companies was just absolutely incredible.... DI was an incredibly flat management style. For the most part it was Virgil and Gus, the president and the VP. In production there was a little more structure but on the R&D side it was mostly that and the creativity and more a free spirit attitude. What you'd typify as a small company spirit at DI. The neat thing was since the guy who ran the place was a Ph.D. in physics, being a scientist there was as important as say being a sales guy or a production guy or something like that. If you actually look at the Veeco management structure it's almost all sales and those are the people that get promoted and that's what's important there.... [Elings] told some of us that he was planning to retire in May of '99 and so a group of us got together and decided once he retired that it wouldn't be a place we really wanted to work so we started this place in April of '99. [JC2, 3/20/01]

Today, Asylum (still based in Santa Barbara) makes force pullers and is starting to market an AFM and clearly intends to offer a counterweight to their much bigger parent and neighbor. In doing so, they've received moral support from Hansma and other old-time academic builders. The advantages to academic builder groups of cultivating a DI spin-off should be clear – it increases competition (and thus more variety) in the market, it offers more opportunities for academic builders to establish collaborative projects with manufacturers, and it turns back the clock to an earlier era when manufacturers were small and depended heavily on the academic builders.

Conclusion

Whether Asylum succeeds or not, its appearance marks one kind of turning point for SPMers. For the first time, some in this community have pointed out the loss that attends routinization and rationalization. DI has been tremendously successful at spreading the routine use of SPMs into countless new areas; yet *use* has so far never been completely separable from *innovation* in probe microscopy, and the routinization of the one has led almost inevitably to the routinization of the other. Many builders, inside and outside DI, are unsure how to justify their work in this new era, and the existence of Asylum is a pointed testimony to this confusion. Yet it is also taken as a sign of hope and health. Commercialization is, in some sense, anathema to builder culture, in that it seeks ever larger markets for off-the-shelf instruments; yet, as we've seen, it is also strongly *dependent* on builder culture for sustaining the growth and evolution of the instrumentation. If the manufacturers (particularly DI) are too vigorous in rooting out or routinizing builder culture, they will only suffer in the end. With Asylum, this realization has hit home at DI, and Digital is now taking steps to reinvigorate its own builder culture, and to re-establish its presence in the research market that inspired its most innovative period in the early '90s. Asylum's appearance, then, may be a sign that the SPM community has found a way to signal stagnation and provide an occasional much-needed escape from the iron cage. This is not a new story – much the same process happened in the semiconductor industry in the '60s and '70s, when the rapid proliferation of start-ups and spin-offs from giants like Fairchild and Texas Instruments drove innovation and discouraged ossification among manufacturers (Lecuyer 1999). Probe microscopy may not be as frenetic as semiconductors, but many now see a need for that kind of safety valve.

In general, the appearance of Asylum, and the reinvigorated response to it by DI, is synecdochic for larger shifts in the SPM community at the start of the millennium. Many SPMers' lives reached various existential crises at the turn of the decade: the corporate labs were suffering a long decay, the STM Conferences were searching for a raison d'etre, and many pioneers of the technique were retiring. Just as in the DNA controversies of 1990-2, the SPM community of 2000-1 seemed to be looking for a new kind of social order, a new kind of knowledge to go with it, and new technical identities to secure within the new order. As of 2000, with the announcement of the National Nanoinitiative, this new order seemed to be on the horizon. Nanotech, whatever it is, is for now flexible enough to offer the kinds of safety valves and social glue that SPMers seem to need – it is fuzzy enough both to let builders be charismatic and innovative, and to let ordinary users of commercial microscopes forge a common instrumental and visual language. In the conclusion of this dissertation we will briefly look at some of the relationships between nano and SPM; those relationships, though, are rooted in the exigencies of commercialization that we have explored here.

Chapter Nine

Probe Microscopy and Nanotechnology

In 2000, when Bill Clinton prepared to announce the creation of a National Nanotechnology Initiative, his aides looked for an appropriate place for him to make a speech on the topic. They chose Caltech, one of the mythical birthplaces of nanotechnology, the school where Richard Feynman taught for most of his career and, in 1959, gave the now-famous "There's Plenty of Room at the Bottom" after-dinner speech that today plays such a prominent role in the imaginations of nanotechnologists (Feynman 1999). Clinton's aides also looked for an appropriate backdrop to hang behind Clinton during the speech, one that would display American achievements in the field. Here, they chose the other great bonfire of the nanoimagination – atomic manipulation with an STM. It is difficult to find reviews of nano that do not prominently mention or display Don Eigler's spelling of "IBM" with single xenon atoms in 1990. Eigler's images are beautifully rendered and rhetorically powerful – they were the first to speak to the current reality of nano's long-term vision of putting atoms exactly where we want them.

So Clinton's aides called Eigler's lab at IBM Almaden, and asked his postdoc, Hari Manoharan, if he would spell out "NANO USA" and send them an image for the presidential backdrop. At the time, Eigler's group was conducting experiments on CO on nickel – one of the classic systems of surface science. Since Eigler's machine can maintain an ultraclean, ultracold environment for years at a time, he rarely changes samples unless a series of experiments on that material has finally run its course. So Manoharan decided to use the CO that was already in the chamber to spell out the presidential headline. When he was done, he sent the image to Washington, and one of the aides quickly called back to congratulate him and to ask, for the record, what atoms were used in the message. "Well, it's actually not an atom, it's a molecule." "What molecule, then?" "Carbon monoxide." "Isn't that a poison? I don't think we can use that" – and the NANO USA image quietly disappeared from the scene.

Probe microscopists are fond of telling this story, both publicly and privately, and they usually give it a very knowing spin [PR1, 3/7/04]. That is, it demonstrates for long-time STMers and AFMers that, whatever nano is, they are central to it, from which they take great pride; but it also shows to them that the whole nano rubric is a joke, a concoction of bureaucrats and financiers with little regard for the daily practice of science. There's a lesser-known sequel that adds a codicil to this moral. After they dropped Eigler from their plans, Clinton's aides went back to Almaden to find another group that would give them a suitable image. This time they contacted Dan Rugar and John Mamin, who had developed a technique to deposit small mounds of gold, containing about a thousand atoms each, in a controlled way. Clinton's people asked them to make not "NANO USA," but an image of the Western Hemisphere as seen from space – a reminder, perhaps, of one of the last great American "big science" intitiatives, the race to the moon [DR1, 3/14/01; JM1, 3/15/01].

The relationship between nanotechnology and probe microscopy goes back a very long way, but it is also so complex that is obscure even to the participants. Part of this relationship is the work of people who are seen to be appropriating STM and AFM work without being credible members of the community; part of the relationship has been forged by important mediators at the margins of the community, in response to significant changes in probe microscopy, in the disciplines which use STM and AFM, and in the wider scientific establishment of Europe, North America, and Japan; and part of the relationship was created by SPMers themselves in response to role dilemmas brought on by commercialization and other changes in the landscape of probe microscopy. Thus, there is a great deal of ambivalence among SPMers about nanotech; some see it as the savior of the probe microscopy field, others see it as a healthy opportunity to gain funding, forge collaborations, and create markets; and others see it as an unwelcome imposition by bureaucrats and dreamers.

Drexler and Futurism

In this epilogue, I want to trace two different tracks of rhetoric about probe microscopy and nanotechnology, one older but with thinner ties to the field, the other more recent but somewhat more organically related to SPM work. The first is the tradition most closely associated with Eric Drexler and the Foresight Institute in Palo Alto. According to legend, Drexler began thinking about "molecular engineering" while still an undergraduate, reading up on the latest in supramolecular chemistry – especially computerized molecular modeling – and genetic engineering.[1] This led him to posit the possibility of designer molecules, of a world carefully put together atom-by-atom. This was a "bottom-up" world of goods built one atom after another, with complete precision in their placement, rather than the familiar and ancient "top-down" world in which goods are made by chiseling away at large, undifferentiated masses. From this simple starting point, Drexler imagined endless possibilities made real – cheap spaceflight, unlimited energy, green manufacturing, a universe of information at everyone's fingertips, even immortality. Crucially, Drexler's world was also one of terrifying hazards – of ubiquitous surveillance, of home-brewed weapons of mass destruction, and even the extinction of humanity, or even all life on earth, at the hands of our intelligent, autonomous, nanobot creations.

Though Drexler based much of the technical content of his discussions on advancements in the '70s in biochemistry, the traditions that gave birth to those discussions were the visionary communities surrounding interplanetary spaceflight, artificial intelligence, and new information technologies. These were communities

[1] I draw on Regis (1995) for biographical information on Drexler.

that took as their heroes people like Babbage, Tsiolkovsky, and Goddard – visionaries who failed to accomplish their grands schemes, and sometimes were even laughed at in their own time, but who were later celebrated as heralding an inevitable technological revolution. Thus, there is at times a strange mixture of utopianism, paranoia, and resignation in Drexler's writings and presentations – a belief that he and his group are forecasting something which is inevitable and which will likely be beneficial, but that such prescience can be both boon and bane.

There is also, as Stefan Helmreich has pointed out, a recurring trope of liberation in these futurist traditions – whether liberation from the earth (spaceflight), from the body (artificial intelligence), from dead media such as books (Drexler was also involved in early hypertext projects, and it is a little-noticed fact that almost all his writings end with proclamations about hypertext and other new media), or liberation from death (nanotechnology) (Helmreich 1998). The institutions of such visionary projects tend to be unique, disparate, and only loosely affiliated with traditional academic disciplines – organizations like the Space Studies Institute (founded by Drexler's undergraduate mentor, Gerard K. O'Neill), the MIT Media Lab (where Drexler nominally received his Ph.D. under AI guru Marvin Minsky), and Stewart Brand's *Whole Earth Catalog* and Global Business Network, of which Drexler became a part when he moved to Palo Alto and founded his own nanotechnology organization, the Foresight Institute.[2]

My point is that Drexler belonged to a well-established techno-visionary tradition, with its own culture, rhetoric, and institutions. Through the 1980s, though, the links between these institutions and the settings where most STMers and AFMers could be found were quite spare. Since Drexler's vision depended on getting

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[2] Brand is on the board of the Foresight Institute and Fred Turner informs me that he and Drexler are personal as well as professional friends. See Turner (forthcoming) and Brooks (2003) for fascinating descriptions of Bay Area futurism and techno-counterculture.

practicing scientists to think about the consequences of their work and to begin coordinating their research around the concept of molecular nanotechnology, he set out to increase his contacts with working research communities, particularly the probe microscopy field. One way to do so was to reference prominent scientists who stood at the intersection between the techno-visionary and research worlds. No figure played this role more perfectly than Richard Feynman, the Nobel Prize-winning physicist and maverick.[3] In an after-dinner speech to the American Physical Society meeting at Caltech in 1959, Feynman laid out a compelling vision of tiny mechanical motors and manipulators and nanometer-scale information storage, a vision Drexler and other nanotechnologists have repeatedly appropriated as the founding statement of their field, even as they use Feynman's vision to justify quite contradictory interpretations of that field.

As Davis Baird and Ashley Shew have nicely shown, Feynman's "Room at the Bottom" speech has become the rhetorical entrée for probe microscopy into the nanotech arena (Baird and Shew forthcoming). In the speech, Feynman makes a loud call for microscope development:

> If I have written in a code, with 5 times 5 times 5 atoms to a bit, the question is: How could I read it today? The electron microscope is not quite good enough.... I would like to try and impress upon you ... the importance of improving the electron microscope by a hundred times.... [I]t should be possible to see the individual atoms. What good would it be to see individual atoms distinctly? We have friends in other fields – in biology, for instance. We physicists often look at them and say, "You know the reason you fellows are making so little progress?..." You should use more mathematics, like we do." They could answer us ... "what *you* should do in order for *us* to make more rapid progress is to make the electron microscope 100 times better...." It is very easy to answer many of these fundamental biological questions; you just *look at the thing!* You will see the order of the bases in the chain; you will see the structure of the microsome. Unfortunately, the present microscope sees at a scale which is just a bit too crude. Make the microscope one hundred

[3] Various hagiographies of Feynman, such as Gleick (1993) demonstrate his enduring iconic status for physicists and non-physicists alike. Kaiser (forthcoming-a) contains a somewhat more sober yet fascinating account of the growth of Feynman's network.

> times more powerful, and many problems of biology would be made very much easier. (Feynman 1999)

As Baird and Shew demonstrate, in today's standard histories of nanotechnology, this statement from Feynman is almost always given point of pride; and it is almost always used as a stepping stone to discuss the STM, atomic resolution of the 7x7, and the implications of probe microscopy for nanotechnology.

When Drexler first began formulating his version of nanotechnology, the STM had not yet been invented, and he seems never to have referenced the atomic resolution capability of the field-ion microscope or the transmission electron microscope. His first published writings on the subject, though, came in 1983, the same year Binnig and Rohrer's images of the 7x7 were published; and his first, widely-read book on the subject, *Engines of Creation*, appeared in 1986, the same year Binnig and Rohrer won the Nobel Prize (Drexler 1990). *Engines* is Drexler's *popular* opus, so it is sparing in technical details about anything, yet even here he points to the STM's importance to the grand project. His far more technically-oriented book, *Nanosystems* (published in 1992), goes a step further and includes an entire section discussing use of the AFM in "mechanosynthesis" (i.e., the formation of molecules and supramolecular systems by pushing atoms into place mechanically, rather than by the more haphazard methods of traditional chemistry) (Drexler 1992). Paul Hansma and a number of his and Cal Quate's collaborators are thanked in the preface, making probe microscopy (along with the protein engineering community) one of only two fields to be acknowledged so specifically.

Thus, Drexler made a serious effort in the early '90s to enroll prominent probe microscopists in his project – and with good reason, since much of his argument depended on both the *inevitability* and the *outlandishness* of his claims. That is, he paints the post-nanotechnology world as inevitable, yet as un-believably different from our own world. One way to do this is to point to current research that has already achieved things that are outlandish, and which point the way to an even more unbelievable future. Atomic and molecular manipulation with an STM or AFM – especially when colorfully done, as with Don Eigler's 1990 spelling out of "IBM" using xenon atoms – is canonically an activity of this sort: unbelievable, yet real, with the possibility of more unbelievable parlor tricks and even products down the road. As Drexler put it in testimony before the US Senate in 1992,

> Five years ago when I spoke on the subject, audiences would reply, "You say that the basis of molecular nanotechnology is putting molecular building blocks in precise places, but is that really possible?" Today that question does not arise because part of my talk is a slide showing 35 precisely placed xenon atoms on the surface of a nickel crystal, spelling the letters IBM, from work done by Don Eigler's group at IBM's Almaden Research Center. (Drexler 1993)

Yet not all probe microscopists agreed that their work pointed to the future Drexler envisioned, and some resented his attempts to draw them into his network. Eigler, for one, said that Drexler "has had no influence on what goes on in nanoscience. Based on what little I've seen, Drexler's ideas are nanofanciful notions that are not very meaningful" (Rotman 1999). Quate, too, observed of Drexler (in the *New York Times*) that "I don't think he should be taken seriously. He's too far out" (Regis 1995, 232).

One of the few probe microscopists who did engage with Drexler, though, was Quate's former student, John Foster. Recall that Foster, along with Jane Frommer, had developed a project at IBM Almaden to use air STM to examine complex molecules (organic compounds and liquid crystals) on graphite, yielding some of the first unequivocal images of solitary molecules. Later, Foster discovered that he could actually move the molecules around, "dissect" them with voltage pulses, and even "herd" them into groups. This was exactly the kind of "mechanosynthesis" Drexler saw as heralding the coming age, and, given Foster's geographical and cultural proximity to Foresight, it was natural that he was invited along with a few other practicing scientists to join the Institute's first conference on nanotechnology in 1989

(Foster 1992). There, Foster found an enthusiastic audience and a welcome understanding of the importance of work on the nanoscale, but also a sometimes unnerving willingness to believe the impossible:

> Drexler .... entered this picture at that time. He would invite me to those early conferences that he had. Because frankly there were only so many of us that were actually doing anything and we were among the few who were actually doing any kind of nanomanipulation at all, making it work. So he wrote that up in his books and so forth.... Drexler – there is this funny thing in science .... if you have a new idea it's hard to get it in.... Not that I'm comparing Drexler to any of these folks, but if Galileo comes up with an idea everybody says "no, that's not right, you're crazy...." It's easy to pick on people that come up with a vision. That being said, Drexler is kind of way out there on the end because he had quite a strong vision, it was just huge.... And he had some people sort of in his court that were very powerful, the Feynmans of the world.... There have to be people that are hard-core scientists and are show-me. On the other hand you have to have the visionaries, because if you don't they aren't going to go very far in a hurry, and you won't make some leaps of faith.... I can see there's value in both camps, though Drexler was so hard over in this other camp that it was difficult for some people to take him seriously at all.... It's not like people [mainstream researchers] were really discussing "well, gee, what's physically wrong with that [Drexler's] picture, why can't that happen? Isn't this okay, won't this work out? I mean biology works this way." That's really [Drexler's] argument. Certainly it's that we ought to be able to do what biology does.... I believe his arguments, I believe it can be done. But anyway he wasn't very well received by the scientific community. [JF1, 10/19/01]

Foster makes an important observation here. It is common to hear practicing probe microscopists express unease about Drexler's vision of the future. It is also common, particularly since the founding of the Nanoinitiative, to hear prominent nanotechnologists decry Drexler's work in terms that are simultaneously technical and *ad hominem*. Nanotechnology today is entering a phase in which the ritual expulsion of Drexler is central to constructing the boundaries and the legitimacy of the discipline (Gieryn and Figert 1986).

This is one reason why nano is such a fascinating laboratory for exploring the themes of science and technology studies. In the divide between Drexler and his detractors we can see "interpretive flexibility" and the "social construction of knowledge" played out before our eyes. There are, for instance, a handful of technical issues on which Drexler and his detractors (some of whom, such as Rick Smalley, were at one point admirers) have repeatedly argued – most dramatically the “sticky fingers, fat fingers” discussion about how, exactly, Drexler proposes that atoms will be positioned precisely by molecular assemblers (Baum, et al. 2003). Each time they debate, they produce the same claims and counter-claims and counter-counter-claims. As in any technical debate, they disagree not just on the scientific matter, but on whether each has adequately understood the other, whether counter-claims are germane, and whether more iterations of claim and counter-claim are necessary. In such a situation – as the sociology of scientific knowledge has amply demonstrated – there is no way for “reality” to speak for itself and inform audiences to the debate as to which side has access to nature and which does not. Any such truth-claim can always be met with yet another counter-claim that in turn begs for refutation.

Thus, audiences must make social judgments to supplement the technical claims at play. Indeed, much of the time audiences radically truncate their attention to the potentially long-winded repartee of claims and counter-claims; knowing what they know about the participants in the debate, many audience members will hear and believe one side’s claims without hearing out the other side’s initial argument, much less their succeeding iterations of counter-claims. Most probe microscopists watching Drexler have, for good but eminently social reasons, decided that Drexler’s visions are fantastic and unhelpful; by dint of his style and position outside the institutions to which they belong, most probe microscopists (even the ones Drexler routinely cites) do not view themselves as contributing directly to the nano world Drexler imagines.

Nano Succeeds Micro

Nonetheless, it is difficult for even the most die-hard skeptic to deny that Drexler put his finger on a quickening pulse within various research communities, and that in doing so he brought attention to research that could easily have labored in obscurity. Moreover, the Drexlerian vision of "nanotechnology" proved ambiguous enough that many different actors could seize on it and broaden its scope. Indeed, as it turned out, Drexler was not the first to refer to nanotechnology; the label was probably coined by Norio Taniguchi in 1974 in a paper entitled "On the Basic Concept of 'Nano-Technology'" (Taniguchi 1974). Taniguchi was writing for an audience of precision engineers, the same community for whom Russ Young had tried to make the Topografiner relevant just a couple years before. Young and Taniguchi were (perhaps ahead of their time) both pointing to the fact that precision engineering was beginning to deal with tolerances well below the 100 nanometer mark. This was the case for macroscale artifacts like stepper motors and ball bearings, but it was even more true for microscale artifacts like integrated circuits and air bag accelerometers.

That is, as the microelectronics industry exerted itself to fulfill Moore's Law (the rough doubling of the number of components per chip every 18 months), the *absolute size* of some features began to dip below 100 nm (at least in one or more critical dimensions), and *tolerances* crept to even smaller sizes.[4] Taniguchi was the first to give a label to what many in the microelectronics industry realized: that if Moore's Law were to be sustained, the industry would be working in the region of the nanometer (10^{-9} meters) rather than in the region of the micron (10^{-6} meters). By the time Drexler popularized "nanotechnology" in the late 1980s, those working in the avant-garde of this miniaturization effort had begun to substitute the prefix "nano" for "micro" in much of their rhetoric.

Drexler's own vision of nano differed from that of miniaturization specialists, in that he imagined a bottom-up world of building things from atoms, rather than a top-down world of making miniaturization technologies ever finer. Yet the term itself was ambiguous enough that almost anyone working on systems where at least one

[4] See Mackenzie (1996) for a nice analysis of the performative aspects of Moore's Law.

dimension had features with critical lengths less than 100 nm could lay claim to it. Moreover, as some researchers began pointing out, the purview of "bottom-up" fields like supramolecular chemistry had begun to overlap with the that of "top-down" fields like electron lithography. "Nanotechnology" now began to refer to the *overlap* of the two approaches; and probe microscopists were especially well-situated to capitalize on and promote this interpretation of the term. Heini Rohrer, for instance, began writing on the topic in the early '90s, stating in 1993 that:

> Miniaturization naturally carries us far beyond microtechnology, it carries us to science and technology on the nanometer scale – into the nanometer world. In the following I would like to discuss some aspects of the next big step of miniaturization, the one from the micrometer to the nanometer, in which local probe methods will play a most important role.... Ten years ago nanoscience and technology were not yet commonly used terms. But it was foreseeable already at that time that in advancing into the world of the ever smaller, miniaturization would not stop at the micrometer. (Rohrer 1993)

In 1995 he wrote that:

> While solid-state science and technology have moved down from the millimeter to the nanometer scale, chemistry has simultaneously and independently progressed from the level of small, few-atom molecules to macromolecules of biological size.... The nanometer age can thus be considered as a continuation of an ongoing development: for example, miniaturization in solid-state technology [and] increasing complexity in chemistry. (Rohrer 1995)

Probe microscopists could be found at all points along this spectrum between bottom-up and top-down. On the one hand, there were a few (mostly builder) groups working on manipulation of objects from single atoms to fullerenes (C_{60} molecules) and buckytubes (sheets of carbon rolled into cylinders) and everything in between. On the other hand, AFMs, MFMs, and more exotic tools like scanning capacitance microscopes and scanning thermal microscopes were becoming useful to people working in both corporate research and industrial quality control labs trying to deal with integrated circuit features that were now smaller than 100 nm, and magnetic thin films with thicknesses diving below 10 nm [DR1, 3/14/01; DB3, 4/30/01].

It is in this context of a perceived convergence of top-down and bottom-up approaches in the late ’80s and early ’90s that we begin to see the first gestures toward nanotechnology among probe microscopists. In general, these gestures exploited the ambiguity of the term by latching onto a buzzword without signing onto any particular (especially Drexler’s) interpretation, and maintaining the plausibility that “nano” simply referred to probe microscopy’s higher-than-“micro” resolution. For instance, the “nano” in Digital Instruments’ NanoScope (introduced in 1987) was probably meant to signify that DI’s STM was superior to traditional light and electron *micro*scopes in that its resolution could push well below one *nano*meter. At the same time, whether intentional or not, selling a product with “nano” in its name rhetorically positioned DI well, if and when nanotechnology became the next “big thing.”[5]

Possibly the earliest attempt by probe microscopists to align squarely with “nanotechnology,” rather than to hedge on commitment to a label with doubtful technovisionary baggage, was Quanscan’s motto “innovators in nanotechnology.” This, however, was the exception that proves the rule. Recall that in 1988 Quanscan was a marginal company living off government grants and venture capital money rather than the sale of products. As such, Quanscan was always oriented to a much more distant and visionary future than competitors like DI and Park. Indeed, I think we can see the instinctive aversion of many DI employees to Quanscan/Topometrix’ perceived preference for glossy marketing and long-range, visionary planning over solidly-engineered products as strongly akin to the instinctive aversion of most probe microscopists to the rhetorical style of Drexler and the Foresight Institute.

One other mechanism for probe microscopists to hedge their way closer to nanotechnology came through a series of scientific “bandwagons” that occurred in the

[5] Again, this is nice example of the skilful use of interpretive flexibility that Lynch and Bogen (1996) have labeled “sleaze.”

early ’90s. As we saw in Chapter Six, SPMers (particularly builder groups) were always on the lookout for “hot” materials to characterize, a predilection that left the early days of the technique marked by fads and gold rushes. Some of these fads (such as the sudden popularity of graphite) were more or less internal to the probe microscopy community. Others, though, began outside the field and drew probe microscopists in. The discovery of high T_c superconductors at IBM Zurich in 1986, and the attendant frenzy of research on superconductivity, for instance, spurred many STMers and AFMers to turn their attention to these materials. Similarly, when the Human Genome Project came into being in 1990 and started casting around for suitable instrumentation, probe microscopists suddenly turned in droves to DNA.

Some of these bandwagons involved materials that stood right at the point of convergence between the top-down and bottom-up approaches, materials that today are seen as prototypical nanomaterials. Probe microscopists, for example, were among the first to think about the *mechanical* (rather than just biochemical) properties of DNA [SL1, 1/6/03; EH1, 6/22/01], and a number of groups collaborated with experimenters who could make (but not see) intricate erector-set-like three-dimensional nucleic acid *structures*. Similarly, probe microscopists became interested in nanometer-thick Langmuir-Blodgett films just when those materials experienced a revival under the new moniker “self-assembled monolayers.” Indeed, “self-assembly” soon became a much-discussed topic among nanotechnologists, and people like George Whitesides who are today leading nanotechnologists began growing monolayers (a bottom-up process) in combination with top-down techniques like lithography. Likewise, when fullerenes and nanotubes hit the scene in the late ’80s and early ’90s, probe microscopists caught the “fullerene fever.”[6] Builder groups, especially, quickly procured the new materials, provided the first images of them, and

[6] The spread of fullerene research has in fact been studied using epidemiological models (Braun 1992).

then started manipulating them to form nanostructures – a fullerene abacus, a transistor made from nanotubes, even gluing nanotubes to AFM cantilevers and using them to probe deep trenches (where a long, thin tip is more desirable).

Institutional Support

Thus, by 1991-2, probe microscopists stood ready to take advantage of their nanotechnological credentials, but they also hedged those bets and continued working within more traditional programs in surface science, biophysics, electrochemistry, etc. Through the '90s, this was the general orientation of most probe microscopists who knew anything about nanotechnology, and it is probably where the community would still be if nano had not started to accrue institutional support. Though it is clear that many of the constituent components of nanotechnology would have been supported whether there had been a "nano boom" or not, it is also clear that research-sponsoring institutions like the NSF and IBM, as well as disciplinary organizations like the American Physical Society and the Materials Research Society played a key role in herding the disparate sectors of nanotechnology under one umbrella and thereby lending credence to proclamations that nano was the "Next Industrial Revolution."

Interestingly, it was in putting nano on the agenda of institutions like these that Drexler may have had his most lasting, if indirect, influence. *Engines of Creation* reached a wide audience, including influential politicians and civil servants, and by 1992 Drexler's ideas were beginning to get a hearing. Al Gore, for instance, seems to have been inspired by the ecological implications of nanotechnology, and brought Drexler to testify before a Senate committee hearing on "New Technologies for a Sustainable World" just a few weeks before becoming Bill Clinton's running mate (Drexler 1993). Later, Gore seems to have given instrumental support for the founding of the National Nano Initiative (Atkinson 2003, 86). On the same trip to Washington for the committee hearing, Drexler also gave a presentation to Adm.

David Jeremiah (the Vice Chairman of the Joint Chiefs of Staff) and the Joint Requirements Oversight Council.[7] Jeremiah has become one of the primary links between Drexler and mainstream nanotechnology; and, though he was certainly not the only instigator, it was through naval research that the institutional grip of nanotechnology began to take hold of probe microscopy.

The roots for this go back to the very earliest days of tunneling microscopy. When the STM first came on the scene, it caught the attention of surface scientists at the Naval Research Laboratory outside Washington, DC. One, Rich Colton, took a sabbatical in the Baldeschwieler group, helping to build their first air STM. When he came back, his group bought one of Doug Smith's "commercial" air STMs, and brought in a former NIST STM postdoc, Lloyd Whitman, who became a staff scientist and set up a UHV system at NRL [RC1, 6/27/02; LW1, 6/27/02; NB1, 2/20/01; GL1, 7/19/01]. Thus, the NRL embarked on probe microscopy at about the same time, to the same extent, and by the same means as other national labs with prominent surface science groups – NIST, Lawrence Berkeley National Labs, and Lawrence Livermore National Labs.

Importantly, though, Colton's manager was Jim Murday, a leading member of the AVS and a leading surface science grant officer. Through Murday, the ONR began sponsoring probe microscopy research, both within surface science groups as well as non-surface science builder groups like Quate's and Hansma's, groups working on sensor and data storage technologies derived from what Murday termed "proximal probe" techniques [JM2, 7/6/00]. At the same time, with support from Murday and others, the AVS also began supporting probe microscopy work – again, across the whole range of research, not just within surface science. For instance, along with its international counterpart, the IUVSTA (the International Union for Vacuum

[7] See Jim Murday's introduction to Ratner and Ratner (2004).

Science, Technique, and Applications) the AVS became the primary institutional sponsor of the STM Conferences. Moreover, the AVS published the proceedings of most of these conferences in its *Journal of Vacuum Science and Technology* (Feenstra 1988; Ichinokawa 1990; Bai, et al. 1994; Colton, et al. 1991; Hamers 1995).

By 1990, though, surface science was beginning to lose its primacy, both within the AVS and more broadly. The formation of an Electronic Materials and Processing Division at AVS in 1979 brought in practitioners from the microelectronics industry and, according to the president of the AVS at the time, doubled the size of the society in three years [CD1, 10/30/03], leading to the splitting of the *Journal of Vacuum Science and Technology* in 1983 into JVST A (Vacuum, Surfaces, and Films) and JVST B (Microelectronics, Processing and Phenomena). Through the 1980s, as the dimensions of integrated circuit components diminished, microelectronics began to eat away at surface science's purview over angstrom- to nanometer-scale surface phenomena in semiconductors. Moreover, with the break-up of AT&T and the collapse of IBM's dominance of the server and personal computer markets (and even more so during the recession of 1991-2), the big corporate research labs that had sustained surface science began to demand that its practitioners orient themselves more to product lines than research on reconstructions and other fundamental phenomena. As we've seen, this led many corporate STMers to shift to AFM and to more commercial projects.

Within the AVS, probe microscopy itself spurred the erosion of surface science's influence. When the AVS first became involved with probe microscopy, almost all work was done in UHV, on samples of direct relevance to surface science question. With air STM, and even more so with the AFM, the AVS found itself sponsoring conferences and publishing reams of articles with very little surface science or ultrahigh vacuum content. The AVS wanted to maintain the prestige and increased membership of its relationship with a hot field like probe microscopy, but at the same time the rationale for that relationship began to look like an historical vestige. Moreover, even the probe microscopy work within surface science introduced existential questions about the direction of the discipline. With the solving of what one prominent surface science theorist calls the "Rosetta Stone" of the 7x7, most other unsolved reconstructions fell quickly, largely with the aid of the STM (Duke 2003; Lagally 2003). Reconstructions, which had once framed much of the work and culture of surface science, became a much simpler game through the '80s [FO1, 10/24/01]. LEED practitioners, in particular, felt this acutely. Though LEED is still a useful technique, it is used more often to check experimental procedures than to generate knowledge; it has gone from being the mainstay of the discipline to an auxiliary tool, and those who specialized in it have had to find a new focus and new instruments.

Also, the direction of probe microscopy itself began to turn at this time. With the commercial instruments becoming more routine and more widespread, the character and composition of the field were changing in ways that questioned the need for a dedicated probe microscopy community at all. The rationale for the STM Conferences, for instance, became less clear by the end of the '80s. Their original purpose had been to display new innovations to the design of the instruments and to get new builders up to speed on how to get started. Commercialization could, therefore, have sounded the conference's death knell; indeed, Binnig and Rohrer's original conception was that the meetings would last for a few years and then when the technique became routine everyone would disperse back to the professional conferences of their home disciplines – the American Physical Society meeting, the Materials Research Society meeting, etc. In many ways, the manufacturers could fulfill the function served by the STM Conference – they could advertise new designs, train new users, and absorb innovations from the few remaining builder groups.

For a variety of stakeholders, though, the STM Conferences continued to play an important role. For the AVS, with Murday's guidance, they were a source of prestige and dues. For the manufacturers, they represented the greatest concentration of actual and potential customers, competitors, and builders and, hence, the greatest source of credibility, sales, information, and new innovations. For builder groups, they were a place to cut out the manufacturers as middlemen, or at least to use the desires of the community of commercial microscope users to leverage manufacturers to commercialize builders' innovations. Since the manufacturers and the AVS were the primary sponsors of the meetings, and since builders were usually in the self-selecting clique that ran them, the STM Conferences continued on; but the need for a new rubric with which to justify them had become urgent by 1990.

That year, the STM meeting was held in Baltimore (not far from the NRL), and Murday and Colton were the organizers. As such, they took the opportunity to reshape the direction of probe microscopy, surface science, and the AVS by renaming the STM Conference as the "NANO Conference" (or, to give its full name, the "Fifth International Conference on Scanning Tunneling Microscopy/Spectroscopy and the First International Conference on Nanometer Scale Science and Technology"). From Murday and Colton's vantage, "nanometer scale science and technology" was a natural way of tying together the now-disparate threads of the AVS and the probe microscopy community. Probe microscopists, of course, had little reason to deny that their instruments were working at the "nanometer scale." Microelectronics was rapidly approaching the "nanometer scale," and some both in the semiconductor industry and in probe microscopy were beginning to talk about replacing electron microscopes with AFMs on semiconductor fab lines (which, as we saw in Chapter Seven, resulted in the "big machines" and metrological AFMs developed in the '90s). Even surface science was now cast as always already nanoscience – by definition, the surface layers that interestingly differ from the bulk material are only a few nanometers thick, and surface scientists interest in defects and atomic structure had accustomed them to thinking in terms of angstroms and nanometers. Finally, Murday could see that many of the hot materials of the day (fullerenes, LB films, etc.) had critical dimensions on the nanometer scale, and were likely to intersect someday with the interests of microelectronics, probe microscopy, and surface science.

After 1990, the NANO Conference and the STM Conference alternated years, beginning a slow but perceptible trend away from probe microscopy and toward nanotechnology. In 1991, the STM Conference was held at Interlaken, Switzerland, and celebrated the 10th anniversary of the invention of the tunneling microscope.[8] Many early builders, especially surface science STMers, look back on this conference as the high water mark of the probe microscopy community. 1991 marked the beginning of the discrediting of air STM and the more formal parting of STMers and AFMers, along with the growing regulatory role of SPM manufacturers. 1991 was also a recession year that kicked off IBM's lean time, in which most of its corporate STMers either refocused their attention or left to take up academic jobs. Many surface science STMers stopped attending the STM Conferences, and returned full-time to meetings like APS and ACS.

1992 saw the second NANO Conference and, as a result of Murday's presidency of the AVS that year, the founding of a Nanometer-Scale Science and Technology Division within the society. Parallel to this institutionalization of the links between nanotechnology, probe microscopy, and the AVS, nanotechnology began taking on organizational complexity in other arenas as well. In 1991, for instance, the National Science Foundation opened a Nanoparticle Synthesis and

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[8] The official birthday of the STM dates from Binnig and Rohrer's first recording of a tunneling signature with their stationary apparatus in 1981.

Processing initiative, and in 1994 began sponsoring a National Nanofabrication User Network, modeled partly on the Materials Research Laboratories that the NSF had used to seed the discipline of materials science at university campuses in the ’50s. Some of these academic centers built on locally pre-existing, pre-nano institutions. For instance, at Cornell, the NSF had sponsored a National Research and Resource Facility for Submicron Structures (NRRFSS) since 1977; only in 1987, after Drexler and others had popularized the nano prefix, did the NRRFSS change its name to the National Nanofabrication Facility (later, the Cornell Nanofabrication Facility, and now the Cornell Nanoscale Science and Technology Facility), still with NSF support (Rathbun, et al. 2000).

Government support for nano-ization of research began to extend beyond the NSF in this period as well. The Department of Energy, for instance, began holding conferences on nanostructured materials as early as the late ’80s; and other research-sponsoring organizations began taking notice of nano in the wake of Drexler’s increasing prominence. According to the internal history of the National Nano Initiative, various grant officers (including Murday) began meeting casually in 1996, then formally starting in 1998 as the Interagency Working Group on Nanotechnology, a section of the National Science and Technology Council (an organization created by Bill Clinton in 1993 and in which Al Gore seems to have played a major role).[9] It was through the IWGN/NSTC that proposals for a National Nano Intiative began to take shape, with Murday as the Executive Secretary of the NSET subcommittee on Nanoscale Science, Engineering and Technology that drafted the structure and vision of the NNI. To see just how central probe microscopy has been to the framing of that vision, one need look no further than the subcommittee’s final report to Congress:

[9] This narrative is from http://nano.gov/html/about/history.html.

> In 1959 Richard Feynman delivered his now famous lecture, "There is Plenty of Room at the Bottom." He stimulated his audience with the vision of exciting new discoveries if one could fabricate materials and devices at the atomic/molecular scale. He pointed out that, for this to happen, a new class of miniaturized instrumentation would be needed to manipulate and measure the properties of these small – "nano" – structures. It was not until the 1980s that instruments were invented with the capabilities Feynman envisioned. These instruments, including scanning tunneling microscopes, atomic force microscopes, and near-field microscopes [i.e., NSOM], provide the "eyes" and "fingers" required for nanostructure measurement and manipulation. In parallel, the expansion of computational capability enable sophisticated simulations of material behavior at the nanoscale. These new tools and techniques have sparked excitement throughout the scientific community. (Anonymous 2000, 20)

That is, the subcommittee rhetorically positioned only two classes of tools – probe microscopes and computers – as having made the difference in bringing Feynman's vision to fruition.

Nano and Probe Microscopy Today

With the founding of the NNI, many probe microscopists have given up on hedging their relationship with nanotechnology and have speedily (if not always enthusiastically) converged on the new rubric. Where in 1999 it was possible to think of nano as something peripheral to probe microscopy, today it is impossible to avoid thinking about nano in describing the experience of probe microscopists. The funding, the prestige, and the community of nano have become an everyday reality for many STMers and AFMers, both in academic and industrial settings. Some, of course, feel queasy about the changes wrought by nano. As one surface scientist summarized:

> To me nano is just a buzzword used to generate money from the federal government. It has no meaning. Nano came on the scene when the government funded nano. In fact I just got back from a conference where one guy was talking about the difference between nanotechnology and nanoscience. Nanoscience is when you repackage what you were doing before so that you can get money for it now. Nanotechnology is projecting the consequences of something that you're already doing so that you can get money for that from the government too. So the notion was, you either just sort of rephrase what you were doing and get money or you say that you will do something slightly different and get money. [CD1, 10/30/03]

For many probe microscopists, though, nano brings cultural as well as financial inducements. Because of its unusual constitution, nanotechnology holds the promise for some SPMers of easing tensions within their community, of making their community relevant to a wider audience, and of allowing the continuation of traditions within their community that would otherwise have a much shorter shelf-life.

Most importantly, because of the variety of organizations supporting it, nano has a large and diverse enough ecology of participants that there is plenty of room for all the different kinds of probe microscopists – builders, runners, ordinary users, exceptional users, manufacturers. For instance, the NSF sponsors ordinary users to buy commercial AFMs in order to tie different research together in a nano-coordinated way. This was how I first saw the interaction of nano and probe microscopy at Cornell in 2000. Different groups that had been independently doing their own thing in their own traditions of making nanoscale objects (which, before, they had been treating as too small to image) suddenly found that there was a cultural and fiscal rationale for them to buy AFMs (sometimes to share among groups), image their "epistemic things," and build collaborations, with the AFM as a boundary object (Star and Griesemer 1989) that allowed them to overlap their various epistemic things (now with hot new labels like "nanohills" and "nanoropes") in a generative way – for instance, one group would make little hills, then another would make molecules stick on top of each hillock in an array, then another group would magnetize each of the molecules.

At the same time, the NSF and other sponsoring agencies continue to fund builders to innovate designs. There is a sense among many SPMers that closure has essentially been reached on what counts as a good probe microscope; or, at least, enough closure that future innovations are likely to be derivative on available commercial designs. There is an even stronger sense, though, that closure has *not* been reached on what counts as nanotechnology, nor on what instrumentation will be needed in the nano community. Probe microscopes are clearly an important set of tools for nano; but builders see the nano audience as larger and more differentiated, and thus able to accept innovations that depart farther from the commercial designs. Thus, people who used to build AFMs, for instance, often now spend much of their time building AFM-based nano instrumentation – things like multiprobe data storage systems, AFM "noses," or molecular force pullers [GB1, 9/26/01; CG1, 11/12/01; HG1, 11/14/01; PH1, 3/19/01].

One of the most interesting characteristics of nano in its current (and perhaps long-term) pre-closure state, therefore, is something much like the ethic of naïveté that we saw attending the invention and much of the development of STM and AFM. The public face of nanotechnology right now is marked by a playful, insouciant style that emphasizes making artful objects as much, if not more than, creating new knowledge. Nowhere is this clearer than in surface science. Once, surface scientists sought "cleaner, flatter, colder" surfaces because those were the ones that most approximated tractable forms of theory, and hence could be used to generate knowledge [JM2, 7/8/02; CD1, 10/30/03]. Now, "surface science is dead.... long live surface science in a different incarnation as nanoscience" [Himpsel, 5/9/01]. With the drift to nano, surface scientists have veered away from surfaces that are amenable to creating disciplined, positive *knowledge*, and moved toward surfaces that are amenable to making imaginative nanoscale *objects*:

> [Surface scientists] were interested in understanding the science base necessary to grow materials of interest to the electronics community.... You had to understand the surface in detail, and how you could grow a thin film on top of it while retaining a very fine, smooth surface. A tremendous amount of work had to go into the preparation of the surface, understanding how things settled down, where they went, what structures were there, and how you varied the process and conditions to get the desired result. One of the amusing things to me was that for many, many decades the people who were trying to grow superlattices worked very, very hard to get these perfectly smooth surfaces. So

> anytime they found processing conditions in which they got a non-flat surface, they would turn and run another direction. Appropriate attitude at the time. Now when we get into the nanoworld, what we've discovered is that some of those conditions they were trying desperately to avoid back then were giving "ordered nanostructures." It was killing them at the time, but now becomes of a high degree of interest. That prior experience is a real advantage because some of the things that were the poison back then now become the candy. You can resurrect those conditions and say "ooh, yeah!" We turned and ran the other direction back then, but let's go back and try "what happens if we push harder, can we now enhance that growth rate and give us these little pyramidal islands which are" – so that accelerates the progress when we get into 3D nanostructures. [JM2, 7/8/02]

One can speculate on the roots of this phenomenon in nano's need for user-friendly publicity, or in the lightning-quick production cycles of the new economy, into which many corporate and academic SPMers are integrated. Whatever the case, what politicians, reporters, and the general public often see of current nano research are nanoscale guitars, abacuses, trains, corrals, stick figures, and other playful nanoentities.[10] As we saw in Chapters Three and Five, for as long as a community is expanding rapidly without significant internal frictions, this style of work can be extraordinarily generative. In making nano-things, researchers can plausibly claim that they are both exploring the fundamental properties of small objects and demonstrating proofs of concept for technologically relevant techniques.

Thus, as in Chapter Six, researchers slide easily between repertoires of "nanoscience" and "nanotechnology." This, in turn, aids the further expansion of the nano community by helping newcomers cope with the phenomenon of experimental vertigo that plagued air STM. If the nano community places a high value *both* on research into fundamental properties ("nanoscience") and on playful building activities ("nanotechnology"), then newcomers have an easy way to avoid criticism – they can be seen to be contributing to the nano community, even if what they are discovering cannot always be represented as new, rigorous, disciplined, formal knowledge. One

[10] I would also argue that this kind of work has a long tradition in the microelectronics industry, where chip designers regularly work cartoons, signatures, and playful messages into the architectures of their chips. See http://micro.magnet.fsu.edu/creatures/ for some interesting examples.

hallmark of nanotechnology, therefore, has been the rapid explosion of interest in certain "hot" materials – things like nanotubes, where researchers can begin constructing elaborate, technologically-relevant structures using these objects, even while many chemists and physicists remain perplexed about their fundamental nature.

Thus, instrument manufacturers love nanotechnology because it allows them to continue finding imaginative solutions to the boxwallah's dilemma. On the one hand, nano provides vast new markets within subcultures that will innovate on *applications* but not on *designs*; on the other hand, nano provides breathing room for builder groups, thus keeping alive one of the manufacturers' most important sources of design modifications. Builder groups, therefore, have also flocked to nano. For those who have (or want) special relationships with manufacturers, the large and diverse nano community gives them leverage in getting their innovations commercialized. For those who have stayed away from the large manufacturers, nano has enough wrinkles that they can start their own small companies in niches where companies like DI and Omicron do not compete. For those builders who represent themselves as a craft elite, who embed considerable artisanry in their instrument designs and image renderings, nano provides a much larger community to be the elite *of*. Someone like Don Eigler, for instance, would receive a great deal of attention under any circumstance, but with nanotechnology he and his former postdocs can command enormous respect because of the high-end, nano-relevant science they do.

Exceptional users, too, find nano extraordinarily appealing. These people have tied their interests to those of the manufacturers, and as instrument makers have gravitated to nano so have their stable of exceptional users. Indeed, the exceptional users have probably done more work than anyone in figuring out how to turn the available commercial products into nanotechnology instruments. That is, they are not building new nano instrumentation themselves, but they are taking commercial AFMs and STMs and using them in canonically nano ways. This can be seen, for instance, among people like Chad Mirkin or Charlie Lieber, who have figured out how to put nanotubes on commercial AFM probes in order to do sophisticated kinds of manipulation, or people like Ari Requicha and Rich Superfine who have figured out how to make an AFM more like Feynman's original vision of a tiny "hand" that would respond at the nanoscale to the motions of a human, macroscale hand [AR1, 3/27/01].

Finally, the institutions most relevant to probe microscopy have also found solutions to their difficulties in nano. The AVS, for instance, has seen its focus shift so much in the '90s, that by 2003 it had dropped both the "American" and the "Vacuum" from its name and is now the "AVS Science and Technology Society" (where "AVS" no longer stands for anything). As surface science morphs into nanoscience, though, nano may yet provide the AVS a way to unify its different constituencies. Nano seems to have reinvigorated the National Bureau of Standards (now the National Institute of Standards and Technology) as well. In the '90s, when some of the remaining SPMers at the corporate labs started working on large, factory-line instruments for semiconductor metrology, NIST started its own parallel project, the Molecular Measuring Machine, under Russ Young's old protégé, Clayton Teague. The MMM developed closed-loop scanning that made possible ultrahigh resolution nanoscale measurement of surface features over macroscale lengths [JG3, 2/28/01; CT1, 6/28/02].[11] For nanotechnologists coming from the microelectronics industry, the MMM represents a gold standard, the kind of ultimate metrology at the heart of NIST's mandate. Thus, NIST generally has become deeply involved in nano, and Teague himself has moved up to become the director of the National Nanotechnology Coordination Office, which handles some administrative responsibilities for the

[11] A "closed loop" SPM is one in which there is independent verification of where the probe is, rather than just an assumption about its position based on the voltages put into the piezo scanner. This is often done using interferometric sensors that record very small displacements of the probe during scanning.

Nanoscale Science and Engineering Taskforce, is involved in preparing NSET's budgets, and does outreach to academia, industry, and the public.

Finally, the rise of nano has coincided with the renaissance of IBM Research. Again, the builder culture of nano is more in tune with IBM's current situation than the structure-obsessed surface science of the past. Some IBMers like Don Eigler or Dan Rugar or Phaedon Avouris now have the freedom to play around with nanoscale objects in ways that marry the repertoires of "science" and "technology." If, for example, Avouris pushes nanotubes around to make a working transistor, that makes headlines within the nanocommunity and can be spun as presaging future IBM products; but if, in the process, Avouris and company learn something more fundamental about nanotubes, then they write more traditional journal articles and accrue a more disciplined kind of prestige for IBM Research.

None of the old corporate labs, though, can play the role they once did. The regulated monopoly style of capitalism, exemplified by AT&T, Xerox, IBM, GE, and the other research giants, is seen as a dead letter, at least in industries where surface science and probe microscopy are relevant. The niche they filled in the ecology of research, though, is not a trivial loss. Whole fields like surface science and information theory were buoyed by these organizations, in the knowledge they created, the technologies they developed, and in the personnel they trained. Nano leaders like Murday explicitly see nanotechnology as allowing sponsoring agencies to fill some of the holes left by the decline of the great corporate labs.

> One of the problems, to which I think we in the US have to pay some attention, is that the industrial laboratories by and large are being scaled back.... They are not the dominant force they used to be globally across surface science or nano.... If [the big corporate labs] go away, we still have very good people doing basic research, they just tend to be more in the universities than in an industrial lab. Universities have different strengths, they generally have a harder time getting the good equipment.... That says the science we do now is going to be a bit different. Will this help or hurt? I think it'll hurt a bit. The really bright people won't have quite the ambience, they won't be surrounded

> by equally bright, well-equipped people.... Maybe at the federal level we need to think a little differently about how we fund basic research. In the [National Nanotechnology] Initiative you already begin to see some evidence of that awareness. The NNI is creating centers. That's in some sense what the IBM and Bell Labs did, they brought a bunch of very good people and put them in a central location at the same lab and equipped them well. To an extent that's what the centers are meant to do at the universities. [JM1, 7/8/02]

Nano right now is a community of communities, held together locally on university campuses by large, dedicated "Nano Centers" that provide a nexus for different kinds of researchers, a homegrown version of differentiated ecology that we have seen has been vital to the growth of communities like probe microscopy. Whether probe microscopy is now or ever will be as central to nano as nano's elites claim it to be, then, it is clear that many of the lessons of probe microscopy are visible once more in the development of nanotechnology. In spreading so widely and garnering so much attention, probe microscopy has made itself a key part of, and a template for, the dreams of nano-visionaries of all stripes; and nano, in offering wider vistas for a community burdened by existential constraints, is slowly making itself a promised land for probe microscopists

Conclusion

The development of probe microscopy helps us understand the constitution of nanotechnology because many nano elites see probe microscopy both as the central instrument in the history of the field, and as a technique that has a promising future role in the nano community. Probe microscopy can also show something more generally, though, about the art of invention and knowledge-creation in the late twentieth century. In particular, we can use probe microscopy as a lens to examine the extraordinarily complex ecologies needed to create modern scientific knowledge and high technology. In one sense, these ecologies support different kinds of *actors*. We saw how the corporate labs relied on the differentiation between postdocs, staff scientists, group leaders, technicians, and upper-level managers to construct a kind of research that would accord with corporate culture and, over the long-run, contribute to corporate needs. In the Quate and Hansma labs, an even more diverse set of people – builders, runners, students, postdocs, on- and off-campus collaborators, afternoon visitors, technicians, retirees, spouses, and so on – made these groups vital and dynamic centers of innovation. Later, these groups became associated with start-up companies that cultivated a very similar type of local ecology, which also were even more oriented to exporting the products of local work around the world.

These local ecologies are a familiar story to science and technology studies; I have particularly tried to examine them in terms of what Robert Kohler calls a "moral economy of experimentation" (Kohler 1994). In such a local moral economy, differentiation of types of participants produces overlapping sets of expectations and obligations that structure the assessment and circulation of new materials, techniques, and ideas; thus allowing for the transformation of grand visions into individual contributions, and making it possible for individual contributions to form a new and often unexpected basis for further grand visions. As Kohler points out, the roles and expectations associated with moral economies of experimentation can undergo significant disruptions as the products of the local order propagate more widely. We've seen how a much wider ecology formed with the commercialization of probe microscopy, where builders, runners, ordinary users, exceptional users, and manufacturers of various stripes competed and collaborated to bring forth new microscopes, new applications, and entrées into new communities. It is within this wider ecology that debates about the proper relationship between probe microscopy and nanotechnology are currently open.

One aim of this dissertation has been to point to an interesting feature of this wider ecology – namely, the creativity *and* disruption brought on by the ability and need of some probe microscopists to move from one social role to another within this ecology. By now it should be clear that the line separating "producers" from "consumers" of probe microscopes (and the knowledge generated with or about those instruments) is very thin. Throughout this story, we have seen people move from one position in this network to another – by, for example, starting as runners and becoming builders, or starting as builders and becoming buyers. This should provide a corrective for the standard approach in the social construction of technology literature. In particular, the development probe microscopy highlights the internal differentiation of the "relevant social groups" that are party to a technology – for instance, "builder groups" are clearly a relevant formation, but they also contain within them a variety of actors, some of whom, crucially, are also members of other, equally relevant, social groups. A Hansma postdoc like Jan Hoh, for example, might be a "runner" within that group, but a "pioneer" within the biophysical community; indeed, we can see how a technique flows outward exactly along the linkages provided by a set of individuals whose role is partly to *manufacture* the relevance of the technique for a social group.

Examining the types and frequency of role fluidity within these ecologies can tell us important things about how the community functions. For instance, we saw in Chapters Seven and Eight how commercialization spawns particular kinds of fluidity; if commercialization is successful in a subfield, it can cause "builders" to become "users," while if commercialization becomes too routinized it can provoke "customers" to start their own companies to cater to the research niche they formerly occupied. The management of this kind of role fluidity is a central point of the literature on trading zones; often, the ability to perform different roles (and assume the relevant identities) is traded among technical communities along with techniques, ideas, and materials. Yet commercializers of instruments face this challenge even more acutely than participants in traditional trading zones; this is one important aspect of what I have called the boxwallah's dilemma. Instrument manufacturers are continually on the lookout for elite microscopists whom they can coopt as exceptional users or conscript as applications scientists; while they are also looking for builders whom they can turn into buyers, and for research communities they can transform into "probe microscopists." Yet these companies also need to be aware of kinds of role fluidity that can work against them – as, for instance, when customers bolt to build their own microscopes or, even worse, found their own start-ups, or when the manufacturers' own employees become so dissatisfied with the routinization of their work that they, too, leave to start competing firms.

Also, as actor-network theory points out, role fluidity is not restricted to human participants. Indeed, the roles and identities present within an experimental culture are often co-constructed with the shapes of the tools and materials important to its members. We have certainly seen that probe microscopists grew and evolved along with the probe microscopes they were using. We have also seen how a complex ecology of materials and artifacts was as important to the creation of local cultures of probe microscopy as a complex ecology of human participants. Corporate surface science, for instance, sustained itself on the study of a regularly varying stable of epistemic materials and an ever-expanding stable of interlocking instrumentation and laboratory technologies. Similarly, the Quate and Hansma groups were ravenous for a rich assortment of epistemic materials, culled from a variety of sources, and directed their members to create an equally diverse set of instrumentation to deal with the cornucopia of samples. Later, the start-up manufacturers struggled to tame this surplus of materials by routinizing ways of bringing in new design innovations and tying them to new materials of interest.

In this way, the probe microscopy community grew rapidly; one part of the story of this growth – the importance of instrumentation in the constitution and maintenance of subdisciplines – is well-known to science and technology studies. The *triangular* trade between diverse arrays of human participants, laboratory technologies, and important *materials*, however, has not been thoroughly explored before. One contribution of this dissertation has been to point out a few *kinds* of materials that are central to the propagation of a technique – canonical materials such as the 7x7; test objects like, again, the 7x7 or, later, highly-oriented pyrolitic graphite; and hot materials, including DNA and high T_c superconductors – and to explore the interweaving of social role, technological innovation, and knowledge creation surrounding these materials.

Finally, the story of probe microscopy highlights the complex ecology of *activities* necessary for the creation of modern scientific knowledge. One recent picture of science and technology studies is that it is a kind of "epistemography" (Dear 2001), a discipline dedicated to understanding knowledge creation. I am sympathetic to this view, but wary that it leads us to think of knowledge creation as what science *is*. One aim of this dissertation has been to show that knowledge (and artifacts) fall out of a diverse array of activities. We have witnessed an extraordinary assortment of activities in this story, many of which could only dimly claim to have knowledge-creation as the immediate goal: Chapter Three saw knowledge creation as a sometimes incidental outcome of a playful and naïve approach to lab work; Chapter Four saw knowledge creation as an upshot of institutional means for training new employees; Chapter Five saw knowledge creation (sometimes knowledge of uncertain quality) as a byproduct of teaching graduate students and preparing postdocs to become leaders of their own lab groups; Chapter Seven saw knowledge creation as a commercializable product, where the goals of the commercializers themselves often had more to do with accruing cultural capital or enjoying the benefits of particular places to live.

Two *types* of activities have been an important part of the ecology I have described here – activities that participants can represent as *disciplined* or *undisciplined*. As we saw in Chapter Four, disciplined activity such as that found among postdocs at the corporate labs, could very quickly generate a vast amount of knowledge (or, at least, vast numbers of publications in reputable journals). This is one classic, positivist picture of science – as an institution centered on building up knowledge brick-by-brick through rigorous, focused activity, with a handful of materials and instruments as the factories and resources of knowledge. We also saw in Chapter Three how a self-consciously more naïve and less disciplined approach could lead to all kinds of exciting new knowledge. This, too, is an established, empiricist picture of how science works – the naïve researcher ventures into a field and allows experience and experiment to offer up the truth. I have hoped here not to affirm one or the other of these models, but rather to show the mechanisms, and necessity, of their interaction. The naïveté of the early STMers prompted the rapid diffusion of the instrument and éclat of its first successes; yet Binnig, Quate, Hansma, and others like them faced enormous problems of credibility when they acted without the aid of more disciplined collaborators, and it was the judgment and traditions of the communities they spoke to that dubbed their results as successes. Similarly, surface science had achieved much through professionalization and discipline formation; yet the field could easily have ossified had it stuck to LEED and surface reconstructions, rather than using the advent of the STM to transform itself and introduce the possibility of new kinds of knowledge.

Organizationally, we have seen how the institutions and disciplines linked to probe microscopy have surrounded themselves with projects and practices that are only peripherally related to knowledge creation (or where knowledge creation is itself the sideline of another activity). We might call this circus of activity around science the penumbra of knowledge-making; as we have seen, this penumbra has taken many institutionally- and historically-specific forms. Knowledge is a central goal and outcome of scientific work; yet so are pedagogy, entrepreneurship, the maintenance of communities of practice, the fostering of international alliances, the generation of culturally important symbols, and so forth. If we ignore this penumbra, we diminish our ability to analyze and understand science; if we interfere with or occlude the penumbra, we may make science work less well; and if we cast our eyes only on knowledge creation, we miss the humor and the richness of science as a human process.

We can, finally, use this deepened understanding of complex ecologies of people, instruments, materials, and activities to return to our central question: how do some laboratory technologies move from the local and eccentric to the global and routinized? What allows a lab technique to move from invention to replication to commercialization? What are the consequences of each of those transformations? One standard story – the "pipeline model" of scientific research – says that basic research at universities and a few corporate and national labs produces fundamental knowledge that then is transformed into applied knowledge and technologies that gain widespread use. Usually this is seen to happen because entrepreneurs and corporations find the most commercially feasible fruits of basic research and, through the work of engineers and applied scientists, turn it into products that the market then disposes of. This vision of science has, in the past two decades, become a near-creed of American science policy, as enacted through the Bayh-Dole Act of 1980, which gave universities greater leeway to patent research (and thus gave them an incentive to push research more quickly into the pipeline).[12] Though many in science and technology studies have offered criticisms of this view (Kline 2000a), it remains a

[12] See Shapin (2003) for a short narrative of the long history of university-industry relations and a critique of the adulation poured on the Bayh-Dole Act.

tremendously popular view among social scientists and laypeople alike in explaining the diffusion of high technologies.

Yet the story of probe microscopy shows that the pipeline, if there is one, has many kinks and backflows. According to the pipeline model, we would have expected the big corporate labs to have seized on the fruits of the basic research and quickly turned them into product streams. Similarly, we would have expected academic researchers to stand at one end of the pipeline, offering up research and designs that, through the work of entrepreneurs, could flow straight out to consumers. With probe microscopy, though, the cases of commercialization that have most resembled a pipeline have been the least successful – such as IBM's SXM large atomic force microscope for semiconductor metrology. Instead, we can use probe microscopy to advance an alternative to the pipeline – what we might call the penumbra model for the diffusion of science-based knowledge and technologies.

In the pipeline model, the pursuit of profit, and the influx of capital that accompanies that pursuit, pushes technologies through the pipe; scientists and universities seek profits from their patents on basic research, and entrepreneurs and corporations seek profits from the sale of products based on the application of that research. In the penumbra model, though, profit is as much an upshot of the activities surrounding research as the ultimate motivation for the research. That is, the probe microscopy case shows that commercialization of research is more likely, and more likely to be successful, when it is treated as an adaptation *from*, or an enabler *of*, rather than the reason *for* other cultural activities – i.e., when commercialization sits in the shadow (the penumbra) of other activities. Technologies are more likely to diffuse when their diffusion is a token within some cultural arena, rather than an arena unto itself.[13] Looking at probe microscopy with this lens, we should not be at all surprised that big corporations like IBM and AT&T did not commercialize the STM and AFM, even though their researchers were the pioneers of the techniques. Because of the culture of research in these places, commercialization of probe microscopy was not an answer to any of the local needs (or an extension of existing product lines) of corporate STMers and AFMers.

Instead, the story of probe microscopy shows us quite a wide range of activities from which diffusion can be an upshot. For instance, disciplinary affiliation can be a crucial token for diffusion. When members of a discipline can be brought to see their field's expertise as relevant to a technology, and the technology in turn relevant to pre-existing and ongoing debates framed by the epistemic culture (Knorr-Cetina 1999) of their field, then the technology may well diffuse into that community. Disciplines are not simply markets into which technologies can be pipelined; rather, the internal, differentiated activities of the discipline make it possible for external, personal linkages between the discipline and advocates of a technology to result in the active integration of the technology (where, as often as not, the *activity* of integrating is itself a prized cultural process of the discipline). At a more micro-level, the cultivation of particular kinds of scientific personae may facilitate diffusion, though rarely in a straightforward way; for instance, we saw that the cultivation of a maverick, untutored persona resulted in a significant kinks, missteps, and backflows in the pipeline of STM development. Yet naïvete was also a local cultural value of experimentation in which the peripatetic solicitation of expertise from colleagues eventually resulted in the rapid diffusion of the instrument.

[13] This is somewhat like Howard Becker's notion of the "side bet" in his study of commitment – actors are more committed to institutions when those institutions are tied into seemingly irrelevant aspects of their lives (Becker 1960). In the penumbra model, actors will be more interested in the diffusion of a technology when they can stake it in "side bets" on their other activities.

One of the most important activities from which diffusion and commercialization can be produced through side bets is pedagogy. In the pipeline model, pedagogy is a necessity only because new workers occasionally need to be brought in to replace old ones on the pipeline. Yet this, as we've seen, puts the cart before the horse. In the development of science-based technologies, commercialization is built *on top of* pedagogical activities; indeed, commercialization is often an outlet and adaptation from pedagogy, rather than the other way around. In the big corporate labs, there did not need to be a pipeline for technologies because the labs produced a pipeline of people, and helped to maintain institutions and disciplines that trained future workers. In the academic labs, the need to train successive generations of students in a resource-scarce but collaborator-rich environment was the trigger for the diffusion of the instrumentation. Later, when diffusion had become a routine *auxiliary to* (rather than a reason for) training, commercialization became a natural side bet off of pedagogical practice. Commercialization solved local cultural needs – it gave builders leverage in technical debates, it allowed them to find new collaborators (who could *teach* the builders' students), it gave students a place to go after graduation, and it became a way to reorder the communities in which builders found themselves participating.

Thus, even within manufacturers, where one might expect the pipeline model to be more eagerly seized upon, we find that commercialization was largely an outcome of, rather than an incentive for, experimental activities. Since the start-ups we saw here (and many start-ups in other industries) are outgrowths of local experimental cultures (rather than segments of a pipeline that adjoin those local cultures), start-ups themselves feed on a variety of activities and values in which profit making and technology diffusion sit in the shadows. Successful commercializers *live in* the trading zone, rather than just build pipelines in and through it for profit. This is why makers of science-based technologies must be continually wary of routinization; routinization can make diffusion easier, but it can also threaten prized cultural values, both inside and outside the company, that preempt the desire for profit. Diffusion and commercialization can be worthy goals in themselves; but they can never be isolated, or even overriding, goals. Instead, profit should be one patch in a much larger quilt; and commercialization and diffusion should be one kind of stitch (among many) that allows the patchwork of science and technology to be sewn together.

Appendix

List of Interviewees[1]

National Bureau of Standards/National Institute of Standards and Technology

BC1: Bob Celotta

JD1: John Dagata

RD1: Ronald Dixson

BG1: Bill Gadzuk

LH1: Lowell Howard

JK1: John Kopanski

JK2: John Kramar (*Caltech*)

RS1: Rick Silver (*University of Texas*)

CT1: Clayton Teague

TV1: Ted Vorburger

LW1: Lloyd Whitman (*Naval Research Lab*)

RY1: Russ Young (*Penn State*)

IBM Zurich

SA1: S.F. Alvarado

AB1: Alexis Baratoff (*Uni Basel*)

GB1: Gerd Binnig

UD1: Urs Dürig

CG1: Christoph Gerber

[1] Locations listed in bold are the institutions with which interviewees were associated while they were doing the work most relevant to this study. Locations listed in italics are other relevant institutions to which the interviewees have belonged. Interviews were conducted between June 2000 and February 2004, the majority taking place in 2001. Explicit permission has been given by interviewees for all direct quotes in this dissertation; in some cases, quotes have been slightly modified or anonymized at the interviewee's request.

JG1: Jim Gimzewski (*University of California – Los Angeles*)

HL1: H.-P. Lang (*Uni Basel*)

OM1: Othmar Marti (*University of California – Santa Barbara, Uni Ulm*)

BM1: Bruno Michel

DP1: Dieter Pohl (*Uni Basel*)

HR1: Heinrich Rohrer

IBM Yorktown

PA1: Phaedon Avouris

DB1: Dawn Bonnell (*University of Pennsylvania*)

JC1: Julian Chen

TC1: TC Chiang (*University of Illinois*)

JD2: Joe Demuth

RF1: Randy Feenstra (*Carnegie Mellon*)

BH1: Bob Hamers (*University of Wisconsin – Madison*)

FH1: Franz Himpsel (*University of Wisconsin – Madison*)

JK3: John Kirtley

NL1: Norton Lang

JS1: Joe Stroscio (*NIST*)

RT1: Ruud Tromp

JV1: John Villarrubia (*NIST*)

KW1: Kumar Wickramasinghe (*University College London, Stanford*)

IBM Almaden

SC1: Shirley Chiang (*University of California – Davis*)

JF1: John Foster (*Stanford, Innovative Microtech*)

JF2: Jane Frommer

BJ1: Barbara Jones

JM1: John Mamin (*University of California – Berkeley*)

MM1: Matthew Mate

GM1: Gary McClelland

DR1: Dan Rugar (*Stanford*)

BT1: Bruce Terris

PW1: Paul Weiss (*Bell Labs, Penn State*)

BW1: Bob Wilson

AY1: Ali Yazdani (*University of Illinois*)

Bell Labs

SB1: Steve Buratto (*University of California – Santa Barbara*)

DE1: Don Eigler (*IBM Almaden*)

JG2: Jene Golovchenko (*Harvard*)

JG3: Joe Griffith

DH1: Don Hamann

JK4: Joel Kubby (*Xerox*)

BS1: Brian Swartzentruber (*University of Wisconsin – Madison, Sandia National Lab*)

BW2: Bob Wolkow (*IBM Yorktown, National Research Council – Canada*)

Other corporate researchers

GC1: Gordon Chao (*Charles Evans Associates*)

DC1: Don Chernoff (*Standard Oil – Ohio, Advanced Surface Microscopy*)

CD1: Charles Duke (*Xerox*)

BJ2: Bob Jaklevic (*Ford*)

BK1: Bill Kaiser (*Ford, University of California – Los Angeles*)

MP1: Mike Pashley (*Philips, Cambridge*)

Other government researchers

NB1: Nancy Burnham (*Naval Research Lab, Worcester Polytechnic Institute*)

RC1: Rich Colton (*Naval Research Lab*)

BD1: Bob Dunn (*Battelle National Laboratory, University of Kansas*)

RG1: Reinhard Guckenberger (*Max Planck Institute – Martinsreid*)

GL1: Gil Lee (*Naval Research Lab, Purdue*)

JM2: Jim Murday (*Naval Research Lab, Office of Naval Research*)

FO1: Frank Ogletree (*Lawrence Berkeley National Lab*)

MS1: Miquel Salmeron (*Lawrence Berkeley National Lab*)

HW1: Hollis Wickman (*National Science Foundation*)

SX1: Sunney Xie (*Battelle National Laboratory, Harvard*)

Stanford

TA1: Tom Albrecht (*Park Scientific Instruments, IBM Almaden*)

TK1: Tom Kenny

GK1: Gordon Kino

SM1: Steve Minne (*Nanodevices*)

HM1: Howard Mizes (*Xerox*)

KM1: Kathryn Moler (*IBM Yorktown*)

JN1: Jun Nogami (*University of Wisconsin – Milwaukee, Michigan State*)

University of California – Santa Barbara

JC2: Jason Cleveland (*Digital Instruments, Asylum Research*)

BD2: Barney Drake (*Imaging Services*)

SG1: Scot Gould (*Claremont McKenna College*)

HH1: Helen Hansma

PH1: Paul Hansma

JH1: Jan Hoh (*Johns Hopkins*)

CP1: Craig Prater (*Digital Instruments*)

JZ1: Joe Zasadzinski

Other Hansma collaborators

CB1: Carlos Bustamante (*University of Oregon, University of New Mexico, University of California – Berkeley*)

HG1: Hermann Gaub (*Ludwig-Maximilians Universität*)

AG1: Andy Gewirth (*University of Texas, University of Illinois*)

SL1: Stuart Lindsay (*Arizona State University, Angstrom Technology, Molecular Imaging*)

Digital Instruments

DA1: Dennis Adderton (*Nanodevices*)

MA1: Mike Allen (*Lawrence Livermore National Laboratory, University of California – Davis, Biometrology*)

KB1: Ken Babcock

DB2: Dan Bocek (*Asylum Research*)

VE1: Virgil Elings (*University of California – Santa Barbara*)

MH1: Monte Heaton

KK1: Kevin Kjoller

SM2: Sergei Magonov

PM1: Pete Maivald (*University of California – Santa Barbara*)

JM3: James Massie (*University of California – Santa Barbara*)

TM1: Terry Mehr (*Asylum Research*)

ER1: Eric Rufe (*Stanford*)

MT1: Matt Thompson

JW1: Jerome Wiedmann (*University of California – Santa Barbara, Zinc Power Matrix*)

Park Scientific Instruments

JA1: John Alexander (*Angstrom Technology, KLA-Tencor*)

DB3: David Braunstein (*Stanford, IBM San Jose*)

FG1: Franz Giessibl (*IBM Munich, Uni Augsburg*)

MK1: Mike Kirk (*Stanford, KLA-Tencor*)

BP1: Becky Pinto (*KLA-Tencor*)

MT2: Marco Tortonese (*Stanford, VLSI Standards*)

BT2: Brian Trafas (*KLA-Tencor*)

Other instrument manufacturers

GA1: Gary Aden (*Topometrix/Thermomicroscopes*)

AB2: Andreas Berghaus (*Surface/Interface*)

TB1: Thomas Berghaus (*Omicron, Uni Bochum*)

CB2: Chuck Bryson (*Surface/Interface*)

RE1: Ray Eby (*Topometrix/Thermomicroscopes, Royal Ontario Museum*)

DF1: Dave Farrell (*Burleigh Instruments*)

JG4: John Green (*RHK Tech*)

EH1: Eric Henderson (*BioForce Labs, Iowa State*)

BJ3: Bob Jaymes (*Surface/Interface*)

TJ1: Tianwei Jing (*Molecular Imaging, Arizona State University*)

SK1: Stefan Kaemmer (*Thermomicroscopes*)

VK1: Vic Kley (*General Nanotech*)

GM2: George McMurtry (*Quesant*)

KM2: Katerina Moloni (*Piezomax*)

CM1: Curtis Mosher (*BioForce Labs*)

VN1: Vance Nau (*Molecular Imaging*)

RS2: Robert Sum (*Nanosurf, Uni Basel*)

KW2: Kelvin Walsh (*Surface/Interface*)

OW1: Oden Warren (*Hysitron*)

KW3: Klaus Weishaupt (*WITec, Uni Ulm*)

PW2: Paul West (*Quanscan/Topometrix/Thermomicroscopes, Caltech*)

DY1: Daphna Yaniv (*Molecular Imaging*)

Other academic researchers

JB1: John Baldeschwieler (*Caltech*)

PB1: Paul Bryant (*University of Missouri – Kansas City*)

DC2: Dongming Chen (*Harvard, Rowland Institute*)

MC1: Mike Crommie (*IBM Almaden, Boston University, University of California – Berkeley*)

PC1: Paul Cutler (*Penn State*)

DF2: Dan Frisbie (*University of Minnesota*)

WG1: Wayne Gladfelter (*University of Minnesota*)

CG2: Cynthia Goh (*University of Toronto*)

NG1: Nick Guilbert (*Peddie School*)

HG2: H.-J. Güntherodt (*Uni Basel*)

WH1: Wolfgang Heckl (*IBM Munich, Ludwig-Maximilians Universität*)

EH2: Eric Heller (*Harvard*)

GH1: Grant Henderson (*University of Toronto*)

MH2: Mark Hersam (*University of Illinois, Northwestern University*)

HH2: H.-J. Hug (*Uni Basel*)

KK2: Khaled Karrai (*Ludwig-Maximilians Universität*)

DK1: Dieter Kolb (*Uni Ulm*)

ML1: Max Lagally (*University of Wisconsin –Madison, Piezomax*)

FL1: Fred Leibsle (*University of Illinois, University of Missouri – Kansas City*)

NL2: Nathan Lewis (*Caltech*)

JL1: Joe Lyding (*University of Illinois*)

AM1: Arun Majumdar (*University of California – Berkeley*)

EM1: Ernst Meyer (*Uni Basel*)

LN1: Lukas Novotny (*Rochester Institute of Technology*)

MP2: Marc Porter (*Iowa State*)

AR1: Ari Requicha (*University of Southern California*)

PR1: Phil Russell (*North Carolina State University*)

DS1: Dror Sarid (*University of Arizona*)

SS1: Steve Sass and group (*Cornell University*)

WS1: Walter Smith (*University of Texas, Haverford College*)

ST1: Stuart Tessmer (*University of Illinois, Michigan State*)

IT1: Ig Tsong (*Arizona State University*)

MW1: Mike Ward (*University of Minnesota*)

UW1: Uli Wiesner and group (*Cornell University*)

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